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What 21st-Century Works Will Merit a Close Reading in 2050?: First Tranche of Responses
Comment on Sen, Karaca-Mandic, and Georgiou on Stay-at-Home Orders and COVID-19 Hospitalizations in Four States

John A. Spry

The JAMA research letter “Association of Stay-at-Home Orders with COVID-19 Hospitalizations in 4 States,” by Soumya Sen, Pinar Karaca-Mandic, and Archelle Georgiou (2020; hereafter the authors are referred to as SKG), should not be used to make any claims about the effects of stay-at-home orders because its statistical method is flawed. The letter shows that, after the effective date of stay-at-home orders in Colorado, Minnesota, Ohio, and Virginia, actual hospitalizations were well below the putative forecast from a univariate exponential time trend.

The effects of stay-at-home orders on COVID-19 hospitalizations cannot be determined by only comparing actual hospitalizations to forecasts of hospitalizations from an exponential trend model within a state with a stay-at-home order. That method suffers from the lack of any comparison to control data. Unfortunately, the SKG research letter did not examine whether actual COVID-19 hospitalizations were also lower than a similar forecast of exponential growth in states without stay-at-home orders.

This paper shows that the SKG method fails the following placebo tests. In three states without stay-at-home orders, cumulative COVID-19 hospitalizations were also lower than a similar forecast of exponential growth in states without stay-at-home orders.

1. University of St. Thomas, St. Paul, MN 55105. I am grateful to Mufaddal Baxamusa and Paul Novosad for their contributions to the working paper that served as the core of this comment. All errors are my own.
cumulative hospitalizations are substantially lower than the forecast from a univariate exponential model in Iowa and North Dakota. In South Dakota, COVID-19 hospitalizations fall below the projections from a univariate exponential model in May. Actual COVID-19 cases are significantly below a projection of exponential growth in Arkansas, another state without a stay-at-home order. A reduction in actual hospitalizations below projections of exponential growth in states without statewide or local stay-at-home orders should not be attributed to non-existent stay-at-home orders.

**SKG’s methods and results**

SKG estimate separate univariate linear and exponential time trend models using statewide cumulative COVID-19 hospitalizations in four states, Colorado, Minnesota, Ohio, and Virginia, with stay-at-home orders. They favor the exponential model because its goodness of fit is better than that of the linear model.

The exponential time trend model is \( y = ae^{bt} \), where \( y \) is cumulative COVID-19 hospitalizations and \( t \) is a time trend. SKG estimate this model using data from the beginning of a state’s available data until twelve days after the state’s stay-at-home order to allow for reported medians of five days from infection until symptoms and seven days from symptoms until hospitalization.

SKG interpret the deviation of actual hospitalizations from the projected exponential growth in cumulative hospitalizations as evidence of an association between stay-at-home orders and reduced hospitalizations. They include this remark: “Other factors that potentially decreased the rate of virus spread and subsequent hospitalizations include school closings, social distance guidelines, and general pandemic awareness” (SKG 2020, 2522–2523). They also note that other factors were not modeled, and “data on only 4 states were available” (ibid., 2523).

**Problems with SKG’s analysis**

In general, without the use of any control group it is not possible to identify the effects of a single policy intervention during the COVID-19 pandemic by fitting a univariate exponential time trend to cumulative COVID-19 events in a state from the beginning of the pandemic and viewing the difference between the exponential forecast and subsequent observed data as the treatment effect of the policy intervention.

The exponential functional form ignores Farr’s Law. Farr (1840) observed that the flow—infections, hospitalizations, or deaths—of an infectious disease
could be approximated by a normal bell curve (Bregman and Langmuir 1990). Cumulative counts of disease events, like hospitalizations, may be approximated by a sigmoid (s-shaped) function, such as the logistic function. In contrast, the exponential functional form assumes constant exponential growth. Actual cumulative disease events will tend to fall below exponential growth forecasted from initial data, without any intervention, when cumulative disease events follow Farr’s Law.

SKG do not explain why they favor fitting an exponential function over a sigmoid function. The univariate exponential functional form would only be appropriate when there is a credible argument that the initial exponential trend would continue in the absence of the policy intervention.

Data and methods

In this paper I implement the SKG method by estimating univariate exponential models for four states where either COVID-19 hospitalizations or cases are a function of time during the curve fitting period shown in Table 1.

There were five states without stay-at-home orders at either the state or local level: Arkansas, Iowa, Nebraska, North Dakota, and South Dakota (Mervosh, Lu, and Swales 2020). Nebraska is excluded because cumulative hospitalization data is not available until June 1. Each of these four remaining states did have other official state actions, including school closings, intended to reduce interactions that could spread the virus. Arkansas, Iowa, North Dakota, and South Dakota are not a random selection of states. They are more agricultural, rural, and centrally located than a typical state.

Data on cumulative hospitalization was downloaded from the COVID-19 Hospitalization Tracking Project website (link) for Iowa, North Dakota, and South Dakota. SKG have performed a valuable service by disseminating data on cumulative COVID-19 hospitalizations via that website. Iowa, North Dakota, and South Dakota have data on cumulative hospitalizations beginning on April 1, March 21, and March 30, respectively. Such data begins on March 10 for Colorado, March 17 for Ohio, and March 19 for both Minnesota and Virginia.

Cumulative COVID-19 case data for Arkansas was downloaded from the Arkansas Department of Health (link) because Arkansas did not have cumulative hospitalization data available until April 28. Arkansas data on cumulative COVID-19 cases begins on March 11.

I use a counterfactual stay-at-home placebo date of March 30, the same day Virginia issued its stay-at-home order, to determine the curve fitting period. The counterfactual stay-at-home placebo date of March 30 is my best guess of when a state would have enacted a stay-at-home order. It is two days after Minnesota’s
stay-at-home order and the same day as Virginia’s order.

Following SKG, I make the median effective date for hospitalizations in Iowa, North Dakota, and South Dakota twelve days later, to allow for a median of five days from infection until symptoms and a median of seven days from symptoms until hospitalization. The median effective date for COVID-19 cases in Arkansas is five days later to allow for five days from infection until symptoms.

Data on Iowa hospitalizations starts on April 1, resulting in an April 1–April 10 fitting period. Data on North Dakota hospitalizations starts on March 21, resulting in a March 21–April 10 fitting period. Data on South Dakota hospitalizations starts on March 31, resulting in a March 31–April-10 fitting period. Data on Arkansas cumulative COVID-19 cases starts on March 11, resulting in a March 11–April 3 fitting period.

### Results

Actual COVID-19 cumulative hospitalizations are substantially lower than the forecasted exponential growth rates in Iowa and North Dakota. The difference becomes progressively larger with time. The 95% confidence intervals (c.i.) for forecasted cumulative hospitalizations shown in Figure 1 are relatively large.

**TABLE 1. Cumulative hospitalizations or infections due to COVID-19 in Iowa, North Dakota, South Dakota, and Arkansas**

<table>
<thead>
<tr>
<th>State</th>
<th>COVID-19 data</th>
<th>Curve fitting period</th>
<th>Counterfactual stay-at-home placebo date</th>
<th>Median effective date</th>
<th>On first day of reporting</th>
<th>On April 28</th>
<th>ln(α) (95% CI)</th>
<th>b (95% CI)</th>
<th>R²</th>
</tr>
</thead>
<tbody>
<tr>
<td>Iowa</td>
<td>Hospitalizations</td>
<td>April 1–April 10</td>
<td>March 30</td>
<td>April 10</td>
<td>47</td>
<td>618</td>
<td>34.85 (29.40–41.32)</td>
<td>0.12 (0.09–0.15)</td>
<td>0.93</td>
</tr>
<tr>
<td>North Dakota</td>
<td>Hospitalizations</td>
<td>March 21–April 10</td>
<td>March 30</td>
<td>April 10</td>
<td>3</td>
<td>79</td>
<td>4.54 (3.40–6.08)</td>
<td>0.12 (0.10–0.14)</td>
<td>0.86</td>
</tr>
<tr>
<td>South Dakota</td>
<td>Hospitalizations</td>
<td>March 31–April 10</td>
<td>March 30</td>
<td>April 10</td>
<td>12</td>
<td>157</td>
<td>11.48 (10.06–13.10)</td>
<td>0.09 (0.07–0.11)</td>
<td>0.93</td>
</tr>
<tr>
<td>Arkansas</td>
<td>Cases</td>
<td>March 11–April 3</td>
<td>March 30</td>
<td>April 3</td>
<td>5</td>
<td>3146</td>
<td>8.19 (5.17–11.73)</td>
<td>0.22 (0.19–0.24)</td>
<td>0.94</td>
</tr>
</tbody>
</table>

In Iowa actual hospitalizations start falling below projected hospitalizations around April 20, ten days after the median effective date for the placebo stay-at-home order. By May 26 actual cumulative Iowa hospitalizations were 1,504, while forecasted cumulative hospitalizations were 29,000 (c.i. 5,300–162,000).

In North Dakota actual hospitalizations started falling under the forecasted cumulative hospitalizations around April 8, two days before the median effective
placebo date. The difference between actual and forecasted hospitalizations is not statistically significant on this date. On April 15 projected hospitalizations were 98 (c.i. 40–240), and actual hospitalizations were 44. This was five days after the median effective placebo stay-at-home date. By May 26 projected hospitalizations were 12,600 (c.i. 1,991–79,000) and actual hospitalizations were 156.

In South Dakota actual cumulative hospitalizations stayed around forecasted cumulative hospitalizations for most of April. South Dakota hospitalizations began falling below forecast around April 28. By May 26 projected hospitalizations were 2,200 (c.i. 586–8,600), and actual South Dakota hospitalizations were 378.

The exponential parameter $b$, in $y = ae^{bt}$, is estimated to be 0.012 in both Iowa and North Dakota and 0.09 in South Dakota. This is almost half the magnitude estimated in SKG. They find that $b$ ranges from 0.178 in Virginia to 0.24 in Colorado. These differences show a slower rate of growth in cumulative hospitalizations in these three states than in the four states studied in SKG. This difference could be due to inherent differences among the states or the different dates of the fitting periods, especially the later start of the Iowa and South Dakota fitting periods.

In Arkansas, actual cumulative cases started falling below projected cases around March 31, three days before the median effective placebo date. However, the March 31 difference between 612 actual cases and 783 projected cases was not statistically significant at a 95% confidence level. Actual cumulative cases were 1,146 on April 8, five days after the median effective placebo stay-at-home date, while projected cases were 4,400. By April 28 in Arkansas 342,000 cases were forecasted while there were only 3,146 actual cases.

**Figure 1.** Projected vs. observed COVID-19 events before and after placebo stay-at-home dates
Figure 1 (cont’d). Projected vs. observed COVID-19 events before and after placebo stay-at-home dates.
**Discussion**

If the SKG method was sound, states without stay-at-home orders should have experienced exponential growth in COVID-19 hospitalizations and cases—but they experienced COVID-19 events below exponential forecasts. Actual COVID-19 cumulative hospitalizations fall below the projections from a univariate exponential time trend model in Iowa, North Dakota, and South Dakota. Actual COVID-19 cases were significantly below an exponential time trend forecast in Arkansas. The timing of when these deviations occur does vary by state.

The best available conclusion from observing that COVID-19 hospitalizations fall below the forecast from a univariate exponential time trend in states with and without stay-at-home orders is that nothing should be concluded about the effect of stay-at-home orders on COVID-19 hospitalizations from the evidence in
This does not mean that stay-at-home orders do not have an effect on COVID-19 hospitalizations. Whether or not stay-at-home orders play a role in reducing hospitalization is impossible to determine using this method. And the findings certainly do not mean that not enacting a stay-at-home order is associated with reduced COVID-19 hospitalizations in Iowa, North Dakota, and South Dakota or reduced COVID-19 cases in Arkansas.

Finally, a brief discussion of the history of related work is needed to credit the contributions of other people. Paul Novosad first publicized concerns with SKG’s method in a Twitter thread on May 27, 2020 (link), when the SKG research letter was first published. I then collaborated with Mufaddal Baxamusa and Paul Novosad on an unsuccessful submission of a research letter to *JAMA* that served as the core of this comment. The *JAMA* editor suggested that we could submit a comment on the SKG research letter. Our submission of an online comment on the SKG research letter was also rejected. We then made that comment available online (Baxamusa, Novosad, and Spry 2020). Separately, Joe Gibson (2020) and Arnab K. Ghosh and Nathaniel Hupert (2020) also raised methodological concerns with SKG. Ghosh and Hupert especially criticized SKG’s use of exponential functions.

### References


About the Author

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Reply to John Spry on Stay-at-Home Orders and COVID-19 Hospitalizations

Soumya Sen¹, Pinar Karaca-Mandic², and Archelle Georgiou³

LINK TO ABSTRACT

The comment by John Spry (2020) is based on erroneous interpretations of the results of our paper (Sen, Karaca-Mandic, and Georgiou 2020), abbreviated as SKG. First, Spry reports that in three states without stay-at-home orders, cumulative COVID-19 hospitalizations do not continue to grow at the initial estimated exponential rate and uses this as the argument against any association between stay-at-home policies and hospitalizations. However, this is not the result that SKG reports. SKG shows that in all four states that enacted stay-at-home orders, the cumulative hospitalizations deviated from the initial exponential trend line (and its 95% prediction interval) at approximately 12 days—the median incubation period of COVID-19—after the order’s effective date in each state. In an unpublished analysis (not included in the JAMA article due to the space constraints), SKG show that in South Dakota, a state without a stay-at-home order, COVID-19 hospitalizations rates continued to follow an exponential growth function closely throughout the study period without any deviation like those observed for the four states reported in the SKG study. The plots in the Spry paper, specifically that of South Dakota as a control, also show this. We have included our plot below (Figure 1).

Second, the SKG paper does not claim that hospitalizations in a pandemic continue to grow exponentially for an infinite duration. Rather, exponential or sub-exponential rates are most often observed in the initial stages of a pandemic which

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reflects the time period of the SKG analysis. The early phase of an S-shaped growth curve shows the sharp rise and fits the early data (e.g., March) that was used in the SKG study. In contrast, the data points used in Spry’s work are primarily from April and May.

Third, as reported in SKG, the exponential function fitted the actual hospitalization numbers better than linear growth models. The main association highlighted in the SKG study is that in all four states, deviation from the initial exponential growth consistently occurred after approximately 12 days of the issuance of stay-at-home orders and remained below the initial rates thereafter. The publication also highlighted the limitation of assigning causal interpretation to this observation.

Figure 1. Projected vs. observed COVID-19 hospitalizations in South Dakota, hypothetical stay-at-home order on April 7, 2020

References


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Discuss this article at Journaltalk: https://journaltalk.net/articles/6011
The Moving to Opportunity Experiment: What Do Heterogeneous Estimates of the Effect of Moving Imply About Causes?

Robert Kaestner

LINK TO ABSTRACT

The Moving to Opportunity (MTO) experiment was a federally funded program conducted between 1994 to 1998. It provided housing vouchers to low-income families to use to move out of public housing—either housing projects or project-based Section 8 housing—located in high-poverty areas (Sanbonmatsu et al. 2011). The experiment was conducted between 1994 and 1998 in five cities: Baltimore, Boston, Chicago, Los Angeles and New York.

Families in the MTO were randomized into three groups:

- those who received no voucher, or subsidy, to move (referred to as the ‘control’ group);
- an experimental arm that received a standard, government housing voucher to move (referred to as ‘Section 8 voucher’ arm);
- and an experimental arm that received a housing voucher that could be used only for housing in census tracts with poverty rates below 10 percent (referred to as ‘experimental voucher’ arm).

In an important article, Raj Chetty, Nathaniel Hendren, and Lawrence Katz (2016) report findings from a study of the long-term impact from moving out

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of public housing during childhood. Chetty et al. (2016)—henceforth CHK—examined the adult earnings of children who participated in MTO. Adult earnings information came from federal income tax records (e.g., W2).

In most analyses of earnings, CHK included children who were born between 1976 and 1988, and look at earnings in the years between 2008 and 2012. CHK conducted analyses on children stratified into two groups by age when the randomization was done between 1994 and 1998: (1) those below 13 years old, and (2) those 13 to 18 years old. Results reported by CHK are as follows:

- The estimate of the effect of moving (i.e., treatment-on-the-treated, or TOT) for younger children (<13) in the experimental voucher arm was $3,477, which is an increase of 31 percent relative to the control group mean of $11,270.
- The estimate of the effect of moving (TOT) for younger children in the Section 8 voucher arm was $1,723, which is an increase of 15 percent relative to the control group mean of $11,270.
- The estimate of the effect of moving (TOT) for older children (13 to 18) in the experimental voucher arm was negative $2,427, which is a decrease of 15 percent relative to the control group mean of $15,882.
- The estimate of the effect of moving (TOT) for older children in the Section 8 voucher arm was negative $2,051, which is a decrease of 13 percent relative to the control group mean of $15,882.

Chetty et al. (2016) highlight that escaping poverty is the key to success

CHK recognized that neighborhood poverty is correlated with many attributes of the neighborhood, saying: “The treatment effects we report in this paper should thus be interpreted as the effect of changing a bundle of neighborhood attributes rather than any one feature of neighborhood environments” (p. 869). They elaborate some of their main results as follows: “The key implication of Table 2 for our analysis of exposure effects is that the younger MTO children received a much larger dosage of exposure to improved neighborhood environments than the older MTO children. The TOT effects on post-RA

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2. To be more precise: CHK look at the earnings of such an individual only for the years 2008 to 2012 during which that individual was 24 years or older. That is, they do not include not-yet-24 include income data.
[random-assignment] neighborhood poverty rates are similar for the younger and older MTO children. That is, families who took up vouchers moved to similar neighborhoods irrespective of their children’s age. However, the younger children got the improvements in neighborhoods starting at younger ages. On average the younger group got 9.8 years of childhood exposure to better neighborhoods up to age 18, because they were 8.2 years old on average at RA. In contrast, those in the older group received only 2.9 years of childhood exposure to better neighborhoods on average, because they were 15.1 years old on average at RA” (870).

Despite recognizing that neighborhoods are bundles of attributes each of which may affect children’s adult well-being, the authors pervasively use language suggesting that their results show that moving to a low-poverty neighborhood increases adult earnings of children living in high-poverty areas. Here are some quotations:

- “We find that moving to a lower-poverty neighborhood when young (before age 13) increases college attendance and earnings and reduces single parenthood rates” (CHK, 855).
- “First, we hypothesize that moving to a lower-poverty area improves long-term economic outcomes for children who were young at the point of random assignment (RA)” (856).
- “On average from the date of RA until age 18, children below age 13 at RA in the control group lived in census tracts with a mean poverty rate of 41 percent. Children whose families took up the experimental voucher lived in census tracts with 22 percentage point lower poverty rates than those in the control group on average until age 18. Those who took up the Section 8 voucher lived in census tracts with 12 percentage point lower poverty rates than the control group” (857).
- “The fact that the experimental voucher had larger effects on children’s outcomes than the Section 8 voucher therefore suggests that actively encouraging families to move to lower-poverty neighborhoods—either through counseling or by restricting their choice set—increases the impacts of housing vouchers on young children’s long-term economic success” (857–858).
- “[T]he Section 8 voucher increased individual earnings of young children about half as much as the experimental voucher, consistent with the fact that it reduced neighborhood poverty rates half as much” (876).
- “Nonetheless, regardless of the underlying mechanisms, the experimental results are adequate to conclude that providing subsidized housing vouchers to move to lower-poverty areas produces larger benefits for younger children” (858).
• “We conclude that the Moving to Opportunity experiment generated substantial gains for children who moved to lower-poverty neighborhoods when they were young” (859).

• “These results further support the view that moving to lower-poverty areas improves outcomes when one moves as a young child but not at older ages” (879).

• “We estimate that moving a child out of public housing to a low-poverty area when young (at age eight on average) using an MTO-type experimental voucher will increase the child’s total lifetime earnings by about $302,000” (859–860).

• “In short, subsidized housing vouchers produce durable benefits that persist into subsequent generations for children who moved to lower-poverty neighborhoods at young ages” (882).

Chetty et al. (2016) highlight that disruption is the explanation of poor outcomes of older children

For older children, CHK speculated that the disruption of a child’s life is harmful to older children and more important than possible benefits of a lower-poverty neighborhood, particularly because older children were less exposed to lower-poverty neighborhood life.

• “Moving as an adolescent has slightly negative impacts, perhaps because of disruption effects” (CHK, 855).

• “The point estimates suggest that, if anything, moving to a lower-poverty neighborhood had slightly negative effects on older children’s outcomes” (858).

• “One potential explanation for these negative impacts at older ages is a disruption effect: moving to a very different environment, especially as an adolescent, could disrupt social networks and have other adverse effects on child development (Coleman 1988; Wood et al. 1993; South, Haynie, and Bose 2007)” (858).

• “These three facts are consistent with a simple model that combines positive exposure effects from moving to lower-poverty neighborhoods with a negative disruption cost of moving to such a neighborhood” (876).
The heterogeneous effects of MTO raise doubts about the Chetty et al. (2016) conclusions

At first glance, the conclusions reached by CHK seem plausible. The younger children in the experimental arms moved more and to neighborhoods with lower poverty, compared to younger children in the control group, and those experimental children saw increased earnings after the age of 24. Also, the increase in earnings associated with moving for younger children in the experimental voucher arm was greater than it was for children in the Section 8 voucher arm, and the former group moved to lower poverty neighborhoods than the latter group. Similarly, moving was associated with lower earnings for older children in the two experimental arms and there was little difference in the magnitude of estimates across experimental arms.

The estimates reported in CHK are average effects of moving on earnings for children across all five experiments (i.e., sites). Estimates for children from individual sites, found in the Online Appendix (link), were much more heterogeneous (see CHK, Appendix Table 7B). For example, the intention-to-treat (ITT) estimate of the effect of being randomized into the experimental voucher arm for young children in Baltimore was $415; in Boston, the analogous estimate was $2,619. In the Appendix, heterogeneous estimates were reported for other sites and age groups too. In the article itself, CHK provide only a brief assessment of the heterogeneity and conclude that the heterogeneous results are consistent with the main conclusions.

- “We find no systematic differences in the treatment effects of MTO on children’s long-term outcomes by gender, race, or site” (CHK, 859, my emphasis).
- “In summary, the main lesson of the heterogeneity analysis is that the long-term benefits of childhood exposure to lower-poverty neighborhoods are highly robust across genders, racial groups, and geographic locations” (884, my emphasis).

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3. Whether the estimate of the effect of moving on earnings among younger children differed significantly across experimental arms is unclear and unreported by CHK. There is considerable overlap between the confidence intervals of the two estimates.
In the article proper, there is no examination of site-specific results. For example, the word “Baltimore” appears only twice, and only in cursory listing along with the other four sites, thus “Baltimore, Boston, Chicago, Los Angeles, and New York” (CHK, 860, 884).

In this article, I explore the heterogeneity across sites to assess whether the pattern of estimates across and within sites is consistent with the explanations proposed by CHK. Those conclusions were based on the average estimates across all five sites. I conclude that the more detailed evidence does not support the two principal explanations offered by CHK: that neighborhood poverty is the key part of the explanation of the results for younger children, and that disruption is a likely explanation of the findings for older children. Moving increased adult earnings of young children on average, but there is little relationship between the poverty of the destination neighborhood and the earnings increase across or within experimental site. Similarly, moving was associated with similar decreases in adult earnings among older children across both experimental arms on average, but this is not the case in four out of five sites!

The low level of statistical power of the MTO experiment (e.g., lack of statistically significant estimates) and the incongruence between the explanations of the change in earnings associated with moving based on average estimates and the heterogeneous estimates from the different experimental sites suggest that we still do not know what caused the observed changes in earnings reported by CHK. Therefore, designing current (e.g., Seattle) and future experiments based on the CHK conclusions may be misguided. The uncertainty over the causes of the MTO results suggest that current and future experiments should be undertaken with a more exploratory approach. To me it seems much more prudent and sensible to design experiments that explore mechanisms besides neighborhood poverty, for example, moving to a neighborhood with better schools (e.g., Laliberté 2020), less crime, closer to jobs, or with greater walkability and infrastructure regardless of the poverty level.

Calculating TOT estimates by experimental site

In the Online Appendix, CHK reported ITT estimates of the effect of being randomized into the two experimental arms on earnings by child age (0–12, 13–18) and city/site. However, they do not report analogous TOT estimates. Therefore, I calculated them using the fact that the TOT estimate is the ITT estimate reported by CHK divided by the housing voucher (moving) take-up rate. The voucher take-
The voucher take-up rates are ITT estimates of the difference in mean voucher use (moving) between families in the two treatment arms and families in the control arm. The sample consists of the 3,273 families present at the long-term follow-up. I assume that the voucher take-up rate is the same for families of younger and older children. ITT estimates of the take-up rate by site and experimental group are shown in Table 1.

<table>
<thead>
<tr>
<th>City/Site</th>
<th>Experimental voucher</th>
<th>Section 8 voucher</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baltimore</td>
<td>0.551</td>
<td>0.769</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.047)</td>
</tr>
<tr>
<td>Boston</td>
<td>0.427</td>
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<td></td>
<td>(0.038)</td>
<td>(0.040)</td>
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<td>Chicago</td>
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<td>(0.036)</td>
<td>(0.039)</td>
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<tr>
<td>Los Angeles</td>
<td>0.630</td>
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<td></td>
<td>(0.034)</td>
<td>(0.038)</td>
</tr>
<tr>
<td>New York</td>
<td>0.483</td>
<td>0.446</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
<td>(0.039)</td>
</tr>
</tbody>
</table>

*Note:* Author’s calculations using data from Ludwig et al. (2012). Standard errors in parentheses.

As indicated in Table 1, voucher take-up rates were relatively higher among families in the Section 8 voucher arm than in the experimental voucher arm (except in New York) and varied considerably across sites. Among families in the experimental voucher arm, the voucher take-up rate ranged from 0.33 (Chicago) to 0.63 (Los Angeles). A similar wide range characterizes estimates of take-up rates among families in the Section 8 voucher arm: 0.45 to 0.77. The substantial differences in take-up rates likely reflects the significant differences in the characteristics of participants and cities (e.g., housing market) across sites, and

---

4. See the NBER website that provides much useful information about the MTO study (link). The data are derived from aggregate data that “have been expanded to a pseudo individual level dataset (n=3273) that for each outcome mimics that outcome’s mean value, standard deviation, and approximate number of observations within a cell” (link to documentation file; quotation is from its page 4).
5. This is a smaller sample than that used in CHK, who used 4,604 families who were initially randomized. To calculate the mean within each group, I follow the documentation (link) of Ludwig et al. (2012).
6. These are similar to the voucher take-up rates reported in the Final Impacts Evaluation (Sanbonmatsu et al. 2011, 15, Exhibit 1.4).
these differences highlight why an “average” effect found by CHK may not be particularly relevant to any specific city or location.

I use these voucher take-up rates along with the ITT earnings estimates reported by CHK to calculate TOT estimates of the effect of moving on earnings. In their Online Appendix CHK reported ITT estimates for earnings for the two randomized groups by site and I reproduce those estimates here in Table 2.

**TABLE 2. ITT estimates for earnings by experimental group, child age and city/site**

<table>
<thead>
<tr>
<th>City/Site</th>
<th>Experimental voucher</th>
<th>Section 8 voucher</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Younger children</td>
<td>Older children</td>
</tr>
<tr>
<td>Baltimore</td>
<td>414.9</td>
<td>2720.4</td>
</tr>
<tr>
<td></td>
<td>(1435.8)</td>
<td>(2235.1)</td>
</tr>
<tr>
<td>Boston</td>
<td>2618.7</td>
<td>−3456.7</td>
</tr>
<tr>
<td></td>
<td>(1713.2)</td>
<td>(2105.8)</td>
</tr>
<tr>
<td>Chicago</td>
<td>681.2</td>
<td>−2336.1</td>
</tr>
<tr>
<td></td>
<td>(877.8)</td>
<td>(1467.8)</td>
</tr>
<tr>
<td>Los Angeles</td>
<td>2791.5</td>
<td>−508.3</td>
</tr>
<tr>
<td></td>
<td>(1554.9)</td>
<td>(1629.8)</td>
</tr>
<tr>
<td>New York</td>
<td>1652.4</td>
<td>−583.2</td>
</tr>
<tr>
<td></td>
<td>(1612.4)</td>
<td>(2051.0)</td>
</tr>
</tbody>
</table>

Source: Chetty et al. (2016, Appendix Table 7b). Note: Standard errors in parentheses.

The TOT estimates of the effect of moving on earnings can be calculated from these ITT estimates by dividing estimates in Table 2 by the take-up rates in Table 1. The heterogeneity of ITT estimates reported by CHK is reflected in the TOT estimates in Table 3. The impact of moving among younger children in the experimental voucher arm ranged from $754 in Baltimore to $6,090 in Boston. The analogous range of TOT estimates for younger children in the Section 8 voucher arm was −$786 in New York to $5,408 in Boston. Note how Boston has largest effects for both groups. Among older children in the experimental voucher arm, moving was associated with a change in earnings of between −$8,039 in Boston to $4,946 in Baltimore. The analogous range of TOT estimates for older children in the Section 8 voucher arm was −$6,913 in Boston to $1,301 in Los Angeles. Again, Boston has the largest effects.

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7. To calculate standard errors of the TOT estimates in Table 3, I assumed that the ITT estimates in Tables 1 and 2 were normally distributed random variables with the mean being the ITT estimate and the standard deviation being the standard error. I then drew 5,000 samples of each ITT estimate and calculated the TOT for each of the 5,000 draws. The standard deviation of 5,000 TOT estimates is used as the standard error in Table 3. To assess the accuracy of this approach, I did the same thing using the ITT estimates reported in CHK. The standard errors I obtained using this approach were very close (i.e., within 3 percent) to those reported by CHK.
TABLE 3. Calculated TOT estimates of the effect of moving on earnings by child age and city/site

<table>
<thead>
<tr>
<th>City/Site</th>
<th>Experimental voucher</th>
<th>Section 8 voucher</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Younger children</td>
<td>Older children</td>
<td>Younger children</td>
<td>Older children</td>
</tr>
<tr>
<td>Baltimore</td>
<td>754.4</td>
<td>4946.2</td>
<td>1123.5</td>
<td>-1455.1</td>
</tr>
<tr>
<td></td>
<td>(2627.5)</td>
<td>(4112.0)</td>
<td>(2071.8)</td>
<td>(2484.4)</td>
</tr>
<tr>
<td>Boston</td>
<td>6090.0</td>
<td>-8038.8</td>
<td>5408.2</td>
<td>-2517.9</td>
</tr>
<tr>
<td></td>
<td>(4061.2)</td>
<td>(5070.7)</td>
<td>(3301.3)</td>
<td>(4237.4)</td>
</tr>
<tr>
<td>Chicago</td>
<td>2064.2</td>
<td>-7079.1</td>
<td>1189.9</td>
<td>-6913.3</td>
</tr>
<tr>
<td></td>
<td>(2660.5)</td>
<td>(4570.2)</td>
<td>(1674.3)</td>
<td>(2305.5)</td>
</tr>
<tr>
<td>Los Angeles</td>
<td>4431.0</td>
<td>-806.8</td>
<td>1339.9</td>
<td>1301.3</td>
</tr>
<tr>
<td></td>
<td>(2502.1)</td>
<td>(2575.3)</td>
<td>(2002.8)</td>
<td>(2386.1)</td>
</tr>
<tr>
<td>New York</td>
<td>3442.5</td>
<td>-1215.0</td>
<td>-785.6</td>
<td>458.9</td>
</tr>
<tr>
<td></td>
<td>(3389.7)</td>
<td>(4213.8)</td>
<td>(3246.3)</td>
<td>(5524.0)</td>
</tr>
</tbody>
</table>

Notes: Author’s calculations. Standard errors in parentheses.

Assessing explanations of the effect of moving on earnings

CHK argue that neighborhood poverty is a likely explanation of the average estimates of the effect of moving on earnings they found for younger children. Is the hypothesis consistent with the heterogeneity of estimates in Tables 2 and 3?

I used the same data provided by Ludwig et al. (2012) to calculate the TOT effect of moving on neighborhood poverty for each experimental arm and site. I used the neighborhood (Census tract) poverty rate five years post randomization. The ITT estimates for neighborhood poverty are presented in Table 4.

The TOT estimates of the effect of moving on neighborhood poverty are calculated by dividing estimates in Table 4 by analogous estimates of the ITT estimate of take-up in Table 1.8 Again, I assume that younger and older children in each arm moved to the same places. Table 5 shows the calculated TOT estimates.

Estimates in Table 5 indicate that there is significant variation across sites in the effect of moving on neighborhood poverty within experimental arms, and between arms within a site. For example, among families in the experimental voucher arm in Baltimore, moving lowered neighborhood poverty by 14 percentage points relative to the control group. In Chicago, Los Angeles, and New York, the analogous estimates are approximately 23 percentage points. Similar heterogeneity characterizes TOT estimates of the effect of moving on poverty

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8. Standard errors are calculated as described in footnote 7.
among the Section 8 voucher families, although the change in poverty is lower than that among families in the experimental voucher arm.

TABLE 4. ITT Estimates of neighborhood poverty by experimental group and city/site

<table>
<thead>
<tr>
<th>City/Site</th>
<th>Experimental voucher</th>
<th>Section 8 voucher</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baltimore</td>
<td>−0.077 (0.018)</td>
<td>−0.057 (0.020)</td>
</tr>
<tr>
<td>Boston</td>
<td>−0.081 (0.012)</td>
<td>−0.058 (0.013)</td>
</tr>
<tr>
<td>Chicago</td>
<td>−0.075 (0.018)</td>
<td>−0.038 (0.019)</td>
</tr>
<tr>
<td>Los Angeles</td>
<td>−0.139 (0.015)</td>
<td>−0.127 (0.016)</td>
</tr>
<tr>
<td>New York</td>
<td>−0.111 (0.013)</td>
<td>−0.058 (0.014)</td>
</tr>
</tbody>
</table>

Notes: Author’s calculations using data from Ludwig et al. (2012). Standard errors in parentheses.

TABLE 5. Calculated TOT estimates of the effect of moving on neighborhood poverty by experimental group and city/site

<table>
<thead>
<tr>
<th>City/Site</th>
<th>Experimental voucher</th>
<th>Section 8 voucher</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baltimore</td>
<td>−0.14 (0.036)</td>
<td>−0.07 (0.026)</td>
</tr>
<tr>
<td>Boston</td>
<td>−0.19 (0.034)</td>
<td>−0.10 (0.025)</td>
</tr>
<tr>
<td>Chicago</td>
<td>−0.23 (0.061)</td>
<td>−0.06 (0.028)</td>
</tr>
<tr>
<td>Los Angeles</td>
<td>−0.22 (0.026)</td>
<td>−0.18 (0.024)</td>
</tr>
<tr>
<td>New York</td>
<td>−0.23 (0.032)</td>
<td>−0.13 (0.034)</td>
</tr>
</tbody>
</table>

Notes: Author’s calculations using data from Ludwig et al. (2012). Standard errors in parentheses.

Using the TOT estimates of the effect of moving on earnings and the TOT estimates of the effect of moving on poverty, I assess whether there is a correlation between these estimates, which would be expected if neighborhood poverty was an important explanation of the earnings effects. Focusing on the results for younger children, I plot estimates of the effect of moving on earnings from Table 3 against estimates of the effect of moving on neighborhood poverty from Table 5. If neighborhood poverty is an explanation of the effects of moving on earnings, it is reasonable to expect that there is a dose-response (positive) association where
larger earnings estimates are associated with larger poverty changes. 9 Figure 1 graphs the relationship.

**Figure 1.** Estimates of the effect of moving on earnings (Y) and neighborhood poverty (X) by site for young children in experimental voucher and Section 8 voucher arms

Figure 1 shows that, at best, there is only a weak positive relationship between estimates of the effect of moving on earnings and the change in poverty. An OLS regression of the earnings estimates on the poverty estimates yields an insignificant coefficient on poverty (p-value 0.34) in regressions both unweighted and weighted (by number of children at time of randomization in each site and experimental arm). The coefficient suggests that every 10 percentage-point decrease in poverty is associated with a $1,125 (approximately 10 percent) increase in earnings of younger children. 10 Note that Figure 1, and the regression results associated with it, are across sites and does not take into account a site-specific effect that is obvious in the data. For example, earnings effects are relatively high in Boston and low in Baltimore.

While there is a relatively weak positive relationship between the effect of moving on earnings and the effect of moving on poverty across sites, that is not the evidence that CHK relied on to support their claim. Instead, the primary evidence used by CHK to support their claim is the average differences between the effect

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9. For a similar analysis see Kling et al. 2007, Figure 2.
10. CHK (p. 873) report the ratio of the TOT estimate of the effect of moving on earnings and the TOT, which yields a measure of the earnings change per unit of poverty change. The calculated ratios indicate that a 10 percentage-point reduction in poverty is associated with a 13 percent earnings gain for the Section 8 voucher children and a 15 percent earnings gain for children in the experimental voucher arm. Note that the ratio does not calculate the association between the change in poverty and change in earnings, which is what my calculation above does.
of moving on younger children’s earnings and neighborhood poverty between the experimental voucher and Section 8 voucher arms (see quotes above). Again, such argumentation loses sight of how heterogeneous the results are between sites.

To assess the evidence used by CHK to support the poverty explanation more fully, I plot the difference in estimates of the effect of moving on earnings between experimental arms (experimental voucher minus Section 8 voucher) within a site against the difference in the effects of moving on poverty between experimental arms within a site. This approach accounts for site-specific effects. If poverty was an explanation of the estimates of the effect of moving on earnings, then it is reasonable to expect the difference in estimates of the effect of moving on earnings between experimental arms within a site to be positively related to the difference in the effect of moving on poverty between experimental arms within a site.

**Figure 2.** Difference in estimates of the effect of moving on earnings (Y) and moving on poverty between experimental voucher and Section 8 voucher arms for young children by site

As observed in Figure 2, there is no relationship between the difference in the effect of moving on earnings between experimental arms and the difference in the effect of moving on poverty between experimental arms. This no-relationship finding is obscured in CHK because of their focus on average effects. While there is a larger earnings gain and a larger poverty reduction for children in the experimental voucher arm than in the Section 8 voucher arm, that relationship only holds on average. The same dose-response relationship is not present in the underlying constituent parts of that average. Differences in earnings between experimental arms are large in Los Angeles and New York while differences in poverty between experimental arms are large in Boston, Chicago, and New York.
Unlike New York, however, differences in earnings between experimental arms in Boston and Chicago are relatively small. Overall, the heterogeneity of results by experimental site and experimental arm are not supportive of the poverty explanation for the effect of moving on earnings.

As for the effects of moving on earnings for older children, CHK suggest that the disruption of moving is a plausible explanation. If so, then whether a child was in the experimental voucher arm or Section 8 voucher arm should matter little because the disruptive effects of moving are arguably the same in both cases. Figure 3 plots the difference in estimates of the effect of moving on earnings between experimental arms (experimental voucher minus Section 8 voucher) for older children.

**Figure 3.** Difference in estimates of effect of moving on earnings (Y) between experimental voucher and Section 8 voucher arms for older children by site

Figure 3 shows that the difference in estimates of the effect of moving on earnings by experimental arm among older children were similar in Chicago, somewhat different (roughly −$2000) in LA and New York, and very different in Baltimore and Boston. Thus, evidence in Figure 3 does not lend support to the disruption explanation for the earnings results of older children. In only one case is the effect of moving on earnings among older children relatively similar for those in the experimental voucher and Section 8 voucher arms.

Differences in the effect of moving on poverty by experimental arm and site are unlikely to reconcile the results in Figure 3 with the disruption explanation. For example, in Chicago, older children in the experimental voucher arm experienced a much larger reduction in poverty than older children in the Section 8 voucher arm. If living in a lower poverty neighborhood was beneficial for children and offset some of the disruption effect of moving, then the differences between the
experimental voucher and Section 8 voucher groups in Chicago should be the largest. In fact, they are the smallest. Similarly, in Baltimore, the difference in poverty among children in the experimental voucher and Section 8 voucher arms was relatively small, but older children in the experimental voucher arm had much higher earnings than older children in the Section 8 voucher arm. Overall, the heterogeneity of results across sites and experimental arms is not supportive of the disruption explanation.

Discussion

The findings reported by CHK about the effects of moving out of public housing during childhood on adult earnings are important. On average, moving clearly had effects on earnings and these effects differed by experimental arm and age. However, the average effects reported by CHK, which were the basis of their conclusions, obscured a significant amount of heterogeneity of results across the five experimental sites. Once that heterogeneity is considered, the conclusions put forth by the authors, that neighborhood poverty explains the (average) positive effect of moving on earnings for young children and that disruption explains the (average) negative effects of moving on earnings for older children, are not reliable and may be misleading.

The MTO was five separate experiments conducted in five cities. The composition of families differed significantly across sites (see Orr et al. 2003, Table C1.2). For example, only 33 percent of families were African-American in Boston while 98 percent of families were African-American in Chicago. Similarly, only 51 percent of families in Boston relied on AFDC as their primary source of income whereas 65 percent of families did in New York. In New York 54 percent of families were headed by a never-married person (mostly women), but in Baltimore the figure is 74 percent. It is also the case that the baseline neighborhood quality differed substantially across sites (Aliprantis and Richter 2014). Finally, housing markets and other characteristics (e.g., segregation, racial/ethnic composition) also differed by city, as did the voucher take-up rates across sites and experimental arms.

Given these differences in family, neighborhood, and city characteristics, it is not surprising that effects of moving out of public housing during childhood on adult earnings differed qualitatively across places. While confidence intervals of the estimates of the effect of moving on earnings by site are large, the point

11. In fact, while few previous MTO studies reported estimates separately by experimental site, there is evidence of such heterogeneity in the interim evaluations. For evidence on test scores, see Sanbonmatsu et al. 2006.
estimates are quite heterogeneous and reasonably suggest that there were substantial differences in the effects of moving on earnings across sites. It is also evident that the likely site-specific heterogeneity was not taken into account when designing the experiment, as sample sizes within each site are generally too small to detect reliably anything but large effect sizes. Even when pooling samples across sites, standard errors on estimates of the effect of moving on earnings reported in CHK are not sufficiently precise to detect reliably an effect size smaller than 20 percent to 30 percent. Effect sizes of this magnitude are large when measured against the fact that the exposure to a low-income (<20 percent poverty) neighborhood among experimental groups is approximately one to two years during an entire childhood. In fact, if estimates in CHK are judged solely by conventional levels of significance, as one may want to do for the analysis presented here, there are few results of importance, as most are not statistically significant and most differences of estimates across experimental arms, and even age groups, are not statistically significant.\(^{12}\)

The implication of my analysis is that taking into consideration the substantial heterogeneity, between sites and arms, of estimates of the effect of moving on earnings weakens the evidence supporting the conclusions that neighborhood poverty is a primary explanation of the (average) increase in earnings associated with moving among younger children, and that disruption from moving is a primary explanation of the (average) decrease in earnings associated with moving among older children. Therefore, it seems imprudent to undertake policies and implement experiments based on the conclusions of CHK. The next experiment could just as likely replicate the results of Baltimore in which younger children did worse than older children and older children experienced a positive increase in earnings on average than replicate the average effect. Or it could be like New York where children in the Section 8 voucher arm experienced little change in earnings. We should ask ourselves: If a new MTO experiment takes place in Houston, Phoenix, Philadelphia, or any other city, will the results align with the conclusions that CHK published in the *American Economic Review*? Based on the evidence I review, I see little reason to think so.

As Nancy Cartwright (2013) argues, in most cases evidence from one experiment, for example in Boston, will not travel well and will not be replicated in another experiment, for example in Baltimore. The reason, she says, is that the structure and context that underlies the causal effect is likely to differ across the two experiments. She refers to supporting factors, which in the case of MTO are

\(^{12}\) The age-specific estimates of the effect of moving on earnings reported in CHK’s Figure 2 and Appendix Figure 2 are almost all not statistically different from zero or each other, and do not suggest a strong age gradient even if one ignores statistical significance.
the characteristics of the participants and their baseline circumstances, and the characteristics of the cities, such as the spatial configurations of neighborhoods, racial/ethnic composition, extent of segregation, and the features of the housing market. She argues that it is only when these supporting factors are the same across experiments is it likely that the results of one experiment will replicate in another experiment. In the case of MTO, the supporting factors across sites clearly differed and so did the results. More importantly, there is no unique set of ‘average’ supporting factors underlying the average effects of the MTO experiment. The old saying that no one is the average is particularly apt in the case of the MTO. Therefore, it is highly unlikely that implementing an experiment designed on the basis of the average effects of the MTO experiment will produce results similar to that average.

In summary, while we have some evidence that moving increased adult earnings, at least for younger children, we do not know much about what caused this effect, and it is based on a noisy, large average experimental estimate obtained from an under-powered experiment. While CHK suggest it is the poverty rate in the neighborhood, a closer look at the evidence casts doubt on that explanation even when considering the noisy nature of the evidence. It seems reasonable to proceed more cautiously when drawing conclusions about the causes of the effects from moving and designing future experiments. So, yes, we should conduct more experiments about the effects of a child’s environment on their current and future outcomes, but we should do so in an exploratory way that tests mechanisms beyond neighborhood poverty (e.g., Laliberté 2020).

References


Ludwig, Jens, Greg J. Duncan, Lisa A. Gennetian, Lawrence F. Katz, Ronald C.
Robert Kaestner is a Research Professor at the Harris School of Public Policy of the University of Chicago. He is also a Research Associate of the National Bureau of Economic Research, an Affiliated Scholar of the Urban Institute and a Senior Fellow of the Schaeffer Center for Health Policy of USC. Prior to joining Harris, Kaestner was on the faculty of the University of Illinois, University of Illinois at Chicago, University of California, Riverside, the CUNY Graduate Center and Baruch College (CUNY). He received his Ph.D. in Economics from the City University of New York. He received his BA and MA from Binghamton University (SUNY). His research interests include health, demography, labor, and social policy evaluation. He has published over 125 articles in academic journals. Recent studies have been awarded Article of the Year by AcademyHealth in 2011 and the 2012 Frank R. Breul Memorial Prize for the best publication in Social Services Review. Dr. Kaestner has also been the Principal Investigator on several NIH grants focused on Medicare and Medicaid policy. Kaestner is an Associate Editor of the Journal of Health Economics and the American Journal of Health Economics, and on the Editorial Board of Demography and Journal of Policy Analysis & Management. His email is kaestner.robert@gmail.com.

Raj Chetty¹, Nathaniel Hendren², and Lawrence F. Katz³

In Chetty, Hendren, and Katz (2016), we revisited the Moving to Opportunity (MTO) experiment, which provided housing vouchers to families living in high-poverty public housing projects in the mid-1990s to move to lower-poverty areas.

Earlier work found little effect of MTO moves on the economic outcomes of adults and older children (e.g., Kling, Liebman, and Katz 2007; Ludwig et al. 2012). We returned to the data to study the long-run economic impacts on younger children. We found that young children (less than 13 years old at the time of random assignment) in the experimental group who moved using an MTO voucher grew up to have 31 percent higher incomes than those in the control group who did not have access to an MTO voucher. In addition, young children in the MTO experimental voucher group were more likely to attend college, more likely to be married, and less likely to give birth as a single mother.

Robert Kaestner (2020) revisits our analysis and its interpretation. He does not report new empirical results; rather, he questions the interpretation of the findings we reported. In this response, we discuss the premises of and conclusions of each of these critiques.

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Causal effects of poverty rates

Kaestner’s primary criticism is that Chetty, Hendren, and Katz “pervasively use language suggesting that their results show that moving to a low-poverty neighborhood increases adult earnings of children living in high-poverty areas.”

As background, the MTO experimental voucher arm provided vouchers to low-income families living in high-poverty public housing projects to move to Census tracts with a poverty rate below 10 percent. Because the MTO experimental treatment explicitly required that families had to move to low-poverty neighborhoods, we followed prior literature and referred to the neighborhoods to which these families moved as “low-poverty neighborhoods.” We recognized that this language could be misinterpreted to suggest that a particular characteristic of a neighborhood (e.g., poverty rates or peer effects from low-income peers) has a causal effect on children’s upward mobility. For this reason, we wrote: “The treatment effects we report in this paper should thus be interpreted as the effect of changing a bundle of neighborhood attributes rather than any one feature of neighborhood environments” (Chetty et al. 2016, 869).

Kaestner (2020) repeats the quote above but then points out other instances in which, he argues, we implicitly suggest moving to a lower-poverty neighborhood increased these children’s outcomes in adulthood. We had no such intention and disagree with this interpretation. All of the statements Kaestner (2020) quotes are true when understood through the factual lens that MTO required families in the experimental group to move to lower-poverty neighborhoods—defined as census tracts with poverty rates lower than 10 percent as of the 1990 Census—to use their vouchers. This was a definitional characteristic of the MTO program and therefore it is accurate to reference these neighborhoods as “low-poverty.” In doing so, we did not imply that exogenously lowering the poverty rate in a neighborhood or providing a child with peers from higher-income families, ceteris paribus, will necessarily lead the child to have higher upward mobility.

More broadly, this discussion boils down to the difference between correlation and causation. Our results demonstrate that neighborhoods have causal effects on children’s outcomes. And, on average the lower-poverty neighborhoods to which families moved in the MTO experiment caused their children to have higher upward mobility. But, this does not mean that the reason those children had higher mobility is because they had fewer peers in poverty. Rather, those neighborhoods

4. Moreover, in our subsequent work providing search assistance and support services for families with children seeking to move to neighborhoods with higher upward mobility for their children, we used estimates from the Opportunity Atlas on observed upward mobility in each Census tract as opposed to the poverty rate to define opportunity neighborhoods (Bergman et al. 2020).
likely also had better schools, teachers, environmental quality, etc., all of which could have independent causal effects on children’s outcomes (but that might be correlated with poverty rates). Our analysis cannot separate those effects, as we noted in the quote above.

Beyond our intentions, the subsequent literature has not misinterpreted our writing to mean that poverty rates are the key causal determinant of neighborhood effects. Indeed, a rich literature has since emerged that investigates why low-poverty neighborhoods improve children’s outcomes and in particular finds many factors beyond poverty rates that are correlated with differences in upward mobility across neighborhoods (e.g., Manduca and Sampson 2019; Rothstein 2019), and we ourselves have studied this question at length in subsequent work (Chetty et al. 2020).

**Subgroup heterogeneity and statistical precision**

Kaestner (2020) next observes that there is less statistical precision of the estimates when evaluated by city rather than when pooling data from all five cities where the MTO experiment was conducted. This is a mechanical feature of working with smaller samples. What is more informative is that our confidence intervals at the site level contain the pooled point estimate, showing that there is no evidence for (or against) the existence of treatment effect heterogeneity across sites.

In addition, Kaestner (2020) notes we find an effect that declines with age and is sometimes negative for children whose families obtained the vouchers when they were teenagers, which he views skeptically. As we discuss in our original paper, this pattern is consistent with a childhood exposure effects model whereby each year of childhood spent growing up in a more upwardly mobile neighborhood can improve children’s outcomes, combined with disruption effects for moving with teenagers (which may also explain why families with children in high school move at lower rates).

A broader concern that might be raised by Kaestner’s critiques is that our results were obtained as the result of specification searching. To assess this potential concern, we conducted several tests including a randomization inference evaluation. We refer readers to Section IV.C of our Chetty, Hendren, and Katz (2016). Additionally, we note that the results of our MTO study—with declining impacts of moving to better neighborhoods by age—have now been replicated in several other experimental and quasi-experimental studies (Chetty and Hendren 2018; Chyn 2018; Deutscher 2020; Faurschou 2018; Laliberté 2020).
Variation in poverty rates across sites

Lastly, Kaestner uses the variation in estimates across sites to explore the relationship between the impact of MTO moves on children and the site-average change in poverty rate exposure for those children using the treatment-on-treated estimates. Here he notes that there is not a strong relationship (Kaestner 2020, 292 Figure 1), and therefore he again questions whether the neighborhood poverty rate is the true “explanation” driving neighborhood upward mobility outcomes.

As we noted above, we do not claim that the neighborhood poverty rate is the causal factor affecting economic outcomes to begin with. Nonetheless, we note that this exercise in Kaestner (2020) is not well designed to test this claim. As discussed above, there are many other factors that vary across places. Therefore, even though poverty-rate exposure changes more for children in one city than another, we do not know what other bundles of factors also change for the children in those two cities. As a result, the identification assumption underlying the test conducted by Kaestner (2020) looking at heterogeneity in poverty-rate changes across sites is inconsistent with his primary claim that there are other factors beyond poverty rates at the area level that might have causal effects on children.

To estimate the causal effect of particular policies or factors, one needs to isolate causal variation in that factor. For example, recent work by Ellora Derenoncourt (2019) uses exogenous variation in the Great Migration of African-Americans north in the early-mid 20th Century to show that cities who randomly had larger migration have lower rates of upward mobility today. We hope our work can inspire further analyses of this form to help uncover the causal drivers and historical factors that generate differences in opportunities across neighborhoods.

Conclusion

We agree with Kaestner’s conclusion that the MTO experiment does not provide evidence on the causal pathways through which neighborhoods affect children’s long-run outcomes. Rather, our results establish that neighborhoods matter for children, and that the lower-poverty neighborhoods to which children in the experimental voucher group moved improved their outcomes in adulthood. We made these points in our 2016 article and appreciate the opportunity to restate them here.
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Re-examination of the Theoretical and Historical Evidence Concerning Colonial New Jersey’s Paper Money, 1709–1775: A Further Comment on Grubb

Ronald W. Michener

In Michener (2019a) in this journal, I pointed out several egregious econometric errors in Grubb (2016a), an article in the Journal of Economic History on New Jersey’s colonial money. The present rejoinder is the latest of a lengthy series of comments I have authored or coauthored pointing out errors in articles Grubb has published concerning the monetary history of eighteenth-century America. I prefaced my most recent comment, Michener (2019a), with a long discussion of my motives, explaining how Grubb’s mistaken notions, if allowed to stand unchallenged, would distort our understanding of the economic history of the era, including our understanding of the discontent that led to the American Revolution. Grubb (2020, 72), ignoring that lengthy discussion, countered with his own interpretation of my motives, suggesting that I am in the grip of some “unnatural, unseemly, and weirdly personal” obsession that somehow dates back more than 40 years, when, blissfully unaware of one another, we overlapped briefly as students at the University of Chicago.

Grubb prefaced his rebuttal by relating the history of our dispute, the “back story.” Here is the back story from my perspective. For the last 15 years Grubb has dismissed my critiques, both publicly and privately, as no more than the ravings of

1. University of Virginia, Charlottesville, VA 22904.
a malcontent pursuing a vendetta. Grubb’s network of friends and acquaintances in economic history is much more extensive than mine, and he has methodically exploited his standing in the profession to defame me. Until 2018, I toiled away struggling to counteract his rewriting of colonial history, but I eventually concluded that my approach up to 2018 was insufficient to bring about the necessary correction. Economists, I came to realize, were never going to immerse themselves in the eighteenth-century documents that would reveal that Grubb peddles something closer to fantasy than history. Accordingly, in Michener (2019a; b) I switched my focus to econometrics and wrote two comments highlighting the shoddiness of Grubb’s empirical work, hoping that journal editors and his colleagues at the NBER, more skilled at parsing econometrics than colonial American history, might come to realize that Grubb was publishing nonsense with their imprimatur. To encourage others to re-examine Grubb’s empirics, I included a link to the data and do files (Michener 2019a, 212).

Grubb (2020) hotly disputes many of the specific criticisms in Michener (2019a), and, although I shall eventually return to discuss some of them, I am reluctant to drag the reader into that briar patch just yet. Instead, I note that Grubb (2020, 76–77) carefully puts some distance between himself and his econometrics: the data set is short, the variables are measured with error, trustworthy standard errors are difficult to generate, and the econometric results were “ancillary to the core results,” namely, the agreement between the levels of APV and MEV. Indeed, he goes so far as to defend the decomposition model he estimates as an identity (ibid., 77). The heart of Grubb (2016a), as I read Grubb’s 2020 rebuttal, lies in the historical analysis and the theoretical model; hence, even if the reader finds fault with his econometrics, nothing substantive has been undermined. Taking this at face value, let me turn my attention to the deficiencies in Grubb’s theoretical model and historical analysis.

Grubb’s theoretical measures and their weaknesses: APV

In summarizing what he describes as a “promising new approach to understanding colonial paper money,” Grubb (2016b, 183) advocates “treating the paper money of most colonies as zero-coupon bonds rather than as fiat currency, and assuming its value is primarily determined by its asset present value.” Grubb introduces a measure he calls APV (Asset Present Value), which is computed by discounting the value at which colonial governments received their own paper money at the colonial treasury. Practically all the market value of colonial bills
of credit, he contends, can be explained as arising from the intrinsic value the bills possessed considered merely as ersatz discount bonds. Applying this to New Jersey, Grubb (2016a; b) computes the APV of New Jersey bills and then compares it to their MEV (Market Exchange Value). The econometric flaws in this comparison were the subject of my comment (Michener 2019a).

APV, Grubb’s method of inferring the intrinsic value the bills possessed considered merely as discount bonds, is, moreover, theoretically flawed. The problem is most easily understood by computing APV for a simple hypothetical security, coupons, that are valued simply because they can be used to extinguish tax liabilities. In this example, the government levies a tax of $100 a year in perpetuity, which may be paid in cash or in coupons. Each coupon you tender to the tax collector reduces your tax bill by a dollar; there is no other use for the coupons. Suppose there are 500 coupons in existence. What is the value of such a coupon in the first period? Grubb (2016a, 1221, 1224; 2016b, 168–170) infers its value in the following manner: One hundred coupons would be tendered in taxes in the first period, another hundred in the second, and so on until at the end of five years all the coupons have been redeemed. Because you do not know when any particular coupon will be presented, the expected payoff per coupon, discounted at the interest rate $r$, is \[ \text{APV} = \frac{1}{5} \left( 1 + e^{-r} + e^{-2r} + e^{-3r} + e^{-4r} \right). \]

My approach to inferring the value of a coupon in the first period begins with the demand curve for coupons. The derivation is straightforward. No one would pay more than one dollar for a coupon, but if you sold coupons for one dollar, you could sell as many as 100 coupons. At any price $e^{-r} < p < 1$ you’d sell exactly 100 coupons. At a price of $e^{-2r}$ you could sell as many as 200 coupons, and at any price $e^{-2r} < p < e^{-r}$ you’d sell exactly 200. At a price of $e^{-2r}$ you could sell as many as 300 coupons. Once the price fell to $e^{-3r}$ you could sell as many as 400 coupons, and so on. The demand and first period supply curves look like this:

![Figure 1. Demand and first period supply curves](image)
This analysis shows that the market value of one of these coupons in the first period would be $e^{-4r}$, not APV. Hence, APV overestimates the value coupons possessed as discount bonds. The demand curve can deliver further insights. At the beginning of the second period, the demand curve is the same, but the supply curve shifts in to 400, boosting the price to $e^{-3r}$. Similarly, at the beginning of the third period supply shifts in to 300 and the price of each coupon rises to $e^{-2r}$; at the beginning of the fourth period, the price is $e^{-r}$, and in the fifth and final period, one. The coupons’ value rises by the rate of interest each period and satisfies the terminal condition that price in the final period is one; the demand analysis reveals the coupons’ value behaves precisely like the value of any other discount bond. To concede that such coupons would appreciate at the rate of interest would be utterly fatal to Grubb’s argument. Grubb (2016b, 174–178) acknowledges as much when he touts the stability of APV as a virtue of his theory, on the grounds that it reconciles discounting with the observed stability in the value of colonial currencies.

Perhaps the greatest insight of neoclassical economics is that value is determined on the margin, and APV’s conceptual flaw is that it violates this fundamental principle. Indeed, each of these hundred-coupon tranches corresponds precisely to one of Eugen Böhm-Bawerk’s (1891, 149–153) famous sacks of corn, an early and classic illustration of marginal utility analysis. Böhm-Bawerk describes a farmer with 5 sacks of corn: “One sack he absolutely requires for the sustenance of his life till the next harvest. A second he requires to supplement this bare living to the extent of keeping himself hale and vigorous.” The third sack is set aside “to feed poultry. A fourth sack he destines to the making of coarse spirits.” The fifth sack serves only to feed “a number of parrots, whose antics amuse him.” On a scale of importance, the farmer is said to assign a value of 10 to the sustenance of his life; 8 to the maintenance of his health, 6 to the improvement of his fare by adding meat, 4 to the enjoyment he derives from the liquor, and 1 to the amusement provided by the parrots. Should he lose a sack of corn, Böhm-Bawerk reasoned, the man would forgo the pleasure of keeping the parrots, and “he will rightly value a single sack of his stock according to this unimportant utility. And not only one sack, but every single sack; for, if the sacks are equal to one another, it will be all the same to our farmer whether he lose sack A or sack B, so long as, behind the one lost, there are still four other sacks for the satisfying of his more urgent wants.” Grubb’s APV logic, however, would value the five sacks of grain possessed by Böhm-Bawerk’s farmer not at one, but rather at $0.20 \times (10+8+6+4+1) = 4.8$.

2. In Michener (2019a, 192), citing an unpublished working paper, I casually mentioned that Grubb based APV on its average utility rather than its marginal utility, putting his theory on the wrong side of the
Ergo, APV, the theoretical linchpin of Grubb (2016a; b; 2018a), Celia and Grubb (2016), and Cutsail and Grubb (2019), does not measure the value these securities possessed considered as discount bonds. Without the linchpin, the wheels fall off Grubb’s research agenda.

Without other problematic steps in computing APV, even the abovementioned flaw would not have made APV large enough to align New Jersey’s APV with MEV as Grubb calculates it. To understand what else Grubb did to boost APV requires a more detailed investigation of APV. Grubb’s calculated values of APV are more intricate than the example used above to illustrate the theoretical shortcomings of APV. Grubb’s (2016a, 1224) general formula for APV is

\[ \text{APV}_i = \sum_{t=i}^{T} \left( \frac{\text{RED}_t}{M_i} \right) e^{-r_t}, \]

where \( M_i \) is the face value amount of New Jersey bills outstanding in year \( i \), \( r_t \) is the interest rate (which Grubb takes to be either 6 or 8 percent), \( \text{RED}_t \) is the face value amount of New Jersey bills redeemed and retired from circulation each year, and \( \text{RED}_T \) is the amount in the last year of redemption that satisfies \( \sum_{t=i}^{T} \left( \frac{\text{RED}_t}{M_i} \right) = 1 \).

Therefore, APV is inherently forward-looking: Today’s value of APV depends on the path of future redemptions stretching out into the future until all currently extant bills have been redeemed. Although the path of future redemptions envisioned in emitting acts was usually smooth and regular, as in my example, the actual path achieved was not. Tax and loan receipts earmarked to retire bills frequently fell short of expectations, unanticipated demands on the colonial treasury sometimes arose, and imperial reimbursements earmarked to retire bills created for military purposes sometimes failed to materialize. New Jersey’s colonial legislature often altered taxes earmarked to retire bills, extended the periods of its bills, and rolled over redemption deficiencies into the next sum of bills emitted. Because decades might pass before New Jersey retired its current stock of bills, this incessant restructuring of redemption plans meant that future retirements were erratic and unpredictable.\(^3\) If one considers bills of credit as

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marginalist revolution and inflating the purported value of bills of credit considered as a discount security. Grubb (2020, 73) responded that I “made that up” and accused me of committing “a fundamental error in microeconomic theory. [Michener] conflates average utility with the average of the marginal utilities and conflates marginal utility with the marginal of the marginal utilities.” Is this anything more than a word salad? Grubb (2019, 402) is less obscure in an earlier rebuttal, however, where he says he “modeled the process as a random draw from a known distribution of payoffs” and that APV is “the average of the marginal utilities of the set of notes available at that point.” By this logic, Grubb would clearly value the five sacks of grain possessed by Böhm-Bawerk’s farmer not at one, but rather at \(.20 \times (10+8+6+4+1) = 4.8.\)

3. Grubb (2015) takes a rosy view of New Jersey’s success in redeeming its bills in a timely fashion but nevertheless accurately describes many instances where the colony reneged on or restructured its redemption plan. That actual retirements of bills ended up being erratic and unpredictable is not, I believe, a point in dispute.
discount bonds, they were discount bonds with random maturity dates that bond holders would have found exceedingly difficult to forecast.

In computing APV Grubb used realized future retirements to compute APV, as if the public had perfect foresight regarding them. The expectations problem admits to no easy solution, so I am not inclined to fault Grubb for assuming perfect foresight about future retirements. My quarrel is with his decision to silently abandon the perfect foresight assumption in the late colonial period when it mattered most. The value of colonial New Jersey’s bills of credit, considered as discount securities, evaporated during the Revolution: They became little more than wastepaper (The New Jersey Gazette, 30 January 1786). By replicating the calculations, I determined that Grubb computed his late colonial values of APV on the counterfactual assumption that during the Revolution colonial New Jersey continued to retire their bills of credit according to the previously legislated timetable. Figure 2 (Grubb 2016a, 1223) reveals that Grubb’s measured APV is generally under MEV in the late colonial period. Had Grubb applied his assumption of perfect foresight consistently, permitting the fate of New Jersey’s bills of credit to be correctly foreseen, APV would have tumbled on the eve of the Revolution, throwing the alignment of APV and MEV seriously out of kilter.

Grubb created his own data series on the New Jersey money supply and redemptions, and he used his own data to compute APV. Both Fuhlbruegge (1937) and Brock (1992) concluded, based on New Jersey treasurers’ reports, that on the eve of the Revolution New Jersey had fallen considerably behind schedule in retiring its bills, whereas Grubb’s (2015, 34) revised data contradict this assertion and purport to show that New Jersey’s retirements were precisely on schedule. By ignoring evidence of lagging retirements in the late colonial period, Grubb boosts APV. Nonetheless, despite the boost Grubb’s revisionist data provide to APV, New Jersey’s MEV usually exceeds APV in the late colonial period. Grubb’s “corrections” to the money supply data hence play a significant role in aligning APV with MEV. Without the boost provided by his data revisions, the two measures would be more severely misaligned, suggesting that in “correcting” the data Grubb manipulated it to support his hypothesis.

The questionable nature of Grubb’s “corrected” money supply data is most transparent in his treatment of the robbery of New Jersey’s eastern treasury on 21 July 1768. The sum stolen and never recovered was materially important, amounting to roughly two years of revenue from the property taxes levied for the support of government. Grubb (2015, 27 n.13) dismisses the relevance of the robbery for his calculations, denying it influenced the local money supply, arguing that 91 percent of the money in the treasury was in pounds sterling, part of the recently received parliamentary donations to the colonies, and therefore that most if not all of what was stolen undoubtedly consisted of “sterling” and not New Jersey
bills of credit. This is demonstrably wrong; almost all the stolen money consisted of New Jersey bills of credit, bills that would have been retired but for the robbery. The treasurer described exactly what was stolen in an official affidavit submitted four days after the robbery (NJ Archives, 10:37–39).

Overlooking the affidavit would be a regrettable blunder, but Grubb’s clumsy rationalization leads one to conjecture that the error was purposeful. The New Jersey treasury never possessed any cash in “sterling.” New Jersey placed the sterling-denominated donations Parliament provided into a London account; New Jersey’s treasurers drew on the account by selling bills of exchange on London (NJ Laws, 4:10). New Jersey’s treasurers used proceeds from the sale of bills of exchange to retire bills of credit, temporarily freeing the populace from the taxes earmarked to retire them. Grubb maintains that in 1765, three years before the robbery, the colony, having exhausted the last of its parliamentary donations, resumed retiring bills of credit with taxes (Grubb 2016c, 155–156 Table 6, 156 n.c). Furthermore, Grubb (2016c, 144–146 Table 3, 146 n.b) credits the colony with receiving and applying the full value of the parliamentary donations, which is impossible to square with his statement in Grubb (2015, 27 n.13) that a portion of it had been lost in the robbery.  

Grubb’s APV estimates required a final feat of legerdemain, empirically the most important of all. Colonial New Jersey never fully retired an existing emission before a new one was made.  New Jersey bills designed to be retired at different dates, bills easily distinguished by their inscriptions, circulated alongside one another. To extend my coupon example, it is as if the original 500 coupons were printed on blue paper, and after two years passed an additional 500 coupons, printed on red paper, were introduced. This raises a host of potential complications. Did people anticipate that 500 new red coupons would appear before the blue coupons had been retired? Are the new red coupons and the blue coupons fully fungible and perfect substitutes for one another or are they two distinct securities potentially bearing different prices? Does the introduction of the red coupons affect the value of the blue ones, and if so, how?

Grubb treats changes in the money supply as perfectly foreseen, an assumption that would imply people saw the red coupons coming. I would not

4. The treatment of parliamentary donations in Grubb (2016c) is also wrong. The eastern treasurer drew on the donations by selling bills of exchange on London, but he did not use all the proceeds to retire bills as Grubb says he did. Instead, he loaned out considerable sums of the proceeds to cronies, taking their bonds. Some of these loans were still outstanding in the 1770s. This is yet another reason that New Jersey’s retirement of bills of credit lagged in the decade before the Revolution. Evidence that the treasurer loaned the colony’s money to cronies is buried in the archives and is not generally known, making this a subtle error, unlike the stark contradiction between the account in Grubb (2015) and that in Grubb (2016c) of the parliamentary donations (Michener 2018, 24–26).

5. “Typically, colonies would authorize a new emission before the prior emission had been removed from circulation” Grubb (2016b, 174).
dispute this. That new emissions would occur before the previous ones had been completely retired was almost inevitable, just as it is inevitable today that whenever the federal debt ceiling binds, the U.S. government will raise it. Although the language of emitting acts suggests that the bills were not fungible—in terms of my example, that certain taxes could only be paid with blue coupons and others could only be paid with red coupons—Governor Franklin wrote a lengthy letter explaining that enforcing such a distinction was impossible, and that New Jersey’s treasurers accepted the bills indiscriminately (NJ Archive, 10:316–317). In terms of my example, the treasurers accepted red coupons in payment of blue coupon taxes and vice versa. Consequently, at the end of their stated term some blue coupons remained outstanding, red coupons having been retired in their place.

Fungibility is a difficult issue for Grubb to finesse. To dispute Franklin’s testimony and insist the bills were not fungible would mean bills of different emissions, my red and blue coupons, were distinct securities. If red coupons could only be used to pay red coupon taxes and blue coupons to pay blue coupon taxes, the coupons would possess different maturity dates and under Grubb’s hypothesis ought to have borne different prices, just as modern treasury bills with different maturity dates bear different prices. There is no evidence to support the proposition that New Jersey bills of different emissions—my blue and red coupons—possessed different values. Grubb tacitly agrees: His measures of APV or MEV are presented as measures for all New Jersey bills, with no distinction made between bills emitted on different dates. Because the bills/coupons were fungible and the infusion of red coupons displaced blue coupons in tax receipts, as Governor Franklin testified, the redemption of the original blue coupons would have dragged out over more than 5 years, which even by Grubb’s flawed logic would alter the distribution of payoffs of the blue coupons, reducing their value as discount bonds. Grubb, however, computes APV as if the fully anticipated creation of red coupons has no effect on either the retirement or value of blue coupons.

If the coupons are perfectly fungible, what effect would the infusion of red coupons have on the value of the existing blue coupons? If we adopt the perfect foresight assumption that underlies most of Grubb’s calculations, the answer can be worked out in various ways. One way is to use a supply and demand analysis but doing so is intricate. One must use backwards induction and then keep in mind that in the initial periods before the red coupons arrive, the demand curve shifts each period as the waiting time until their arrival shrinks. A more transparent demonstration comes from noting that the coupons are an exhaustible resource, and their value follows the well-known Hotelling rule (Hotelling 1931). The coupons’ value increases at the rate of interest subject to the terminal condition that the value in the final period is one. If it is known in advance that the red coupons will arrive before the blue coupons can be exhausted, the situation is precisely the
same as if 1,000 coupons had been created initially; hence, the anticipated arrival of 500 fungible red coupons in period three reduces the initial period value of blue coupons from $e^{-4}$ to $e^{-9}$.

The Revolution upended the colonial government, consigning New Jersey’s bills of credit to oblivion. If New Jersey bills had possessed no value as a medium of exchange, if they had had no value beyond the value they possessed as discount bonds accepted in lieu of taxes, their value would have been zero under the admittedly unrealistic assumption of perfect foresight. The conclusion follows from considering the bills as analogous to the coupons described above and applying the Hotelling rule. The terminal value of New Jersey’s colonial currency was zero, and the discounted value of zero is zero.

Even if one hesitates to treat the Revolution and repudiation of New Jersey colonial currency as foreseen, no one thought in 1774 that the New Jersey treasurer would be retiring the last New Jersey bill anytime soon. In 1774, the colony passed a law creating a new loan office designed to loan out new bills for a twenty-year term (NJ Laws, 5:212–234). In early 1776 the loan office commenced making loans. Hence, chances were nil that New Jersey bills would all be retired before 1796. The present value of a discount bond, discounting at six percent over a twenty-year term, would be about 30 percent of its face value, a number that falls to 9 percent of its face value over a forty year term (that is, discounting back to 1756), and to under 3 percent of its face value over a sixty year term (that is, discounting back to 1736). If we consider New Jersey bills of credit as no more than a discount bond even that valuation is generous: It requires that New Jersey’s colonial government not be overthrown, that the bills authorized in 1774 are retired on schedule, and that the bills authorized in 1774 would be the last bills the colony of New Jersey ever created. Even if the Revolution was entirely unforeseen, the value of New Jersey bills, considered merely as discount bonds, had to be a negligible fraction of their face value.

**Grubb’s theoretical measures and their weaknesses: MEV**

MEV, or market exchange value, is the value a bill of credit possessed in everyday transactions expressed as a percentage of the value at which it was received at the colonial treasury. In the late colonial period, it took 7 shillings and 6 pence in New Jersey bills of credit to purchase a Spanish silver dollar, but Grubb believes that New Jersey’s treasury accepted New Jersey bills at a premium, treating each 6 shillings in bills as equivalent to a Spanish dollar. For purposes of illustration,
let us pretend this is true. MEV would have been $100 \times \frac{6}{7.5} = 80$; that is, the value of New Jersey bills in everyday transactions would have been 80 percent of their value at the treasury. The difference between the value at the treasury and the value in ordinary transactions, 20 percent in this example, Grubb attributes to discounting. To understand why Grubb believes that the New Jersey’s treasurers accepted New Jersey bills at a premium requires a discussion into colonial history.

Attempting to forestall deflation and acquire precious metals, the American colonies sporadically engaged in competitive devaluation by raising the nominal values of the specie coins circulating there. Queen Anne attempted to halt the practice by issuing a proclamation in 1704 that prohibited colonies from rating Spanish dollars at any rate above 6 s. Because Spanish dollars were officially rated in Great Britain at 4 s. 6 d. sterling, the par of exchange associated with rating dollars at 6 shillings was £133.33 proclamation = £100 sterling. In the eyes of imperial authorities, “proclamation money” meant, by definition, money as defined by this rule.

The proclamation and a subsequent act of Parliament designed to enforce it were, for the most part, ignored and openly defied in the colonies. Pennsylvania, unlike other colonies, made an effort to revalue its currency to comply with Queen Anne’s proclamation, and Pennsylvania’s first paper money, issued in 1723, was ostensibly designed to circulate at that value (Brock 1975, 158–162). Although Pennsylvania persisted in calling its paper money “proclamation money,” the pretense quickly wore thin. In 1755 Robert Hunter Morris, Pennsylvania’s governor, confronted the Pennsylvania Assembly declaring “it is known to you, and every One, that Spanish Pieces of Eight [dollars] do now, and for a Number of Years have passed, and been current, at Seven Shillings and Six-pence, when that Act requires, that they should pass for Six Shillings only…from whence it appears, that tho’ you call your Paper Bills Money according to Queen ANNE’S Proclamation,”

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6. The imperial definition of “proclamation money” used here is consistent with previous scholarship but subtly different from the one used by Grubb (Davis 1900, 1:39–40; Nettels 1934, 242, 246; Brock 1975, 130–167; Ernst 1973, 23; McCusker 1978, 126; Hart 2005, 269). Grubb (2016c, 156 Table 6 notes) equates 1£NJ to $3.4235, which would be at the rate of 5.842 shillings per dollar, instead of 6 shillings, the value set out explicitly in the text of Queen Anne’s proclamation. Elsewhere, he appears to adopt the conventional value: Grubb (2015, 18 Figure 1) states that six shillings was the rate in New Jersey money for 0.875 ounces of silver, the weight of a full-bodied Spanish dollar. He concludes, however, that the par exchange rate implied by this valuation is 1.3275 to 1, whereas all previous scholarship and the understanding of the colonists was that the par implied by this valuation was 1½ to 1. It appears to this author that Grubb arrived at the latter conclusion by comparing silver weights without properly adjusting for the difference in purity of Spanish silver and sterling. The novelty of his redefinition is jarring, but the difference is too small to be important.

7. “Ostensibly” because Pennsylvania had overrated gold relative to silver and because Pennsylvania’s paper money initially passed at a discount with respect to gold as well as silver (MacKinney 1931–1935, II:1490, III:1830–1831; McCusker 1978, 176; A Dialogue 1725, 1; Brock 1975, 161 n.67).
it is really not so, but Twenty-five per Cent worse” (MacKinney 1931–1935, 5:1345–1346).

Pennsylvania issued its paper money, styled as proclamation money, in 1723, and a year later New Jersey, which had mostly retired its previous bills of credit, resorted to the printing press once again, creating its first land bank. New Jersey issued this money and all its subsequent paper money as Pennsylvania did, that is, ostensibly in compliance with the imperial standard. This accounts for the inscription on the bills to which Grubb assigns great significance; namely, that the bills should “pass current” (that is, be paid and received) at a rate such that only 6 s. in bills would be required to purchase a Spanish dollar (Newman 2008, 249–258). As Pennsylvania’s proclamation money increasingly diverged from the imperial standard, however, the everyday meaning of the phrase “proclamation money” in New Jersey followed the de facto standard established by Pennsylvania, not the imperial standard. By 1750, New Jersey’s citizens used the phrase “proclamation money” to signify dollars at 7 s. 6 d., the rate at which Pennsylvania and New Jersey’s bills of credit passed in ordinary transactions. Even Grubb (2015, 26 n.7) acknowledges that by the early 1750s 7 s. 6 d. was the “customary current value” of Jersey bills.

8. In 1764 this sentiment was echoed by Ben Franklin. Speaking of Pennsylvania’s bills of credit, he said: “At present every Bill that I receive tells me a Lie, and would cheat me too if I was not too well acquainted with it. Thirty Shillings in our Bills, according to the Account they give of themselves should be worth five Dollars; and we find them worth but four” (Franklin 1967, 13).
9. The “pass current” language in the inscription was used for all bills issued after 1724. The bills issued in 1724 bore a slightly different inscription, one that stated they would be accepted “by the respective Treasurers, and the Commissioners of the several Loan-offices in the Province, in all public Payments, and for any [Fund] at any time in the Said Treasuries and Loan Offices.” (Newman 2008, 249–258). This wording is more congruent with Grubb’s hypothesis, but the 1724 bills were widely counterfeited and were exchanged in 1728 for new bills bearing the “pass current” inscription, omitting any reference to public payments.
10. This language can be found in various accounts of the Board of Proprietors of the Eastern Division, where dollars are received and paid at the rate of 7 s. 6 d. proclamation money (Lurie and Walroth 1985, III:188, 197, 273). In 1758, Governor Bernard, then Royal Governor of New Jersey, wrote the Board of Trade that pieces of eight [dollars] “cost 7 s. 6 d. proc.” (Bernard to the Board of Trade, 31 August 1758, CO 5/977, f. 185v). In 1760, New Jersey received a portion of a parliamentary donation in dollars, and the treasurer credited those dollars at 7 s. 6 d. each in proclamation money in the accounts he presented to the assembly (New Jersey Assembly 1760, 22). This valuation remained in place at the beginning of the Revolution. In 1775 New Jersey’s Revolutionary government authorized its own paper money, not printed until 1776, the face bearing sums proclaimed to be “proclamation money,” but lacking the traditional statement about its purported equivalence to so many pennyweight and grains of silver. A resolution passed in 1776 establishes the government’s intent, the resolution, adopted unanimously, declaring “that all payments and receipts be made in every part of this state in proclamation money of New-Jersey, at seven shillings and six-pence the dollar.” (Newman 2008, 259; New Jersey Convention 1776, 107). Although briefly interrupted by the Revolution, the practice of basing New Jersey pounds, shilling, and pence on dollars at 7 s. 6 d. reappeared after the cataclysmic wartime inflation (New-Jersey Almanack 1787).
That New Jersey’s monetary system mirrored Pennsylvania’s is unsurprising because they were really the same monetary system. With rare exceptions, New Jersey’s proclamation bills of credit circulated in Pennsylvania interchangeably with Pennsylvania’s own bills of credit from the moment they were issued (NJ Archives, 5:154–155, 417; American Weekly Mercury, 6 April 1727). Typically, New Jersey bills were paid and received in Pennsylvania on the same terms as Pennsylvania bills, and Pennsylvania bills were paid and received in New Jersey on the same terms as New Jersey bills.11 It was because New Jersey bills were usually accepted on equal terms with Pennsylvania bills in Philadelphia that “the West Jersey exchange,” as Governor Shirley testified, “is constantly regulated by that of Philadelphia” (NJ Archives, 7:398). Indeed, in 1764, when Parliament demanded that each colony report on its currency, including annual prices of bills of exchange since 1749, New Jersey reported “Rates of Exchange at Philadelphia” (CO 323/19 fol. 38r.).12 Hence, there were two inconsistent definitions of “proclamation money” in colonial New Jersey: the imperial definition that said 6 s. in local money should purchase a dollar, and a local definition that treated proclamation money as money according to the common Pennsylvania/Jersey standard, by which (after 1750) 7 s. 6 d. would purchase a dollar.

The chief disagreement between Grubb and myself concerning his calculation of MEV arises from the two inconsistent meanings of the phrase “proclamation money.” New Jersey’s bills of credit bore inscriptions that clearly indicated the bills were supposed to pass current at the imperial standard, but Grubb’s predecessors, doubtless aware that the bills were not commonly paid and received according to the inscription on the bills, treated the inscription as nothing more than a statement of the colony’s aspirations. Grubb (2015, 140), however, asserts the inscription established the bills’ “specie equivalence for tax purposes,” anchoring their value

11. A published example of Jersey bills being accepted at face value in ordinary commerce within Pennsylvania can be found in Michener and Wright (2006a, 36). Explicit instances of Pennsylvania bills passing in New Jersey can be found in Stephens (1968) and in Aaron Leaming’s (1750–1777) diary. Leaming was a prosperous farmer in Cape May, New Jersey.
12. Occasional exceptions to this rule occurred during Jersey’s land bank years. Payments to the New Jersey land bank had to be made in Jersey bills; therefore, for about six weeks around the time that payments fell due possessors of Jersey bills demanded a small premium for them (Morris 1993, 272–273; JBOT, 8:439). James Alexander suggested to a friend that Jersey bills would be accepted at a higher value “for six weeks after the 25th of March” (Colden Papers, III:100). The significance of the 25th of March is that the loan office received payments and made loans each spring, and annual payments officially fell due on 25 March (NJ Laws, 2:307, 431, 478). Douglass says that in 1738 New Jersey bills were at a 5 percent premium over Pennsylvania bills (Douglass 1740, 16). Once the New Jersey land bank closed in the early 1750s the only payments received by the New Jersey treasurers were tax payments. Taxes could be paid in money as it passed in West Jersey, which meant taxes could be paid in Pennsylvania money, and there was no longer a special demand for Jersey bills. Thereafter Jersey bills seem to have been always treated as equivalent to Pennsylvania’s bills of credit in both Pennsylvania and New Jersey.
at redemption. The inscription, he insists, proves that the New Jersey treasurers accepted them according to their inscription, effectively redeeming them at an enhanced value. Because the Jersey treasurers accepted them at a premium, Grubb reasons, New Jersey’s bills were not a fiat currency, as Federal Reserve notes are. The distinction, Grubb explains, is that “If you take a Federal Reserve note to the Fed and ask for its real non-money value, they will just give you another Federal Reserve note of equal denomination” (Grubb 2020, 74 n.3).

New Jersey’s treasurers, however, did not honor the inscription on the bills. The only evidence Grubb cites to back his claim is the language of the inscription itself, which echoes the lip service paid to proclamation money in the acts emitting it. In 1767 Governor Bernard testified that New Jersey’s treasurers provided bill holders no more satisfaction than the modern Fed does.

When I first came to America as Governor of New Jersey, I was placed amongst paper money the most creditable of any upon the continent, never depreciated, & accompanied with a sufficient quantity of silver specie for external trade. And yet thro’ all this fair face the ill Consequences of these emissions were very apparent at no great distance. In the first place there was no obligation for redemption at a certain time, notwithstanding instructions to Governors & clauses of bills in pursuance thereof. For before any set of bills were redeemable another Set of bills were emitted with the usual clause of their being a general tender. They therefore were a tender at the treasury for the redemption of expired bills. In vain therefore did the possessor of bills read upon the face of them that he was entitled to a certain quantity of silver; When he applied for it, he might be told that by subsequent laws the silver was turned into paper; and that he would have no right to insist upon being paid in silver, whilst Any Emission of Bills remained unexpired. (Bernard 2013, 388–389)

Neither were New Jersey bills of credit treated as imperial proclamation money when tendered for taxes, despite the inscription printed on them. Indeed, in laying taxes on the populace, New Jersey tax laws adopted after 1750 specified that taxes were to be paid “in Money as it now passes in the Western Division of this colony,” (NJ Laws, 3:219, 4:13) or (in 1772 and thereafter) in “Proclamation money of this colony” (NJ Laws, 5:118, 175, 304). The legislature adopted the most striking and sweeping of these acts in 1760, an act instructing the Treasurer “to receive all and every of the Taxes, hereafter to be raised and levied by virtue of all or any of the Laws of this colony, for levying the respective sinking Funds, in Money as it now passes, or as it may pass, in the Western Division of this Colony at the time of paying all or any of the said Taxes into the Treasury; any Law, Usage or Custom to the Contrary” (NJ laws, 4:13). Douglass (1972/1752, 2:285), described how money passed in New Jersey’s Western Division circa 1752:
“At present the West Jersey Money or Currency is in Value equal to that of the adjoining Province of Pennsylvania; the East-Jersey Currency is the same with the adjacent Province of New York; their respective Dealings being almost solely with the respective adjoining Provinces.” In Pennsylvania and West Jersey, Douglass wrote, the “Spanish Dollar passes for 7 s. 6 d. Currency,” whereas in New York a dollar is “8 s. Currency.” In plain English, the law required New Jersey treasurers to treat each Spanish dollar as 7 shillings and 6 pence, not as 6 shillings, notwithstanding “any Law, Usage or Custom to the Contrary.”

A glimpse of the monies received in New Jersey taxes can be had in a letter written by the Eastern division treasurer in 1775. “A considerable part of the last year’s taxes,” he wrote, “[were] paid into the treasury in gold, silver, Pensilvania and Maryland money, which I lodg’d in the hands of different people, in whom I think I can confide, in order to gett chang’d into Jersey money” (Stevens 1968, 11 August 1775, reel 6). This constitutes additional evidence that the New Jersey treasury did not accept Jersey bills at a premium, because it is inconceivable that taxpayers would be so foolish as to pay a “considerable part” of their taxes in cash that did not receive the premium.

The most striking confirmation of how Jersey’s bills were valued at the treasury can be found in a controversy that developed in the late colonial period in New York. New Jersey bills circulated as a medium of exchange not only in Pennsylvania, but also in New York City, where “Jersey money passes…as current as N.Y.” (McCusker 1978, 159 n.102; Michener and Wright 2006a, 27–29). By 1750, the city’s merchants accepted each pound of Jersey’s (local) proclamation money for 1.08333 pounds of New York money, an arithmetically convenient advance corresponding to a premium of exactly 1 d. per shilling. The 8½ percent premium struck some New York merchants as excessive. New York’s monetary system was based on rating dollars at 8 s. each, whereas New Jersey’s local proclamation money was based on rating dollars at 7 s. 6 d. Basing the premium on the dollar par, which was common commercial practice, suggested the “correct” premium for New Jersey proclamation money in New York was 6½ percent, and several members of New York’s Chamber of Commerce wanted to see the premium reduced to that figure. New York merchant William McAdam broached the matter to the Chamber of Commerce: “[W]as it for the interest of the Community,” he asked, “that Jersey paper Money should pass in this Province

13. For further confirmation, see Pargellis 1969, 42.
14. Numerous conversions of New Jersey proclamation money to New York money at this rate of conversion, beginning in the late 1740s, can be found in Lurie and Walroth (1985). Moreover, tables showing the value in New York currency of various sums in New Jersey currency, calculated according to this rule, appeared in each edition of Gaine’s New-York Pocket Almanack published before 1774. For a specific example, see Hutchins 1772, 36–37.
higher than it is taken for in the Treasury of the Province of New Jersey[?]” He then introduced a motion that when members of the Chamber of Commerce “shall pay or receive any Jersey money they shall accept it on the same terms that it passes for in the Jersey Treasury, that is to say—A Bill of £6 Proclamation money for 16 Dollars...[etc.]” Because £6 = 120s., and 120/16 = 7.5, the New York Chamber of Commerce testified that New Jersey proclamation money was taken at the New Jersey treasury at the rate of 7 s. 6 d. per dollar (Stevens 1867, 151–152).

Although the New York Chamber of Commerce initially accepted McAdam’s proposal, it divided the Chamber, many members resigning over the measure, the heat of the controversy testifying to how important New Jersey bills were as a medium of exchange in the city. Eventually so many members resigned that the Chamber relented. Proponents of the change then induced the New York assembly to pass an act for the same purpose (Michener and Wright 2006a, 13–14 n.3). The act, passed in 1774, began by stating that bills of credit issued in the neighboring colonies, for reasons of convenience, passed in New York as money, and, to the detriment of commerce, they often passed at a higher value than they were issued for by the issuing colony. It then decreed that “after the first Day of May next, No Person shall either pass, exchange, pay or receive any Bill of Credit of any of the Neighbouring Colonies for any Sum, or at any Rate more than the Sum payable therefore at the Treasury of that Colony in which the same was issued” (NY Laws, vol. 5, ch. 1654, 638–639). Although the act makes no specific mention of New Jersey, everyone understood that the act targeted Jersey bills. In Gaine’s New-York Pocket Almanack for 1775, the ever-present table showing the value of New Jersey bills in New York is updated “agreeable to a Law of this Colony.” The conversion used in the table applies a 6 2/3 percent premium when converting New Jersey money to York money, again implying that Jersey bills were accepted at the Jersey treasury at 7 s. 6 d. (Hutchins 1774, 50).

That New Jersey’s treasurers accepted New Jersey bills at a premium was one cornerstone Grubb needed to bring MEV into alignment with his conceptually flawed APV, but by itself the supposed premium did not reduce MEV by enough to accomplish that goal. Dollars passed in New Jersey at 7 shillings and 6 pence. If the Jersey treasurers accepted them at 6 shillings each according to their inscription, MEV would have been 80, but even an MEV of 80 is inconsistent with Grubb’s

15. Other evidence confirms that New Jersey’s treasurers used local proclamation money, not the imperial variant, in their accounts. When New Jersey’s treasurer received a parliamentary donation partly in dollars, those dollars were recorded in the accounts at 7 s. 6 d. in proclamation money (New Jersey Assembly 1760, 22). Also, an account book of the Treasurer of the Western Division has survived, and a random slip of paper folded into the book records the details of how much was received from each of several towns in Gloucester County. An entry for “Gloster Township” mentions a dollar received at seven shillings sixpence (Smith 1762–1774).
computed APV. To align them, MEV had to be reduced even more. Instead of using the value at which dollars passed in New Jersey in calculating MEV, Grubb instead estimated the value of silver in New Jersey by using exchange rate data to infer what it would have cost to import silver from London. Inferring the price of silver in New Jersey from exchange rate data makes little sense because after 1750 Spanish silver dollars circulated in New Jersey at 7 shillings and 6 pence, which tells us the value of silver in New Jersey. Inferring the value of silver from the exchange rate data is treacherous both because exchange rate data are exceedingly sparse and because when exchange rates are between the specie import and export points there is no precise arbitrage condition that can be used to infer the price of silver.

Grubb’s approach, however, permits him to whittle MEV down from 80 to a post-1750 average of about 74. When I made this point in Michener (2019a, 193), Grubb (2020, 73) accused me of inconsistency on the grounds that I argue elsewhere in my comment that exchange rate data must sometimes be adjusted, and that Grubb and I use very similar estimates of the cost of transporting specie. Grubb’s objections obscure my fundamental point. If one uses the cost of transporting specie and an implicit arbitrage condition to infer the value of specie in New Jersey from the exchange rate, is it more sensible to add or subtract the cost of transporting specie? Grubb’s approach only makes sense if New Jersey exchange rates on London were pegged at the specie import point, whereas it is notorious that specie more commonly flowed from New Jersey to London. Grubb estimates the price of silver by adding the cost of transporting specie from London to the exchange rate, whereas the direction of specie flows suggests that subtracting the cost of transporting specie to London from the exchange rate would better approximate the historical conditions. Adding rather than subtracting silver transportation costs is how Grubb reduces MEV.

Grubb (2020, 73) concludes his defense of MEV and APV by objecting that in Michener (2019a) I did “no recalculation [of MEV and APV] to show what [I] think the values should be or show whether such changes would alter the coincidence of APV and MEV levels.” I do so now. New Jersey’s treasurers accepted bills of credit in lieu of silver at precisely the same rate at which they passed in ordinary transactions, which means that MEV was 100. By contrast, the value the bills possessed as a discount security, which is what APV purports to measure, was zero under perfect foresight. Even had the Revolution been entirely unforeseen, the value Jersey bills possessed purely as discount bonds could never have exceeded 30 percent of their face value. This demolishes the coincidence of the bills’ present value considered as discount bonds and their MEV, whose alignment Grubb (2016a) purports to display in figures 2 and 3, an alignment Grubb (2020, 76) declares to be “the core of [his] paper.”

The fundamental question that Grubb (2016a) seeks to answer is whether
colonial bills of credit were being held chiefly as bonds or chiefly as a medium of exchange. He purports to find that their real asset present value accounted for over 80 percent of their value, leaving a residual of less than 20 percent remaining to be explained by their value as money (Grubb 2016a, 1231). Hence, Grubb concludes that the “moneyness” of colonial bills of credit was slight, and that they are best considered to be discount bonds. I believe Grubb’s asset present value seriously overestimates the value of bills of credit considered as discount bonds. But comparing MEV to APV, however measured, does not seem to me to be a sensible approach to the underlying issue. There are many modern hybrids of money and bonds, such as money market mutual funds and money market deposit accounts, where the same question has arisen: How should we measure the degree of “moneyness” of these hybrid assets? The most theoretically elegant answer to this question is found in the theory of Divisia monetary aggregates, where the degree of “moneyness” of a particular hybrid asset is determined by the interest foregone in holding the hybrid asset relative to the interest rate offered by bonds (Barnett, Offenbacher, and Spindt 1984). The foregone return from holding bills of credit in Pennsylvania, New York, and New Jersey was practically indistinguishable from the foregone return from holding specie. With a few inconsiderable exceptions, these bills of credit bore no interest, nor did they appreciate vis-à-vis specie, the universal money of the era. Judging by this standard, the answer is clear that colonial bills of credit were money, and this is true however one computes their real asset present value.

**Grubb’s “corrections” to New Jersey’s exchange rates**

In Michener (2019a) I raised a series of objections to Grubb’s econometrics. I pointed out the spurious correlation that links APV to the per-capita money supply; I pointed out that Grubb includes deterministic time trends in many of his regressions, only to omit them from his key regressions, regressions whose results are entirely undone if they are included; I pointed out that a sizable fraction of Grubb’s observations used interpolated values of the dependent variable and explored the various ill consequences of using these interpolated observations. Grubb (2020) stridently objects to some of these criticisms, but his objections are misguided, as would be obvious to any competent econometrician who took the trouble to consider the matter. As a discipline, we are blessed with many competent econometricians, so there is no need to belabor these points. Economics, however, is not similarly well endowed with colonial historians. Therefore I see a need for
some additional comments pertaining to another key issue, namely, Grubb’s “corrections” to New Jersey’s exchange rates, “corrections” disputed by Michener (2019a, 198–205). The “correction” Grubb (2016a) made to John McCusker’s 1741 exchange rate played a crucial role in his empirical results. Grubb (2020, 79–87) defends his corrections, attempting to reframe the issue in the title of his rejoinder to paint me as a champion of uncorrected data. The defense in Grubb (2020), to which I now turn, rests on ad hominem attacks, false accusations, and twisted quotations.

The 1739 exchange rate

Because Lewis Morris (1852, 49) indicated in a letter written on 26 May 1739 that his salary of £1000 New Jersey money was worth only about £550 sterling, Grubb replaced McCusker’s May 1739 exchange rate—170—with 181.8, derived from the ratio of 1000 to 550. Grubb (2020, 87) writes: “Michener is in effect repudiating basic mathematical theory…[by claiming] long division no longer holds (so that 1000 divided by 550 now equals 1.70).” This would make sense as a closing punch but for the fact that nowhere in Michener (2019a) did I affirm McCusker’s 1739 rate or dispute Grubb’s correction to it. My challenges were limited to Grubb’s corrections to the 1741 and 1762 rates, as elaborated in a section titled “New Jersey’s exchange rates in 1741 and 1762” (Michener 2019a, 198–208). It could hardly have been clearer. For better or worse all the regressions presented in Michener (2019a) use Grubb’s 1739 exchange rate instead of McCusker’s.

But I must say “for better or for worse” because there are good reasons to dismiss the citation Grubb relied upon, as perhaps McCusker dismissed it. In the same letter, Morris (1852, 48) says the chief justice’s salary is “not 90£ sterling per ann[um]” and the second justice’s salary is “about £24 sterling per ann[um].” We know the chief justice’s salary was £150 New Jersey money and the second justice’s salary was £40 New Jersey money (NJ Laws, 2:499). The ratios of 150/90 and 40/24 are both 1.67, considerably below 1.818 and close to McCusker’s 1.70. The conversion ratios in Morris’s 1739 letter are inconsistent with one another. Furthermore, in a report prepared in December 1739 the Assembly declared that New Jersey’s exchange had been, until 1714, “at £50 per Cent advance and that from the Year 1714 the Exchange had gradually risen to £65, and within four months last part had risen to £70” (NJ Archives, 15:107). These are numbers consistent with the conversion ratio of the justices’ salaries, not the conversion ratio 16

16. Grubb (2020, 79) says that “The same error shows up in Richard Lester’s book (1970/1939, 127). McCusker may have just copied Lester’s long division error here for his entry for May 1739.” Lester wrote only that New Jersey exchange in 1739 was 170, citing this very report in the New Jersey Archives. Lester never made the long division error Grubb attributes to him. Lester’s book is not even among McCusker’s sources.
Grubb cites. Grubb is quick to accuse McCusker of arithmetic and transcription errors, but it is more likely that Morris made an arithmetic error in computing the sterling equivalent of his salary or that whoever transcribed Morris’s handwritten letter copied the sterling value of Morris’s salary incorrectly. In Michener (2019a) I held my peace because the issue was immaterial and because I could foresee Grubb’s vitriol. I did not anticipate being attacked for something I did not mention!

The 1762 exchange rate

Grubb (2020, 85–86) never addresses my extensive discussion of the 1762 exchange rate (Michener 2019a, 205–208), instead unjustly denigrating McCusker’s scholarship. According to Grubb, McCusker relies on two published sources, Donald Kemmerer (1956, 131) and Joseph Sherwood (1851, 137), both of whom cite two exchange rates for 1762: 175 and 177.5. Kemmerer, Grubb writes, attributes the two rates to March 1762, whereas Sherwood did not reference a month. McCusker averaged the two rates and reported their average for March 1762. “Then,” Grubb continues, “McCusker appears to have taken Sherwood’s second rate [177.5], considered it an independent observation from that reported in Kemmerer, and arbitrarily placed that rate in September of 1762. As such, McCusker appears to be double counting the [£177.5 New Jersey = £100 sterling] rate, counting this single observation in both March and September of 1762.” Grubb explains that he removed McCusker’s September 1762 exchange rate because it was “an erroneous double counting.”

Grubb neglects to mention that in addition to the two published sources McCusker also provided two unpublished sources. One of them was an “Account dated 9 Sept. 1762” in the Alexander papers at the New-York Historical Society, and this account, reproduced in Michener (2019a, 204–205), is unambiguously the source of the September exchange rate. The other, the “State of the Bills of credit in New Jersey,” CO 323/19, fol. 38r., is a document in the British public record office prepared by New Jersey in response to a Parliamentary inquiry. It is the ur-source of the information found in both Kemmerer and Sherwood. A careful reading of Sherwood (1851, 137), especially in conjunction with the original “State of the Bills of credit,” reveals that Sherwood did reference a month, and that month was March 1762. McCusker is innocent of the mistakes Grubb attributes to him.17

17. McCusker is famous for his devotion to careful archival work, and it was because of that reputation that McCusker was selected to oversee the compilation of colonial data for the Millennial Edition of Historical Statistics of the United States.
The 1741 exchange rate

The 1741 exchange rate is indisputably the most important to Grubb’s results and to my critique. Grubb changes the 1741 value to a number that is a six standard-deviation outlier in his own regression (Michener 2019a, 200). As confusing as much of the debate between Grubb and me may be to outsiders, Grubb and I agree that the pivotal issue respecting the 1741 exchange rate is how to interpret statements such as “exchange is at 25 per cent,” because this is how exchange rates are presented in Morris’s key 1741 letter. McCusker and I contend this means exchange was at 25 per hundred over par, or that it took 125 pounds in New Jersey currency to purchase a bill of exchange for 100 pounds sterling. Grubb contends that “exchange is at 25 per cent” means New Jersey currency was worth only 25 percent of sterling, so that it took 400 pounds in New Jersey currency to purchase a bill of exchange for 100 pounds sterling. In Michener (2019a, 203–204 and particularly 204 n.21) I presented detailed evidence supporting my view, most of which Grubb (2020, 79–84) ignores.

Instead, Grubb (2020, 80) doubles down in his effort to besmirch McCusker. Grubb begins by asserting that McCusker made a transcription error in reporting the average exchange rate for 1741. But there was no transcription error. If Grubb had read McCusker (1978, 23–24) he would have discovered that McCusker gathered his data, averaged by quarters, and then computed an annual average by averaging the quarterly data, a detail I pointed out myself (Michener 2019a, 202 n.19). Grubb goes on to accuse McCusker of cribbing his January 1741 data from Kemmerer (1956, 119) while also copying it wrong! In making this accusation, Grubb ignores the fact that in Michener (2019a, 202) I verified McCusker’s January 1741 exchange rate, which may be found in NJ Archives (vol. 15, p. 187). Moreover, by accusing McCusker of having copied data from Lester (1970/1939, 127) and Kemmerer (1956, 120), Grubb inadvertently draws attention to the fact that both Lester and Kemmerer interpreted the 1741 exchange rate data in the same way that McCusker and I do.

The bulk of Grubb’s defense of his supposed correction of the 1741 exchange rate is a fanciful account of what Morris’s letter of 16 August 1741 says. His argument proceeds thus: New Jersey had printed £2000 for outfitting and transporting colonial troops on an expedition against the Spaniards, but foreign expeditions “were funded in specie.” Hence, Grubb continues, “New Jersey paper pounds had to be exchanged for specie,” which was “acutely scarce due to wartime demand.” The want of specie to fund the expedition, Grubb argues, caused an abrupt increase in the price of bills of exchange, an increase consistent with his corrected data.

That claim might sound plausible to someone unfamiliar with the history, but
it is simply untrue. Consider the timing. New Jersey emitted the £2000 in mid-1740, and New Jersey’s troops had already disembarked by October 1740, which makes this an unpromising explanation for an exchange rate anomaly that occurred in the summer of 1741 (NJ Archives, 6:100–103). Moreover, on 1 January 1741, the New Jersey Council related what happened to exchange rates in the aftermath of the emission: “At the time of emitting the said £2000, the Course of Exchange with Great Britain was 160 p. cent and is now fallen to 150 p. cent or thereabouts” (NJ Archives, 15:187).

In support of his analysis Grubb (2020, 83) quotes Morris (1852, 134) as saying that the unusual alteration in the exchange rate “is said to be chargeable to another account viz. the want of specie [specie scarcity caused by wartime demands]….” But the ellipsis and Grubb’s bracketed insertion of “specie scarcity caused by wartime demands” distort Morris’s meaning. Morris testified that all forms of money, both specie and paper money, were exceedingly scarce and that therefore it was difficult to convert bills of exchange into cash of any kind. This is what hides behind Grubb’s ellipsis:

…it is said to be chargeable to another account viz. the want of specie, & of a sufficient quantity of currency to supply [the] ordinary necessities of Jerse & Pensilvania at that time for much provision being to be shipp’d for the troops and Elsewhere in the West Indies, bills of Exch[ange] would not purchase this unless chang’d into specie or bills of credit, both of w[h]ich being scarce there was a necessity of Lowering bills in order to procure it… (Morris 1852, 134, my emphasis)

Morris discussed this dilemma in other letters as well. As Kemmerer noted, in August 1740 “Morris wrote Colonel Blakeney, adjutant-general of the King’s troops in America, that he could not sell the British government bills of exchange for either specie or New Jersey money and so could not give the officers pay for the troops raised in the colony” (Kemmerer 1956, 120; Morris 1852, 104, 107). Instead of an abundance of paper money and a shortage of specie raising exchange rates, as Grubb maintains, a shortage of all forms of money, both specie and paper, led exchange rates to fall. This is exactly the conclusion of Anne Bezanson, Robert D. Gray, and Miriam Hussey (1935, 324–325) that I offered in Michener (2019a, 201–202).

18. Moreover, that foreign expeditions “were funded in specie” is a specious claim, unsupported by any of Grubb’s evidence. In 1759 General Amherst, planning the British expedition that culminated in the fall of Montreal, found himself without funds. He persuaded the New York Assembly to print £150,000 in New York currency, which the Assembly lent Amherst to prosecute the expedition. That the British expedition that conquered Canada was funded with colonial bills of credit certainly demonstrates that military expeditions outfitted in the colonies did not require specie (NY Laws, ch. 1087, 4:350–355; Broadhead 1855, 7:399–400).
Grubb (2020) in fact uses an ellipsis and inserted text to distort Morris’s meaning twice in the same block quotation.19 Grubb quotes Morris thusly:

…it seems plain to me that if a guinea [an English gold coin] was at any time before that current at 30 shillings in bills of credit [New Jersey paper pounds], that, when it was current at 5 pounds in yᵉ same bills, it required 5 pounds to purchase that guinea which 30 shillings of the same currency or bills would have done before, w[h]ich must make those bills (whatever nominal value was Impress’d upon them) of so much less reall value than they were before. (Morris 1852, 134, as quoted in Grubb 2020, 83)

Grubb (2020, 83 n.8) points to this passage as “smoking-gun evidence that Kemmerer, McCusker, and Michener are wrong in their transformation of the exchange rate statements made by Morris,” and he berates us for ignoring it. Grubb calculates that if a guinea—21 shillings sterling—passes at 5 pounds in local currency, it implies the sterling exchange rate is about 476 local pounds per hundred pounds sterling, a number Grubb describes as Morris’s “initial ballpark estimate” of the New Jersey exchange rate in 1741. Grubb points out that this number is not far from the New Jersey exchange rate of 400 that he championed and that I dismissed as an implausible outlier (Michener 2019a, 202). But Grubb omitted the beginning of Morris’s original passage, which reads:

…it seems plain to me that if a guinea went at 5 pound, (w[h]ich I think it did in New England,) it would purchase just as much as a New England paper currency of five pounds would do, so that (according to our Council) there would be no alteration of the currency, 5 pounds being still being 5 pounds; but it seems plain to me that if a guinea was at any time before that current at 30 shillings in bills of credit, that, when it was current at 5 pounds in yᵉ same bills, it required 5 pounds to purchase that guinea which 30 shillings of the same currency or bills would have done before, w[h]ich must make those bills (whatever nominal value was Impress’d upon them) of so much less reall value than they were before. (Morris 1852, 134, my emphasis)

Morris was not describing New Jersey, but rather New England, and he was using New England as the poster child for bad monetary behavior.20 After the phrase “bills of credit” Grubb inserts “[New Jersey paper pounds]” to mislead Grubb’s reader into believing that Morris was describing New Jersey in a passage that does not pertain to New Jersey.

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19. Nor is this the first time Grubb has used an ellipsis and inserted text to distort meaning. See Michener and Wright 2006a, 13.
20. For what it is worth, McCusker (1978, 141) reports that in August 1741 the New England exchange rate was 550 pounds New England currency = 100 pounds sterling, so Morris’s anecdotal evidence and Grubb’s calculations do indeed provide an initial ballpark estimate for New England.
When he is not accusing Kemmerer, McCusker, or me of knavery or incompetence, Grubb (2020) continues to insist that “exchange is at 25 per cent” means that 400 pounds in local currency are required to purchase a bill for 100 pounds sterling. If Grubb believes this, then we need to ask (in the spirit of his rejoinder’s title) why Farley Grubb insists on using uncorrected data, because Grubb (2019b) applies his theory to colonial Virginia and uses McCusker’s exchange rates without making any “corrections.” If one consults the original sources that McCusker used to compile the Virginia exchange rates, we discover exactly the same issue—exchange rates such as “25 per cent” or “30 per cent” that McCusker (correctly, in my view) enters into his tables as 125 and 130 (Soltow 1965, 166–167; CO 323/19, fol. 51r.). If Grubb is sincere in his belief, why did he not “correct” the data he used in Grubb (2019b)? Why does Grubb alter McCusker’s data only when it is required to obtain the result he seeks?

**Conclusion**

I wish to make two points in closing.

The first point relates to a proper understanding of colonial history. That faithful retirement of colonial bills of credit was the *sine qua non* that guaranteed their value is a myth. Faithful retirement of bills of credit was neither necessary nor sufficient for bills of credit to maintain their value. Colonial New York’s record of fiscal probity was abysmal, yet New York’s colonial currency did not depreciate (Michener 2015, 12–16). In the aftermath of the Revolution, the state of Pennsylvania attempted to create another land bank on the colonial model. Even though Pennsylvania emitted comparatively modest quantities of bills of credit, promised their prompt retirement, and then fulfilled those promises, the bills of credit nevertheless depreciated (Michener and Wright 2005, 686–690). The extreme measures Grubb must use to twist history, economic theory, and data to make a case for the central importance of New Jersey’s colonial redemption policies is a damning indictment of the theory. The spurious support Grubb lends to the redemption theory diverts attention from what actually sustained the value of colonial currencies, namely, the concomitant circulation of gold and silver monies at rated values established and honored by local merchants (Michener 1987; 2011; Michener and Wright 2006c). By belittling the critical role played by specie in the colonial money supply, Grubb and other advocates of redemption theories divert attention from issues critical to the economic and political history of the period. The important role specie played in the colonial money supply is what links British trade restrictions in the aftermath of the French and Indian War to the fraught economic and monetary conditions in the colonies during these years, a link that
helps explain colonial discontent with the empire.

The second point concerns standards for economic discourse more generally. Cambridge Core, the publishers of *JEH*, have **publicly committed** to maintaining a conventional set of ethical standards, standards I believe Grubb has violated. Grubb (2020), however, implicitly endorses a different standard. Grubb (2020, 72) berates me for submitting referee reports and comments critical of his many articles, and he points with satisfaction to the success he has had in suppressing my comments, including Michener (2019a), a version of which was originally submitted to the *JEH*, which rejected it after an exceedingly cursory review. Grubb (2020, 77) also defends his right to stonewall me on the method he used to obtain his 1741 and 1762 exchange rates. “I refused,” Grubb (2020, 77) writes, “and said I was not Michener’s research assistant. All the sources were published and were in Michener’s research library. I saw no reason to have to explain to Michener how [to] use a library, how to read English, and how to use his hand calculator to do long division.” He defends stonewalling me despite the fact that, by his own testimony, his interpretation of 1741 exchange rates was inconsistent with Lester’s, Kemmerer’s, and McCusker’s interpretation, making it altogether mysterious to me, and despite the fact that his original article did not include a page citation for the Sherwood passage used for his 1762 exchange rate (Grubb 2020, 85 n.10). Grubb (2020, 77) goes so far as to express dismay that the editor of this journal pressed him to reveal his sources, and then shared that information with me. Grubb (2020) seems to imply that proper academic inquiry precludes the questioning of Grubb’s own work. Is that a standard that any editor should endorse?

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Recalculating Gravity: A Correction of Bergstrand’s 1985 Frictionless Case

Nico Stoeckmann

Jeffrey Bergstrand (1985) derives a theoretical gravity equation for bilateral international trade flows focusing on the role of prices. The gravity equation has served as the basis for thousands of theoretical and empirical works in the field of international economics.

Bergstrand develops a general equilibrium model of world trade, which is based on utility and profit maximization in $N$ countries endowed with one production factor each. He uses nested utility and production functions based on constant elasticity of substitution (CES) and constant elasticity of transformation (CET), respectively. The model generates $N^2$ partial equilibrium subsystems of 4 equations each with 4 endogenous variables and 3$N$ constraints. This system of $4N^2 + 3N$ equations results in the general gravity model if the small market assumption—the neglectable impact of the market between country $i$ and $j$ on other markets—and the assumptions of identical preferences and technologies across countries hold.

1. Paderborn University, 33098 Paderborn, Germany. I would like to express my special thanks to Jeffrey H. Bergstrand and Bernard M. Gilroy for their helpful comments and suggestions.
This equation is the same as equation (14) in Bergstrand’s original paper (Bergstrand 1985, 477), divided into VI parts. The value of bilateral trade is dependent on both countries’ gross domestic products $Y$, the gross transport factor $C$, the tariff rate $T$, the exchange rate $E$, the f.o.b. price of $i$’s good in $k$ $P_{ik}$, the c.i.f. price of $k$ in $j$ $P_{kj}$, and domestic prices and elasticities, where $\sigma(\mu)$ is the elasticity of substitution in consumption between imported goods (between imported and domestic goods) and $\gamma(\eta)$ is the elasticity of transformation in production between export markets (between foreign and domestic markets). This equation is often cited as Bergstrand’s generalized gravity model. It is used to define sufficient control variables for empirical trade analysis and to provide theoretical justification for gravity.

To get a frictionless gravity model that excludes all price terms, further assumptions have to be made. Assuming perfect substitutability, perfect commodity arbitrage, zero tariffs, zero transport costs and normalizing exchange rate to unity implies that $C_{ij} = T_{ij} = E_{ij} = 1$, $P_{ik} = P_{kj} = P_{ii} = P_{jj} = 1$ for all $i$, $j$, $k$ and $\sigma = \mu = \gamma = \eta = \infty$. Bergstrand’s simplification resulted in his equation (15), $PX_{ij} = \frac{1}{2}Y_i^{1/2}Y_j^{1/2}$—but recalculating the derivation, as shown in the following, actually leads to $PX_{ij} = Y_i^{1/2}Y_j^{1/2}$. The coefficient $1/2$ in Bergstrand’s equation (15) should have been omitted. This correction has been confirmed by Professor Bergstrand as part of the valuable comments he provided on this paper.
While the empirical implications of this correction may not be serious, there is a formal theoretical relevance. Besides presenting the correct solution of the
frictionless case, this note further contributes to the literature on gravity models in three ways.

Bergstrand (1985) introduces the theoretical section of his paper by stating that the gravity equation then in use, \( PX_{ij} = \beta_0 Y_i^{\beta_1} Y_j^{\beta_2} D_{ij}^{\beta_3} A_{ij}^{\beta_4} U_{ij} \), lacks a sufficient theoretical foundation. He then defines his goal to be the generation of a gravity equation as similar as possible to this representation. The first equation he derives is from the general equilibrium model. With further constraints, he achieves his goal of creating a frictionless case closer to the widely used equation. With the correction of the constant in this special case, we get even closer to his goal.

Not only the general equilibrium model is frequently used in empirical studies. Some authors also cite the frictionless case as a base for further theoretical studies (e.g., Földvári 2006, 48; Ramesh 2017, 149). These works also need to be corrected.

The range of theoretical derivations of gravity models has grown significantly over time, with the most cited models next to Bergstrand being Anderson 1979; Anderson and van Wincoop 2003; Deardorff 1998; and Eaton and Kortum 2002. Meta-papers (such as Head and Mayer 2014), which have set themselves the task of classifying and comparing gravity approaches, occasionally criticize a lack of comparability. Corrections such as the one presented here contribute to such comparability.

**References**


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Gender, Race and Ethnicity, and Inequality Research in the *American Economic Review* and the American Economic Association’s Conference Papers

Jeremy Horpedahl\(^1\) and Arnold Kling\(^2\)

Link to Abstract

In recent years the public discourse in the United States has put increasing emphasis on issues related to gender, race and ethnicity, and inequality. Have economists also increased their emphasis on these issues? Some economists have written in popular venues that economic research does not focus on these topics enough (Adams 2019; Francis and Opoku-Agyeman 2020; Steinbaum 2017).

In this paper, we provide evidence that economists have increased their focus on the areas of gender, race and ethnicity, and inequality. We examine two major outlets for economics research, publications in the *American Economic Review* (*AER*) and the conference papers of the American Economic Association annual meeting. We find that economists have placed additional emphasis on these three issues, whether taken as a whole or separately. These trends show up for both the *AER* published articles as well as the conference papers. For the period from 1991 to the present, the trend is clearest for published articles and conference papers addressing gender issues, but the trend is also evident for race/ethnicity and inequality when considering the totality of published articles and conference papers.

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papers. *AER* articles on these topics have increased from about 4 percent to 11 percent of the total published articles, and AEA conference papers have increased from under 10 percent to over 20 percent of the total papers included in the published papers and proceedings of the conference.

The 21st century has provided the economics profession with many new topics to consider. These include: outsourcing; supply chain complexity; the issues that the financial crisis of 2008 raised with respect to bank regulation, housing finance, and ‘shadow banking;’ the structure, conduct, and performance of Internet businesses and the policy issues that have arisen; the role of China in the world economy; the rise of artificial intelligence; the opportunities and challenges posed by ‘big data;’ the rise of intangible capital and the increased importance of intellectual property; and changes in U.S. economic geography, with some major cities generating an increased share of income.

With all of these new topics, as well as continued interest in perennial topics in monetary policy, fiscal policy, public finance, economic history, econometric methods, productivity and growth, economic development, and so on, for any topic to have gained market share, as gender, race, and inequality have, is striking. The opportunity cost of an increasing emphasis on these issues means that other areas are given lower priority.

Of course, the question of what is the appropriate level of attention that the profession should give to gender, race, and inequality is a matter of opinion. In this case, opinions are likely to depend on one’s position along the ideological spectrum. The increasing attention placed on these topics could reflect the left-leaning domination of academic economics in the United States (Langbert 2020; 2018; Langbert et al. 2016). But our goal is to provide anyone interested in this topic, regardless of their ideology, with a factual basis for discussion.

The American Economic Association (AEA) is the largest membership organization for academic and professional economists in the United States. Its flagship journal, the *American Economic Review*, is considered one of the ‘top 5’ journals in the field. The AEA’s annual meeting is the largest conference for academic economists in the United States. Papers and proceedings from the conference are published annually, which until 2018 had been published as an issue of the *American Economic Review* and are now published in a standalone journal. These papers are not peer-reviewed.

By studying the AEA conference proceedings and the *AER* journal, we are able to view two different sets of decision-makers in the publishing of economic research. The conference proceedings are chosen by the President-elect along with members of the Program Committee, which the President-elect chooses. The publications in the *AER* represent the outcome of the collaborative process of editor, co-editors, and referees of the journal.
In the Discussion section below, we treat recent discourse by economists in non-academic outlets such as *Newsweek*, the *Wall Street Journal*, *Boston Review*, and the Stigler Center (University of Chicago) publication *ProMarket* about the attention given to these issues in economics.

### Our method for coding articles

To find articles related to the issues of gender, race and ethnicity, and inequality, we started by performing an EconLit search for JEL codes that may contain articles on these topics. JEL codes included J15 (for race and ethnicity), J16 (for gender), J71 (sometimes covering gender or race/ethnicity), and a set of three JEL codes for inequality: J31, D31, and D63. After identifying all the papers tagged with relevant JEL codes, we read each paper to see if it the paper was correctly assigned for the JEL code, since some of these (especially on inequality) will produce false positives. Our results begin in 1991, the first full year that the *AER* began using the current JEL code system for editor-assigned JEL codes.\(^3\)

For papers on gender, in our search we were looking for papers that address issues where there are major differences between genders, where policy might have a disparate impact on one gender, or examining issues specific to one gender.

For papers on race and ethnicity, our approach is similar to papers on gender, but we excluded several categories of papers that appear under JEL code J15: papers specifically on discrimination against religious groups, castes, or immigrants. Immigration papers also addressing ethnicity were included. While these are all important topics, we view them as distinct from race and ethnicity issues. We do include papers on Native Americans as falling under race and ethnicity.

For papers on inequality, the JEL codes were so broad that we excluded over half of the papers. For inequality, we are interested in economic inequality of various kinds, including income, wealth, wage, and consumption inequality, as well as papers on factors that may be causes of these forms of inequality (e.g., skill-biased technological change). We also included papers that focused on individuals who earn high incomes, such as the top 1 percent of earners or CEOs. No single JEL code encompasses all of these issues, thus using several JEL codes and then narrowing down the papers was the most appropriate path. We did not include papers on other kinds of inequality, such as health or education inequality (these

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3. The JEL code system underwent a significant revision in 1988–1990 (Cherrier 2017) and began using this system for editor-assigned JEL codes in 1991. From 1990–2008, the *AER* had both editor-assigned and author-assigned JEL codes, which did not always match (Kosnik 2018), but this practice was discontinued in 2009. Because the editor-assigned JEL codes are searchable through EconLit on ProQuest, and these begin in 1991, we chose this date for the beginning of our search.
areas have their own JEL codes, but also some came up under the codes we searched for).

Many papers address multiple topics, and we used our judgment to determine whether gender, race and ethnicity, or inequality was an issue that was addressed in a particular paper. For example, just including a gender variable in a regression was not enough for us to count the paper as one focusing on gender issues. But if the gender outcome was emphasized in the abstract, introduction, or conclusion of the paper, we likely did include it in our count, even if the paper was not primarily about gender issues. The same goes from the other areas we examined. In other words, our default was to include an article or paper that was tagged with one of these JEL codes, unless by reading the article or paper it was clear that our topics of interest were not a major focus in the paper. We provide a list of all the articles and papers we counted in an online appendix (link).

We excluded comments, replies, rejoinders, corrigenda, and errata from our count of articles.

**Articles in the *American Economic Review***

Between 1991 and 2019,\(^4\) we identified 209 articles in the *American Economic Review* that addressed gender, race and ethnicity, or inequality. Some articles addressed two or all three areas, e.g., a paper on the gender wage gap addressed both gender and inequality. We excluded the AEA conference papers in these numbers, which until 2018 were published in the May issue of the *AER*; in 2018 a new standalone journal was established. The AEA conference papers are discussed in the next section. There were 73 articles that addressed gender issues, 56 articles that address race and ethnicity, and 103 articles that addressed inequality. These numbers add up to more than the 209 total articles, because some articles address more than one of the issues. These 209 articles represent 7.6 percent of the 2,747 articles published in the *AER* during this time frame, but there is considerable variation over time. Looking at the percent of articles published over time, we see that the number of articles on these topics has increased as a percent of all articles in the *AER*. This trend is visible in Figure 1, especially when using a five-year smoothing of papers.

\(^4\)We did not include any 2020 articles from the *AER*, since the full year was not available at the time of writing.
In the first five years we examined, 1991 to 1995, only about 4 percent of articles were on these topics. From 2015 to 2019 about 11 percent of articles were on these topics. And because the total number of articles published in the AER has increased over time, the total number of articles on these topics has increased even more substantially, from just 17 articles in the first five years of our study to 63 articles in the most recent full five years.

Figure 2 shows the numbers for each of the three areas. Articles addressing gender have increased from under 2 percent in the first five years to over 4 percent in recent years. Articles on race and ethnicity are the only area to show no clear upward trend over time. However, if we look at the absolute number of articles on race and ethnicity, they have doubled from an average of 6 articles between 1991 and 1995 to an average of 12 articles between 2015 and 2019. Articles on inequality increased the most, from under 2 percent in the early years to almost 6 percent in recent years, using a five-year average.

One possibility for the changes in the coverage of topics is due to changes in the editors of the AER. To a large extent, the choices of the editors can influence what articles are ultimately published in a journal. The editors for the AER over the period we examined were Orley Ashenfelter (1985–2001), Ben S. Bernanke (2001–2004), Robert A. Moffitt (2004–2010), Pinelopi K. Goldberg (2011–2016), and Esther Duflo (2017–present). We cannot with any certainty say how these editors may have influenced the selection of articles, but the trends we highlight have been present over the entire period we studied.

Another possibility to consider is that the AEA introduced four new journals in 2009, covering applied economics, economic policy, macroeconomics, and
microeconomics. It is difficult to say whether these new journals had any influence on the topics covered in the *AER*. However, there is no clear break in the trends around 2009. Of the overall increase from 4 percent to 11 percent in Figure 1, over half of it had taken place by the mid-2000s. The number of articles published in the *AER* continued to increase after 2009 as well, from about 90 per year in the years immediately preceding 2009, to over 110 per year in recent years.

**Figure 2.** *American Economic Review* articles addressing gender, race and ethnicity, or inequality (5-year average)

![Graph showing articles addressing gender, race and ethnicity or inequality from 1995 to 2019](image)

*AEA Papers and Proceedings*

The selected conference proceedings should not be seen as a representative sample of what was presented at the conference, since the papers selected for publication are chosen by the President-Elect of the AEA and the Program Committee. The papers presented are only a fraction of the total number presented. For example, at the 2020 conference there were 703 papers on the program, but only 114 papers (16.2 percent) were included in the *AEA Papers and Proceedings*. So the papers tell us about what the leadership of the AEA believed was important to highlight from the conference. Furthermore, it is important to note that these papers are typically chosen for inclusion in the proceedings by full sessions, rather than individual papers. Conference sessions are organized by topic.

Here is how the AEA’s website describes the selection of papers to be included in the *AEA Papers and Proceedings*:

the President-elect chooses, at the time of organizing the annual meeting, a number of sessions to be printed in the AEA Papers and Proceedings. Due to
space limitations, the number of included sessions is strictly limited. Usually, each member of the Program Committee invites a session of three papers that is included in the AEA Papers and Proceedings. By Executive Committee decision, the President-Elect chooses at least 12 additional submitted papers for the AEA Papers and Proceedings. Eighteen papers included in the AEA Papers and Proceedings are selected by AEA Committees. (American Economic Association 2020)

Figure 3 shows the share of papers on gender, race and ethnicity, and inequality in the Papers and Proceedings of the American Economic Association’s annual meetings. From 1991 to 1995, less than 10 percent of the selected conference papers addressed these areas, while in recent years it more than doubled to over 20 percent are on these issues. Most of that increase has occurred since 2012, with five of the last eight years being over 20 percent, and one of those years over 28 percent. There were 414 conference papers on gender, race and ethnicity, and inequality, or about 14.6 percent of the 2,846 conference papers published in the AEA journals from 1991 through 2020. There were 207 conference papers on gender, 135 on race and ethnicity, and 131 on inequality during the years we studied.

**Figure 3.** Conference papers published in the AEA Papers and Proceedings on gender, race and ethnicity, and inequality

Figure 4 shows separately the conference papers on gender, on race and ethnicity, and on inequality. Conference papers on gender increased dramatically, from under 5 percent at the beginning of our time period to 14 percent in the most recent five years. Conference papers addressing race and ethnicity did increase over the entire period from under 3 percent in the first five years to almost 6 percent in recent years, though with a lot of variation (the early 2000s were just as high as recent years). One notable difference from the published peer-reviewed AER
papers is that conference papers addressing inequality have not seen as dramatic an increase over the period we studied: the five-year count increased from about 4.4 percent to almost 6 percent, with a lot of variation throughout the period we studied.

**Figure 4.** Conference papers published in the AEA Papers and Proceedings on gender, race and ethnicity, and inequality

Because the total number of conference papers selected annually for publication has increased over time, it is also useful to look at the raw numbers. For race and ethnicity, the first five years we examined had just 11 papers, while the most recent five years had 34 conference papers. For papers addressing inequality, the number of conference papers increased from 18 in the first five years to 34 in the most recent five years. Papers addressing gender issues saw a dramatic increase from 19 to 81 conference papers, again comparing the first and last five years in our period of study.

The annual conference proceedings include a paper based on the AEA Distinguished Lecture, which is presented at the conference. The lecture was very recently renamed, having been named through the 2020 conference for one of the racist progressive founders of the AEA, Richard T. Ely (Thies and Daza 2011). We did not include the Ely/Distinguished Lectures in our count of the AEA’s conference papers, as it is distinct from a traditional conference paper, often summarizing or drawing on an individual’s body of past research rather than presenting new research. But several of these lectures did address our three sets of issues, including Welch (1999) on inequality, Goldin (2006) on gender, Card (2009) on inequality, Currie (2011) on inequality, Poterba (2014) addressing inequality, though it was not the main topic, and Bertrand (2020) on gender.
Discussion

Economists have clearly taken great interest in the topics of gender, race and ethnicity, and inequality. By the measures we have used in this paper, economists have in recent years increased their emphasis on these topics, especially gender and inequality. In recent years, one-tenth of the publications in the *American Economic Review* and one-fifth of the selected conference papers addressed gender, race and ethnicity, or inequality.

Our analysis provides some perspective on the claims that some economists have made about these issues being neglected by the economics profession. In the *Boston Review*, Marshall Steinbaum (2017) wrote about the reception of Thomas Piketty’s work on inequality and stated that there is a “deafening silence that greets [Piketty’s scholarship and interpretation], as well as inequality in general, in broad swathes of the field.” Furthermore, Steinbaum asserted that some leading economics departments do not have “a single publication that has any obvious or even secondary bearing on” Piketty’s work on inequality. Based on our research there have been 105 *AER* publications and 131 conference papers in the past three decades (and 84 publications and 106 conference papers through 2016, i.e., prior to Steinbaum’s essay) addressing various aspects of inequality. There was also a full session at the 2015 AEA conference devoted to *Capital in the Twenty-First Century* (Piketty 2014), and that session’s papers were published in that year’s conference proceedings.

In an article in *Newsweek* about racism in the economics profession, Dania Francis and Anna Gifty Opoku-Agyeman (2020) stated that “between 1990 and 2018, the five top journals in economics published 29 out of 7567 papers that explicitly addressed race/ethnicity,” or less than one-half of one percent. But in our research, we found that the *AER* alone had 56 articles from 1991 to 2018 that addressed race or ethnicity. What accounts for the difference? Francis and Opoku-Agyeman used data from J. Aislinn Bohren et al. (2019), who looked specifically at discrimination regarding race and ethnicity (and other forms of discrimination, including gender), which may account for the smaller number of papers that Bohren et al. found—they were not looking for papers on the economics of race in general. A recent article in the *Wall Street Journal* by the reporter Amara Omeokwe (2020) used the same data and quoted economists saying that it was difficult to get research published on racial issues, but Omeokwe clearly stated that the data only covered racial discrimination, unlike Francis and Opoku-Agyeman (2020).

On the issue of gender, in an article for the *ProMarket* publication of the Stigler Center at the University of Chicago, Renée Adams (2019) provides the
information that from 1975 to 2009, “29 papers were published on the topic of women on boards in the 50 journals on the Financial Times (FT 50) list... All but one of these papers were published outside the field of economics.” The statement may well be true, but the general thrust of her Stigler Center article, including its title and subtitle, is that economists are devoting scarcely any space to research on gender issues. As we have shown in this paper, that is false: since 1991, a large and increasing share of space in the AEA and published AEA conference papers has been devoted to gender issues. Note that one AEA session published in the 2016 Papers and Proceedings was devoted to “Gender in Corporation Management,” including three papers that directly addressed the issue of gender diversity on corporate boards, one of which was co-authored by Adams (Adams and Kirchmaier 2016).

Conclusion

The main findings of this study are that in recent years over 10 percent of papers in the American Economic Review and over 20 percent of papers in the published proceedings of the American Economic Association annual conference have been devoted to issues of gender, race and ethnicity, or inequality. These percentages have been increasing in recent years, especially for papers on gender issues.

Appendix

A list of all the articles and papers included in our analyses is provided in a downloadable appendix (link).

References


About the Authors

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Professional Scholarship from 1893 to 2020 on Adam Smith’s Views on School Funding: A Heterodox Examination

Scott Drylie

“Adam Smith is difficult to apprehend in his totality for the perverse reason that he is in parts so regrettably well known.” So wrote Alexander Gray (1931, 124–125) in his work The Development of Economic Doctrine. The statement might well apply to Smith’s position on who should fund the education of youth. It takes little effort to deduce that there exists an orthodoxy of sorts that Smith supported government-subsidized education through taxation. Scholars regularly assert that Smith “advocated for” (Baum 1992, 152), “pleaded for” (Reisman 1976, 225), “urged for” (Wilson 1989, 60), “insisted upon” (Fleischacker 2004, 235), or “demanded” (Skinner 1996, 192) such subsidization. If true, education ranks as one of the most significant exceptions to Smith’s presumption against government involvement in the production of goods and services outside of nightwatchman-state functions. Yet an important passage may suggest that the totality of Smith’s views on school funding is complex. Using this passage as a lens, the current study inspects the scholarship on this issue.

In previous work (notably Drylie 2020), I have offered an alternative reading of Smith. I propose that Smith is not decidedly favorable to taxpayer funding of education, that he certainly is not favorable to full funding, and that it is reasonable to read him as leaning against any taxpayer funding at all. I am, therefore, suspicious

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of common claims made about Smith’s position and curious as to how those claims have come about and persisted. My reading is based on the full “Article” entitled “Of the Expence of the Institutions for the Education of Youth,” located in Book 5, Chapter 1 of *The Wealth of Nations* (Smith 1976/1776, hereafter WN). It is also based on the passage in question, found in the “Conclusion of the Chapter,” which summarizes each of his positions from Book 5, Chapter 1.

Smith’s final sentence of his summary on education in “Conclusion of the Chapter” presents a grave problem to the orthodox characterization. The complete summary reads:

> The expence of the institutions for education and religious instruction, is likewise, no doubt, beneficial to the whole society, and may, therefore, without injustice, be defrayed by the general contribution of the whole society. This expence, however, might perhaps with equal propriety, and even with some advantage, be defrayed altogether by those who receive the immediate benefit of such education and instruction, or by the voluntary contribution of those who think they have occasion for either the one or the other. (WN, V.i.i.5)

Those are Smith’s final words on the subject. What should be clear is that Smith does not convey any certain or strong support for taxpayer-funded public education. Rather, Smith coolly acknowledges that state provision may be justifiable, and then immediately pivots away to express that it may be better to pursue solutions through voluntarily financed schools and the networks of charity schools in his time. There is no obvious final recommendation, and the passage resists being further reduced. Whether it conveys ambivalence, uncertainty, equivocation, or disinclination toward the state, it is inconvenient for any interpretation that associates Smith with public education.

The seeming incongruence between this passage and some familiar interpretations raises several questions. To what degree has this passage made its way into the scholarship? How have its complex sentiments been treated? How should we judge of the methods and results of the scholarship on the subject? In seeking answers to these questions, I aim to contribute to the tradition of scrutinizing scholarly methods, to bring to light disparate views, to spur further exploration of the passage, and to identify a future research agenda for a topic already frequently commented on. Smith is both a key figure in the history of thought and an authority figure for political persuasions. Smith’s position on an issue as profound as school funding has, then, widespread implications for discussions of the role of the state.
Analysis of patterns

To assess the presence and treatment of this passage in scholarship, I took the approach of creating as large of a sample as possible. Research was conducted from 2015 to 2020 through several university library consortiums and by way of online resources. The disciplines predominantly surveyed were the history of thought, economics, public policy, law, education, and history. Within Smithian literature, topics generally examined were education, justice, charity, beneficence, the division of labor, alienation, the theory of history, the role of the state, liberalism, egalitarianism, democracy, and French influence. A wide variety of search terms were employed to account for these themes, as well as to account for the varying language of ‘schooling’ and ‘education’ across time and place (e.g., public, popular, liberal, universal, elementary, primary, parish, charity, and compulsory). Phrase searches from Smith’s writing were also used within Google search engines. To overcome some of the quirks of Google’s algorithms, searches were conducted from different computers (i.e., IP addresses) and different geographical locations (i.e., servers).

The result of the search is a set of 191 unique English-language publications from 177 different authors, each specifically addressing Smith’s position on school funding. Of the 191, a subset of 54 from 52 different authors cited the key passage in some way. The interpretations range in scope from extensive to passing.

Several exclusions are important to note. First, I have tried to exclude all items for which one has to infer the author’s position based on the spirit of the text or on the author’s other writings. Second, I exclude reprints of the same article. Third, I exclude blogs and online-only discussions. I do, however, include items outside of formal publication sources in the few cases where I found a working paper, thesis, or dissertation which cited the passage. Lastly, commentary prior to 1893 has been excluded. That date is chosen as the publication year of the first editions of James Bonar’s *Philosophy and Political Economy in Some of Their Historical Relations* and Edward Cannan’s *History of Theories of Production and Distribution*—two important works signifying the start of the English-speaking world’s adoption of an academic form of Smithian scholarship (Tribe 2008). Comment on Smith’s views on education prior to the date of 1893 is infrequent and mostly from politicians, social leaders, philosophers, and clergy as opposed to what we might call professional academics (and is addressed in Drylie 2020).

The specific questions I first wish to answer will be done largely with quanti-
tative analysis: (1) How prominent are the various views of Smith’s position? (2) Does the passage in question occur randomly in the sample set, or does its occurrence predict the nature of the interpretation which the author wishes to convey? (3) Has the treatment of the passage changed over time? (4) Does a scholar’s academic discipline predict the treatment of the passage? (5) Can the treatment of the passage begin to provide an indication of the quality of the scholarship?

To these ends, I first organized the examples according to the nature of the interpretation of Smith’s position. The interpretations range from full taxpayer funding to none. However, owing to each interpretation’s unique tone, rhetoric and context, it was not possible to reliably place each along a precise continuum. They thus had to be binned into categories. I decided on two broad categories, which for simplicity I will call ‘state’ and ‘dissenting’ interpretations. The first contains variants that emphasize a concrete and substantial financial role for taxpayer funding; the second contains those which significantly diminish that role, push back against it, or outright reject it.

Two dimensions were looked at to confidently place each interpretation into the categories of ‘state’ and ‘dissenting’: the extent of taxpayer funding, and the nature of Smith’s commitment to government responsibility. For the state category, the extent of taxpayer funding ranges from full to moderate, and the nature of Smith’s commitment ranges from inspired to accepting. The state category includes three common interpretative patterns: (1) It includes those who believe Smith wholeheartedly insisted upon a comprehensive and extensive funding scheme resembling the variants of today’s universal public education. (2) The state category also includes most who identify his proposal as mixed-mode, relying on both taxes and user fees. Although mixed-mode can mean anything from nearly complete taxpayer support to almost none, those included here are interpretations in which Smith accepts taxpayer support of an amount at least equal to other funding (that is, notionally, 50 percent). (3) Finally, the state category includes nearly all of those who explain Smith’s acceptance of government as based on a market failure theory. Although ‘market failure’ is a precondition which could represent either a high or low standard, those included here have treated the precondition as an easily applied, obvious rationale rather than an unsettling decision of when, how much, and how long to intervene. In the end, what all those in the state category have in common is a portrayal of Smith as granting with

3. Keep in mind that Smith in The Theory of Moral Sentiments cautions against forcing beneficence: “Of all the duties of a law-giver, however, this, perhaps, is that which it requires the greatest delicacy and reserve to execute with propriety and judgment…to push it too far is destructive of all liberty, security and justice” (Smith 1976/1790, II.i.i.8).
moderate to significant conviction a moderate to significant role for government, and thus of setting out education as a major exception to his liberty principle.

The second category is ‘dissenting.’ The extent of taxpayer funding in these interpretations ranges from, as it were, 49 percent to none, and Smith’s commitment to government responsibility ranges from unsure to principally resistant. Like the state category, the dissenting category includes three common interpretative patterns: (1) It includes those who believe Smith is uncertain of any role for government. (2) The dissenting category also includes those who emphasize in Smith a different preference and recommendation than state funding—for instance, user fees or charity. (3) Finally, the dissenting category includes those who admit there is a possible role for taxes in a mixed-model concept—but only a minority one marked by a strong limit or principle. What they all convey to me is something distinctly otherly—a reluctance toward state involvement, or merely a fine tolerance for timely, cautious, and highly conditioned state involvement.4

I then assessed the data based on how each author ‘handled’ the passage. Authors took one of four approaches. They may have: (1) cited nothing of the passage nor made reference to it, (2) cited it in whole, (3) cited just the first part, which conveys support for taxpayer funding, or (4) cited just the second part, which contains the sentiment at odds with taxpayer funding. Table 1 organizes the results based on the nature of the interpretation and the handling of the passage. The publications contributing to each cell count are provided in Appendix A.

<table>
<thead>
<tr>
<th>TABLE 1. Nature of the interpretation vs. handling of the passage</th>
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<tr>
<td>State</td>
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<tr>
<td>Dissenting</td>
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<td>Total</td>
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A few simple calculations provide the initial findings. Seventy-nine percent (151/191) of publications present a state interpretation, and 21 percent (40/191) present a dissenting interpretation. There appears, then, to indeed be a dominant interpretation of Smith as an advocate of substantive taxpayer support. As for the prevalence of Smith’s final words on the matter, 71 percent (137/191) omit the passage entirely. Surprisingly, another 10 percent (20/191) elect to truncate the passage and omit those heterodox sentiments in the second part. Thus, of the 191 articles in the survey, just 19 percent cite the heterodox part which motivates the current study—and that figure likely is upwardly biased in my sample, the result

4. Reliable categorizing was of highest concern. In my deciding whether to categorize an item ‘state’ or ‘dissenting,’ I believe that only a few examples, which present mixed-modes or uncertainty, were subject to possible categorization error.
of a search technique specifically intent on finding instances of citations. In short, Smith’s final words, especially the compelling heterodox part, have been broadly excluded from scholarship.

Why has this passage been cited so infrequently? The joint frequencies from Table 1 reveal a pattern which suggests an answer. The table shows that 79 percent (151/191) hold a state view, of which only 23 percent (34/151) cite the passage in any way. Twenty-one percent (40/191) hold a dissenting view, of which 50 percent (20/40) cite the passage. The passage is rare among those holding a state view, and it figures prominently among those proposing a dissenting view. The odds ratio puts into perspective the imbalance. Those with a state interpretation are only 29 percent as likely to cite the passage as those with a dissenting interpretation. The strength of this pattern suggests that authors’ decisions to look to this passage for value is highly determined by the message they wish to convey. With the preponderance of scholars holding an overall state view of the article, this passage has not provided any marginal benefit to their case or its nuance has not been appealing.

How should one judge of this pattern? One could fault orthodoxy, claiming that due diligence requires citing such an important passage regardless of the message one wishes to convey. But it is also possible that the tensions of the article have been resolved through other textual means. And if the state interpretation is unimpeachable, perhaps some authorial discretion as to what to cite can be tolerated. Therefore, the strong association between a state interpretation and omission—though provocative—is not conclusive evidence of deficiency. How one should judge of this pattern in the orthodox publications will require qualitative assessment, which I will provide in the literature review. Similarly, how to judge of dissenting views’ extensive use of the passage will depend upon the case they make.

Upon closer inspection, however, there is a takeaway from this pattern which does reflect poorly on the scholarship, specifically on the body of state interpretations. Table 2 reframes the data, looking only at the 54 who cited the passage in any capacity. It relates each publication’s interpretation to whether it omitted or cited the heterodox part.

<table>
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<tr>
<th>TABLE 2. Treatment of the heterodox sentiment by those who acknowledge the passage</th>
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<td>Omitted</td>
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5. Contingency tables and odds ratios are frequently used in medical research. The odds ratio calculation is (34/117)/(20/20). The Pearson chi-squared test confirms that the decision to cite the passage is related to the choice to offer a state interpretation ($\chi^2 = 11.78, p < .0005$).
Of the 34 publications which put forward a state interpretation, 59 percent (20/34) truncated the passage, omitting the heterodox sentiments. This sort of omission cannot be viewed as acceptable authorial discretion. Nor can it be discounted as simple ignorance of the passage’s existence. To omit part of what is arguably an indivisible whole reveals either exceptionally inattentive reading or a willingness to suppress that which is ill-suited to the author’s intended message. The odds ratio shows that among scholars who saw some appeal in this passage, those with a state interpretation were only about 4 percent as likely as those with a dissenting interpretation to reveal its heterodox part. Or the inverse, those with a dissenting interpretation were 27 times more likely to show it.6 There is a profound asymmetry in method here, with the state interpretations avoiding complexity and the dissenting interpretations admitting of it. Among those who acknowledge the existence of the passage, the heterodox part has been treated either as a skeleton to be kept in the closet or as a skeleton key. The pattern raises the question—one which cannot be answered—as to how many of the other 117 with a state interpretation actively concealed this passage entirely to avoid having to address its complexity.7

A closer reading reveals another shortcoming of the orthodox literature. Of the 151 publications which offer a state view, I have found only one case which responds to a main dissenting argument—and even this case is evasive on the specific dissenting argument of school funding. The case is from Mark Blaug (1975, 572 n.12) who is appreciative of E. G. West’s body of work and grants him authority on many fine points on the history of education, referencing him 14 times. Nonetheless, Blaug claims Smith supported public education and, in doing so, he entirely avoids West’s assertion that Smith favored private funding. Blaug only confronts West on the more elementary argument that Smith insisted upon freedom of choice.8 Proponents of the state interpretation thus have almost universally ignored the dissenting interpretations—and here it is worth noting that they have also ignored the initial commentary, i.e., that from the 18th and 19th centuries, which was particularly drawn to Smith’s resistance to the idea of subsidization (Drylie 2020). This pattern of failing to address dissenting views foreshadows what a closer literature review might be able to confirm: that the orthodox interpretation has not won an argument but rather assumed the victory.

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6. The odds ratio calculation is (14/20)/(19/1). The Pearson chi-squared test confirms that the decision to suppress is highly likely to be related to the choice to offer a state interpretation ($\chi^2 = 15.35, p < .0001$).

7. ‘Turnabout is fair play’ has not been adopted by dissenting literature. The four who cited just the heterodox part paraphrased the state sentiments of the first part. There is no case in which the dissenting literature simply ignores evidence of state sentiments in Smith’s writing.

8. Others reference West as an authority of Smith’s views of education or take issue with other aspects of his work, but steer even further from his position on school funding (e.g., Leathers and Raines 2003, 55).
Have these patterns changed over time? I considered several possible historical demarcations to use for splitting the publications into eras, and chose the year 1976; as the bicentennial of the publication of *The Wealth of Nations*, 1976 coincides with expanded interest in Adam Smith (especially outside of economics) and roughly with a rediscovery of Smith’s other work, *The Theory of Moral Sentiments*, which has elsewhere tended to change the nature of Smithian discourse on the role of the state (Labio 2006; C. Smith 2014). Table 3 in Appendix B shows the contingency table using the 1976 demarcation. There does not appear to be a relationship between the nature of the interpretation and era. Dissenting interpretations constituted 18 percent of the sample before 1976, and 22 percent from 1976 onward, a statistically insignificant difference.9 Also, although there has been an uptick in the presence of the passage in the literature (as seen in Table 4 in Appendix B),10 there remains an unwillingness to fully reveal or engage with the heterodox sentiments.

I also explored whether the academic discipline of the author was related to the decision to cite or suppress the heterodox part of the passage. Fifty-four cited the passage in any capacity. Forty-two of those were economists or historians of economic thought, of whom 31 (74 percent) cited the heterodox part. Eleven were non-economists, of whom only two (18 percent) cited the heterodox part.11 Thus, once it was determined to mine this passage for meaning, the heterodox part has almost exclusively been brought forth into the scholarship by economists and historians of economic thought (31/33). The decision to use the first part of the passage but to suppress the second can be associated with the non-economics disciplines in the sample (e.g., education, law, sociology, public affairs, and philosophy with no known focus on economics).12 Table 5 in the Appendix B shows the contingency table.13

In summary, there is a dominant position that Smith advocated taxpayer funding, and there has been no discernable movement away from that position. Despite Smith providing a summary that stands in tension with that position, only a small portion of the scholarship has cited it. Despite that summary being in a distinct section called the “Conclusion of the Chapter” which has as its solitary function to summarize his recommended roles of the state, many have cherry-picked from it. Smith’s

9. The Pearson Chi-squared test fails to reject the null, thus the era appears unrelated to the nature of the interpretation ($\chi^2 = .26, p = .61$). Robustness checks using different demarcation dates came to the same result. For instance, year 2000 ($\chi^2 = .08, p = .77$).
10. $\chi^2 = 11.87, p = .0005$.
11. One example was removed as that it was published by an organization with no disclosure of the authors.
12. But it should be noted that a great many economists and historians of economic thought fail to cite it at all.
13. $\chi^2 = 11.48, p = .0007$. 
writings have attracted many disciplines but very few outside of economics and the history of economic thought have cited the summary in whole. The tendency to suppress or avoid complexity begins to hint at the possibility that authorial discretion may have to be challenged, and due diligence insisted upon.

**Literature review**

The literature review seeks to answer the following questions: (1) What is it about Smith’s article on education which creates such a strong adherence to the state interpretation and leads to a decision to omit the key passage? (2) What, in contrast, comes into service for the dissenter other than the key passage? (3) Has the key passage moderated the state view in any way? (4) How should we recognize credibility on this matter?

**The logic of state interpretations**

Two passages are frequently cited to serve state interpretations of Smith: “The publick can facilitate this acquisition [of the most essential parts of education] by establishing in every parish or district a little school” (WN, V.i.f.55); and “Though the state was to derive no advantage from the instruction of the inferior ranks of people, it would still deserve its attention that they should not be altogether uninstructed” (V.i.f.61).

Many find it clear that Smith proposed extensive reliance on taxes and government control. Mike Hill and Warren Montag (2015, 75): “What Smith has in mind here…is nothing less than public education.” Patricia Werhane (1991, 19): Smith had in mind “the ideal of universal public education.” Sandy Baum (1992, 152): “Smith advocated universal education with the expenses to be paid out of general public revenues.” And Ellwood Cubberly (1920, 621): “The State has every right, not only to take over elementary education as a state function and a public charge, but also to make it free and compulsory.”

Smith’s plan would be a major change. Ryan Hanley (2009, 59–60): “Smith makes clear precisely what he has in mind…an expansive and unprecedented proposal for public education at state expense.” Gertrude Himmelfarb (1984, 59): “He urged the establishment of a state-administered, state-supported, state-enforced system of education with only token fees to be paid by the parents… He now advanced a scheme requiring a greater measure of government involvement than anything that had ever existed before.” Gavin Kennedy (2005, 226): Smith’s proposal “represented a substantial intervention by the state in the economic mode of his narrative of perfect liberty.” Emma Rothschild (1992, 91): Education con-
tains “some of his boldest proposals.” Dennis Rasmussen (2008, 107) calls Smith a radical, naming public education the “most important” of his recommendations to aid the poor. And Samuel Fleischacker (2004, 234–235) goes further, believing that Smith’s extensive recommendation in education demonstrates that his concept of sovereign is thus “broad enough to include practically all the tasks that modern welfare liberals, as opposed to libertarians, would put under government purview.”

Yet there appears to be a limit imposed upon his would-be proposal. Smith warns, “The master being partly but not wholly paid by the public; because if he was wholly, or even principally paid by it, he would soon learn to neglect his business” (WN, V.i.f.55, my emphasis).

This passage has lent to a different pattern of responses: the mixed-mode interpretation whereby Smith is said to have sought only partial funding through taxation. Jessie Norman (2018): “The remedy Smith advanced is for government to establish a widespread system of local schools…paid for partly at public expense and partly through very moderate fees.” This mixed-mode interpretation is widespread and gains credence with scholars based on the fact that Smith provides an example (and one close to home): the Scottish parish system. Ian Ross (1984, 183): “Smith advocates a system of limited universal education, something like that of the parochial schools of Scotland.” Charles Fay (1930, 32): “He extols the parish schools of Scotland.” As for the right mix of taxes and user fees, Smith is silent. Those whom I have categorized as offering a state interpretation seem to suggest this example is determinant of a request for a large proportion for taxes. Dissenting literature does not.

What motivates Smith to cede responsibility to a state apparatus? A common answer in the state literature is that expanding education through the state could relieve suffering and provide private and public benefits. Smith has much to say on these matters. Of suffering, Smith provides a compelling depiction of the poor laborer, which includes moving comments such as: “He naturally loses…the habit of such exertion, and generally becomes as stupid and ignorant as it is possible for a human creature to become. The torpor of his mind renders him, not only incapable of relishing or bearing a part in any rational conversation, but of conceiving any generous, noble, or tender sentiment” (V.i.f.50).

Many have noted the passion with which Smith describes that suffering, and they imply that these passions motivate him in his endorsement of government intervention. Smith’s words are “harrowing” (Muller 1993, 150), “disturbing”

15. Missing on both sides is attention to the historical record which could shed light on the financial arrangements that Smith may have been aware of and possibly found exemplary. Contrast this oversight to the great attention given to identifying his source for the pin-factory analysis.

As for benefits, Smith writes, “The more they are instructed, the less liable they are to the delusions of enthusiasm and superstition, which, among ignorant nations, frequently occasion the most dreadful disorders.” Educated people are “more decent and orderly,” “more respectable” as well as respectful of superiors, “more disposed to examine, and more capable of seeing through, the interested complaints of faction and sedition,” and “less apt to be misled into any wanton or unnecessary opposition to the measures of government” (V.i.f.61).

This vigorous treatment of benefits has perhaps attracted the greatest attention. There seems to be universal agreement among those in my survey that Smith saw social and/or private value in the poor having education. The state and dissenting literature differ largely in specifying what kinds of actions such potential benefits mandate for Smith. The state literature has generally chosen to elaborate on these benefits as evidence of the strength of his commitment to education and derived from this commitment a willingness to involve government to ensure its access. The dissenting literature has resisted deriving such an implication.

The state literature richly discusses the benefits. Jacob Viner (1927, 227): “Smith supports the participation of the government in the general education of the people, because it will help prepare them for industry, will make them better citizens and better soldiers, and happier and healthier men in mind and body.” Rothschild (1998, 210–220): Smith wants people to be able to “speculate about the origins of the universe, or go to performances of tragedies, or to have conversations about their moral sentiments.” Ross (1984, 182–183): “He is led to interest himself in the vertical penetration of the Enlightenment”—meaning, the transmission of a “homogeneity of knowledge that animated the enterprise of Encyclopédie.” Jack Russell Weinstein (2007, 64): “His education comments are concerned with political as well as moral stability… Class barriers, in a commercial society, are great barriers to mutual sympathy.” Weinstein (ibid., 65), again: “A society only has perfect liberty if its members are actually capable of changing their trade when it pleases them.” James Alvey (1998, 4): “Moderation, a type of self-command (which is integral to Smith’s moral theory) is a sort of public good requiring government support.”

Irrespective of how compelling the case for expanded educational access is, a great many recognize that Smith does set a precondition for state provision. In

16. It should be noted that some judgments such as these are shaded with delight at the notion that Smith is belatedly recognizing the irony of his beneficial system having deleterious side effects, and that some others feel these negative effects were exaggerated by Smith for rhetorical purposes.
the prelude to his public works analysis, Smith considers it appropriate for the sovereign to provide only those goods and services “which it can never be for the interest of any individual, or small number of individuals, to erect and maintain; because the profit could never repay the expence…though it may frequently do much more than repay it to a great society” (IV.ix.51). For Smith, private parties would need to reveal a current and future inability to provide the socially beneficial product.

Some specifically note this precondition. Kevin Quinn (2013, 121) writes, “As with any other good, then, the case for government involvement in its provision or financing will depend, for the economist, on identifying a market failure of some sort.” Others seem implicitly to recognize it, and others to give it no attention. Regardless, for those whom I have classified as having a state interpretation, this precondition has been met in Smith’s time or would inevitably be met in any society built upon liberal commerce. Jerry Muller (1993, 148, 150): Smith was “writing at a time when there was no general education. … As an antidote to the mental degradation caused by the division of labour, Smith recommended universal public schooling, largely at government expense.” Quinn (2013, 121): “There did not seem to be a market for educating the poor.” Alvey (1998, 4): “Education is not sufficiently provided to the public by the private sector.” Dogan Göçmen (2012, 62): “As a solution to alienation…Smith proposes universal education.” Paul McNulty (1973, 361): “His real purpose was to point out that the deleterious effects of division of labor could be overcome or eliminated by governmental programs supporting educational or cultural programs in which the laboring poor could participate.” Public funding of education was necessary if Smith’s liberal project for commerce was to be considered virtuous.

Scholars have consistently focused on these passages to make the case for Smith as an advocate of taxpayer funding. Such a focus, however, is tight, and that potentially comes with the cost of excluding other material that arguably alters the context and the reading of these passages. And it happens that all of the passages occur in the last quarter of Smith’s article. In the first three-quarters of the article there is a remarkably different tone, as there is in the “Conclusion of the Chapter.”

The logic of state interpretations which cite the passage

Some advocates of the state interpretation have let the “Conclusion of the Chapter” passage appear in their articles. Has doing so caused some moderation of the state view that has not been captured by my broad categorization scheme? Is
there something to learn about the passage from their readings of it?

Unsurprisingly, those citing just the first part add no new perspective. Brian Lund (2002): “The economists, [Smith in particular], who promoted the market as the most efficient distributive mechanism agreed that education was the most important exception to the general prohibition against state intervention.” Edmund Phelps (1997): “The public sector was to serve economic development—to expand opportunity.” Irene Gordon and Lawrence Boland (1998, 1235): “Adam Smith clearly acknowledges that there is a necessary social role for government [in defense, justice, roads, and education].… Market failures exist.” Elise Brezis (2006, 21): “Adam Smith was aware of the positive externalities of education on the society, and therefore thought that educational institutions should be financed in some part by the state.”

Rather than showing any particular attenuation of state sentiment, there was even a tendency toward didactic and dubious fiddling with state sentiments. Janice McClung Holtkamp (1994, 3), for example, places the truncated first part in the position of an epigraph—therein, summoning Smith as a definitive and authoritative voice introducing a study which will claim to show “that investment in higher education by society is a necessary and beneficial endeavor.” D. Naismith (1988, 28) includes the first part in artificial block quote which creates a highly deterministic characterization of Smith. He cites the third duty of the sovereign as “erecting and maintaining certain public works and certain public institutions.” But he removes all the conditions, and instead appends to it “Education…may, therefore, without injustice, be defrayed by the general contribution of the whole society” (ibid., my emphasis). The “therefore” takes on a far different association than in Smith’s original. And Frank Solomon (2013, 158) uses an artificial block quotation to the same effect: He conjoins Smith’s “The publick can facilitate” clause to the final paragraph of the “Conclusion of the Chapter” instead of to the key education paragraph I have focused on which is adjacent to it.18 The final paragraph merely restates the general principle of market failure, therein permitting Solomon to avoid the hedged comment about its applicability to education.

Perhaps surprisingly, those who cited the whole passage also did not attenuate their interpretations in any way. Joseph Spengler (1975), Elehanan Cohn (1979), Claire Palley (1991), Brendan Walsch (1998), Orhan Kayaalp (2004), and Wilfried ver Eeck (2013) may have cited the whole passage, but they provide no interpretation of the whole. Instead, they appear to have cited it merely for the apparent drift of its first part.

Only a few authors cite the whole passage and sense the contrary drift of the second part. Andrew Skinner, for example, writes of the conclusion, “The modern

18. Solomon (2013) has not been included in the dataset as he did not cite the passage.
reader has to make a considerable effort to understand Smith’s intentions” (2012, 168). Skinner also writes that “Smith’s position was thus somewhat ambiguous” (1995, 88). Murray Milgate and Shannon Stimson (2009, 114) find that the duality of the passage shows Smith was “less than whole-hearted.” Charles Leathers and Patrick Raines (2003, 54–69) pause at it as well. However, none of these authors maintain these dissenting tacks. They each resolve the passage by adopting a dual-policy interpretation which is common in the literature.

This dual-policy interpretation states that Smith had two views of subsidization, one for the rich attending universities and one for the poor needing elementary education. Smith addresses both types of students within his article “Education of Youth.” The argument goes that Smith cannot recommend subsidies as a financial structure for universities because they tend to corrupt quality, but he nonetheless recommends expanding them as a solution for the poor. Reisman (1998, 376) writes, “In respect to Oxford, Smith was a libertarian…In respect to secondary education for the lower classes, however, he was visibly more interventionist.” Richard Stone (2005, 69–71): Smith is “curiously negative” regarding the “educational arrangements of his day” for higher education; but Smith is an “optimist” regarding “the children of the common people.” Rothschild (1998, 209): “He finds no good reason that the public should pay for educational endowments, or for the great ecclesiastical corporations…but he is strongly in favour, by contrast, of public support for the education of the ‘common people.’” Jerry Evensky (2005, 228): “Although Smith wants government to finance education for the working class children, he is very much against government financing of that institution which provides the instruction of people of all ages: religion.” Clyde Dankert (1974, 164): “It should be added that our author was thinking here [on the disincentives of endowments] primarily of education for the well-to-do classes. He supported a somewhat different arrangement for the education of the masses.”19 Finally, Pierre Tu (1966, 10) and George Fallis (2007, 91), who cite Smith’s final words, assert this dual-policy perspective as well.

The question is: on what basis has this dual-policy account been established? It is unclear. There is no particular citation which establishes it, and the authors do not explain themselves. My inference is that the two distinct policies either appear so self-evident to these authors that they feel it requires no elaboration, or that they arrive at two policies as a logical necessity to make sense of an article which is at first highly critical of subsidization and then seemingly receptive to it. Only those who cite Smith’s final words and sense its duality seem to wrestle overtly with the dual-policy idea.

19. For more examples see Morrow 1927, 326; Robbins 1965, 90; A. Brown 1975, 267; Himmelfarb 1984, 58; Rothschild 1998, 210; Ortman 1999, 303; Birch 1998, 34.
For instance, Skinner provides varying interpretations. In one instance, he resolves the tension in the passage in support of the mixed-mode model (Skinner 1995). In others, he wants to address the passage as only applicable to university students and ignore that it applies, as written, to elementary education as well (Skinner 1988; 2012). Skinner leans toward the dual-policy interpretation, stating that Smith “protested against” university subsidization (Skinner 1988, 12–13) but recommended elementary subsidization even as it “infringe[s] the natural liberty of the subject” (1988, 10). Similarly, Milgate and Stimson (2009) and Leathers and Raines (1992; 1999) want to discuss the passage only in relationship to religious education. Milgate and Stimson (2009, 114) assert that Smith “sharply distin-
guished” his views of elementary education from religious education, rejecting endowments for religion but “unambiguously” recommending taxes for elementary education. It is important to note that to maintain this interpretation, Milgate and Stimson tinker with Smith’s final words which pertain to “education and religious instruction” alike, and they drop the word “education.” These efforts by Skinner (1988) and Milgate and Stimson (2009) seem to imply that Smith’s message requires some rewriting or downplaying in order to align it with their impression from the article itself. But again on what basis is that impression formed?

As far as I can tell, it is only Leathers and Raines (2003) who provide a stated reasoning to adopt a dual-policy interpretation. Leathers and Raines state that "the entire discussion of the education of youth devoted more attention to university education for the sons of ‘gentlemen and men of fortune’ than to the basic education of the ‘common people’” (2003, 54). In addition, they too seem to be governed by another premise—namely, that Smith’s writing tends toward “incompleteness, weaknesses and contradictions” (ibid., 69). My reading of their approach is that Smith’s final words—which make no distinction between the rich and the poor—should have done so if they were to be consistent with Smith’s treatment of endowments. Predominantly derived from observations of universities, his study of endowments produces only weak implications for what to do about elementary education. Smith had no intention for the lessons of endowments to be broadly applicable. 20

If there are other rationales by which to establish a dual-policy perspective, I am unaware of them. Another that I sense but cannot attribute to any particular author is that Smith, in facing whether to commence a public work, contemplated the issue in terms of likelihood and magnitude of effects. Smith’s article might make clear that he views positive effects as unlikely, but perhaps Smith finds that the upside potential for the poor renders public financing a worthwhile experiment,

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20. Leathers and Raines (2003, 55), interestingly, reference E. G. West to dispute his position regarding economic gains, but they do not acknowledge that West had a different reading of the key passage.
i.e., worth the risk of wasted expenses and further marginal degeneration. None have written anything so formal. However, this narrative perhaps fits with the spirit and narrative of many interpretations: Smith is embarrassed by the strength or excessively broad application of his argument for commerce in WN, and now he must concede to a different perspective and make amends for the damages done to the poor. That is, Smith’s cool calculations and excessive prudence must give way to a compassion. The problem with this rationale, however, is that it certainly goes too far if Smith’s final words represent his views. Namely, the only effect of this transformation of thought in his final adjudication is that, instead of rejecting government outright, he cautiously includes it as an option in a solution set, and a disadvantaged option at that.

To conclude, very few of those who cite Smith’s final words note the contrary drift, and those who do nonetheless recast it in favor of a state interpretation. As such, moderation of the state views has not occurred. In this process, some curious liberties have certainly been taken with Smith’s final words, but in doing so these authors elevate the challenge for anyone who would place value in them. Perhaps the conclusion poorly represents Smith’s views, and must be reinterpreted through the greater lens of the article. And indeed, one wouldn’t want to abandon an entire article in favor of a single citation if that citation is not representative of the rest.

But is it the case that Smith’s final words do not match the article? The dissenting literature relies on both, suggesting there are harmonies between them. The dissenting literature also takes some pride in recognizing these harmonies and has accused the orthodox literature of inattentive reading. For instance, they state that much of the article, especially his criticism of endowments, has been “neglected” (West n.d., 2) and treated as “mere curiosum” (Rosenberg 1960, 568). I draw a similar conclusion about the state literature. I find little attention being dedicated to the first three-quarters of the article. I also identify a great many authors who sense the article is “contradictory,” “vague,” “awkward,” “unspecific,” “muddled,” “odd,” “deficient,” “ambiguous,” “wandering” and “unformulated,” and yet who have not pursued these internal tensions and peculiarities for alternative meanings. Different levels of attentiveness to much of the article may account for the disparate views.

The logic of dissenting interpretations

The survey identified 40 interpretations which hold a dissenting view of
Smith’s favored role for government in education. They take great inspiration from the first three-quarters of the article, from which none of the prior quotes come. There Smith maintains that endowments are disincentives to quality instruction because they provide guaranteed salaries to teachers irrespective of performance. For dissenting scholars, his depiction of the ill effects confidently answers the primary question of his study: Are endowments a good model which validates establishing government schools based on a tax fund (V.i.f.2-4)? The dissenting scholars answer with varying degrees of confidence that endowments provide no such validation for taxes. They do not directly address the possibility of a dual-policy interpretation. They instead treat his argument as uniformly condemnatory. As that their accounts are usually relatively terse, I must infer why they do not consider the dual-policy view. They either find the specifics of college and university endowments as unessential to his argument; or they reject the factual basis of Leathers and Raines’s premise that Smith is predominantly focused on colleges and education. I will first identify some of the passages that inform their reading. Then, I will outline what I believe steers dissenting scholars away from a dual-policy perspective.

Some of the most highly critical passages of endowments occur, indeed, in regards to colleges and universities. Smith writes of the fully salaried university teacher: “His interest is, in this case, set as directly in opposition to his duty as it is possible to set” (V.i.f.7). Of partially salaried teachers, he writes of partial effort, and explains that effort is “always more or less diminished” accordingly (6). Authorities cannot improve this causal relationship. “In the university of Oxford, the greater part of the publick professors have, for these many years, given up altogether even the pretence of teaching” (8). Authorities often worsen the causal relationship, rendering the teacher “obsequious” to the arbitrary goals and “one of the meanest and most contemptible persons in the society” (9). Of natural will toward excellence and personal reward, Smith is suspicious (4).

In each case, Smith’s explanation is clear and rhetorically charged. Guaranteed salaries and limits on student choice of college teachers will “extinguish all emulation” (V.i.f.13), “blunt the edge of all those incitements to diligence” in teachers (14), and cause a teacher to operate with interests as “in opposition to his duty as it is possible to set” (7). The college tutor or teacher may “commonly teach very negligently and superficially” (33), may “flatter himself that he is giving a lecture” (14), may “neglect his duty” (8), even “altogether” (9). Endowments, at least, in colleges and universities, fail to achieve quality because of their primary function of subsidizing the teachers’ salaries.

Endowments also fail to produce meaningful curricula. With teachers and administrators receiving guaranteed salaries, students have no means to command change. As a result, over time the curricula fail to render the education “proper” for
“the education of gentlemen or men of the world, or more likely either to improve
the understanding, or to mend the heart” (V.i.f.32). They become full of “exploded
systems and obsolete prejudices” (34). Some fields may even be a “useless and
pedantick heap of sophistry and nonsense” (46). Regardless of intent, a lecture
devolves into a “sham-lecture” full of “nonsense” (14).

Does this negative assessment of colleges amount to a condemnation of
a specific college form of endowment or, instead, does it contribute to
condemnation of all endowments? I believe dissenting scholars generally recognize
that Smith does not set up his study to find a variegated set of cause-and-effect
relationships, nor does he find them. He asserts that performance pay is integral
to effort “in every profession” (V.i.f.4)—even in teaching, about which he
hypothesizes that endowments “have necessarily diminished more or less the
necessity of application in the teacher” (5). Here is not a partial or particular
hypothesis about colleges. In fact, he believes his hypothesis to be true in both
“schools and colleges” (ibid., my emphasis). In the ensuing test of his hypothesis,
in no case does his commendation of them depend upon any variable unique to
colleges.

Dissenting scholars may note that Smith also addresses non-college
examples and his message is consistent. Endowed colleges and universities are
without merit, but England’s privately funded “publick schools” for grammar and
language (by which he means schools outside the home), are “much less corrupted
than the universities” because they are based on user fees. In another comparison,
in endowed riding schools, “good effects” are “not commonly so evident”—but in
privately funded schools of fencing and dancing, a student “seldom fails of learning
to fence or to dance” (V.i.f.16). The distinguishing characteristic of these
comparisons is solely whether endowments exist—not whether a certain kind of
endowment exists or whether a certain kind of student is involved. The
comparisons make the point that endowed schools differ from private schools;
edowed schools do not differ from each other in ways that require him to modify
his judgment.

Dissenting scholars may also note that Smith is consistently critical across
a large sample set. Unsatisfied with the limited variety in his contemporaneous
examples, he turns to the historical records of ancient societies to determine if
endowments have ever had a positive outcome. Smith examines the ancients’
schools of gymnastics and music; reading, writing, and accounting; philosophy and
rhetorick; and civil law. He cannot find a single example where subsidization was a
cause of the success of the school. Moreover, he makes clear that a better direction
of focus for governments may be the stimulation of demand for education rather
than the supply. Counting these schools amongst his examples may render
Leathers and Raines’s premise false. Smith dedicates more time to colleges and
universities, but those examples constitute a minority set in his entire study.

Still, why address colleges and universities at all in a study dedicated to understanding the “Institutions for the Education of Youth?” And then why so much attention? Does this not inevitably confuse the reader as to the proper scope of his condemnation? To the first question, to provide a thorough study would require attention to them, as that the concept of the “education of youth” contained within its domain college and university students. This can be established by legal accounts (e.g., Pickering 1762), as well as through common parlance and other education writing (e.g., Denning 1763). To the second question, there are two natural reasons for the magnified attention. First, endowed colleges predominate among the potential contemporaneous examples. Endowments, as a means of funding, are strongly intertwined with the history of colleges from their 13th century inception (Wood 1792) and thus they represent the bulk of contemporaneous examples at his disposal. Second, they are the examples which his audience had experience with and could relate to. Smith is using familiar college and university examples in the same manner that a parent would use known and particularly poignant examples of car crashes to advise their rash child how to avoid the same end.

But one might argue that these answers are insufficient to explain Smith’s lengthy and well-known history of colleges and universities, which spans approximately a quarter of the article. I cannot speculate as to what dissenting scholars think of this history, but in my own work I have offered some additional context which explains its length. Namely, his history does not occur arbitrarily as a digression (or a fixation). It is prompted by a specific challenge from a rhetorical interlocutor: a warm-hearted and well-intended sentiment which argues that despite endowments’ problems, something is better than nothing (V.i.f.18).22 Smith’s history is a carefully chosen response to this sentiment, a case study or morality tale for the audience unmoved by his methodology thus far. His history goes on to show that endowments are greatly to blame for the intellectual, moral, and social destruction occurring across Europe. A series of ironic counter-responses to the challenge wraps up his history: a portrayal of wealth turning to poverty, legacies degrading into feebleness, science devolving into nonsense, and good intentions ending in social ruin (V.i.f.34–37, 46-47). The history of colleges and universities provides the correct lens to make these points, as worries of moral decay and poor quality in British colleges was a frequent point of conversation.

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22. In Drylie (2020), I more fully show the presence of a dialogic structure used by Smith to further his argument, and I characterize this paragraph as an interjection of incredulity. Leathers and Raines (2003, 56) attribute this warm-hearted sentiment to Smith himself, calling it his “most revealing comment.” I find no merit to that reading based on the context.
at the time. Smith utilizes those anxieties to his advantage. That he again rebukes the challenging sentiment of something-is-better-than-nothing later in the article reveals how much the sentiment worries him for its ability to persuade people to retain some faith in the rotten institution of endowments.  

Finally, I suspect that some scholars are attentive to the tone and manner in which he concludes his analysis of endowments. He writes of the ancients: “The demand for such instruction produced, what it always produces, the talent for giving it; and the emulation which an unrestrained competition never fails to excite, appears to have brought that talent to a very high degree of perfection” (V.i.f.45, my emphasis). He finds that the ancients, unburdened by endowments, were superior to modern teachers. He also introduces the language of markets for the first time in the article. He writes of “competition,” “trade,” “sale,” “goods,” “price,” “profit,” “demand,” and “bankruptcy” (45). Therein, he situates the market for education back into his general view that freely functioning markets can naturally arrive at superior ends for society. Education is unexceptional. It may need encouragement and protection, but it is not so distinct that it requires government production.

Dissenting literature pulls from these citations and impressions to various degrees and arrives at a range of portrayals of Smith. Smith is either unsure, restrained in his invitation to government, principally resistant to government, or advocating alternatives to government. I shall now provide examples of their commentary.

Given his earlier criticism of endowments, some scholars react to the tone, ambiguity, and under-defined qualities of his would-be recommendations for government, and they derive uncertainty. Arthur Lynn (1976, 371): “No final solutions and little practical guidance are to be found here. Given the intractability of such problems, his uncertainty is understandable; one has the impression that he really favored market-like price equivalents in most cases but recognized the pragmatic reality” (my emphasis). Christopher Lucas (1972, 363–364): “Whether the government was to help support the system also is uncertain.” Among those who cite the passage, Peter Browning (1983, 70) writes, “Smith’s long—and delightful—discourse on education does not, however, lead him to any firm conclusion on who should pay.” Tom Stonier (1985, 14–15): Smith “equivocated on the question of whether the expense of education should be borne by public or private funds.”

23. He concludes his overall assessment of endowments, “Were there no publick institutions for education, no system, no science would be taught for which there was not some demand” (V.i.f.46). This statement has frequently been misattributed to the statist idea that a society cannot rely on the preferences of society, but rather must provide what legislators know to be of importance. But an honest assessment of the context shows that it is his second and very pointed rebuke of the idea that any action is better than none.
Others grant there is a role for government but believe there is no textual basis for anything more than a minimal one. Milton Friedman (1976): “Smith himself did not regard his third duty as providing extensive scope for governmental activity.” Jeffrey Young (1997, 200): “Government must take ‘some pains,’ but there is no indication that this requires anything more than a marginal infringement of a natural liberty…It does not necessarily mean government operated schools, much less the demise of capitalism.” James Otteson (2011, 166–167): “This intrusion on the free market that Smith endorses is quite a weak one.” P. J. O’Rourke (2007, 141): “Adam Smith was only a tepid advocate of public education.” West (1964, 466): “Adam Smith was especially careful to avoid a state system which would reproduce the errors of endowed schools.” West (1980, 998), again: “Close textual examination reveals the tamest of Smithian programs…This is surely not a program of social control via coercive state education.” Alexandra Hyard (2007, 76) concludes that Smith’s proposal was “a minor proposition” and “mainly…a private matter.”

Occasionally, the reasons for Smith’s resistance are elaborated upon. Milton Friedman (1976): “Despite Smith’s acceptance of the appropriateness of governmental establishment and maintenance of such institutions, he devotes most of his discussion to a scathing attack on the effects of governmental or church control of institutions of learning” (my emphasis). Fay (1930, 187): “The sacredness of liberty to him explains why, unless there be no other way, he is disinclined to the provision of education by an external body.” Charles Griswold (1999, 254) finds approval for government only “as long of course, as the means chosen are carefully thought through so as to prevent the evils that always accompany the creation of state-enforced standards (e.g., the creation of administrators of those standards, in this case teachers, whose income is assured).” And West (1964, 475) reminds the reader that “suggested improvements must be envisaged in the realm of the politically possible. The state is not a disembodied abstraction and its officials are presumably just as fallible as other human beings.” In large part, these scholars have reacted to Smith’s criticism of endowments and understood his displeasure through a broader understanding of his relationship to liberty.

Those who cite the passage convey a similar message of disapproval, and often characterize it as his pointed, conclusive warning against the state. Steve Bradley and Jim Taylor (2004, 2): “In spite of the warnings delivered by Smith in the 18th century, and the recent interventions by Friedman and other right-wing academics, the trend until very recently has been towards increasing government intervention in education by the state.” David Friedman (1997): “It is often said that Adam Smith, despite his general belief in Laissez-faire, made an exception for education. That is not entirely true. In the course of a lengthy and interesting discussion, Smith argues both that education is a legitimate government function,
at least in some societies, and that it is a function which governments perform very badly.” Robert Wright (2017, 126): “Smith, after all, was a keen observer of government. He realized that although the government could occasionally run a successful business, like the loan office in Pennsylvania, the public pawnshop in Hamburg, and the various postal systems, government businesses often failed because of ‘slothful and negligent’ provision, and ‘thoughtless extravagance.’” And George Montgomery (1949), introducing the whole “Conclusion of the Chapter” writes, “Were Smith’s treatment of government spending and borrowing to have come to the attention of certain politicians and liberal economists of the past two decades, they should have refrained from making some of the flagrantly immature declarations as to the significance of the economy of the state and particularly public debts.” Smith’s final words, then, permit these authors to heavily weight Smith’s overall suspicion of the state. Education is not an exception.

Some scholars conclude that Smith’s faith in private action was discernible in the article and constitutes his preference. Milton Friedman (1976): “[Smith] favored financing [institutions for the education of youth] largely by specific fees.” Edward Ryan (1990, 152): “Smith gave his support to this method [of user fees] in such areas as education and the use of turnpikes.” West (1964, 465–467): Smith preferred to “remove all obstacles to competition” and rely on “market forces in providing education.” Eamonn Butler (2007, 69): “He remains unclear on just how much government should pay towards this basic education, though he expresses high regards for the private schools for the skills like fencing or dancing, where the students pay the whole amount.”

Those citing the passage agree. David Friedman (1997): “His conclusion is that while it is legitimate for government to subsidize education, it may be more prudent to leave education entirely private.” Wright (2017, 126): “Most public services, therefore, were best left to the private sector, including, where appropriate, non-profit corporations.” Kerry King (2005b, 357): “Although the debate over school choice has just recently become a major topic in education reform, implementing choice and competition in education was first discussed by Adam Smith.” Arthur Taylor (1972, 47): Education, “being of benefit to society as a whole, could legitimately be made a public charge…[but] Adam Smith offered [the free market] a modicum of encouragement when he expressed a preference for a system in which the expense of education was ‘defrayed altogether by those who receive the immediate benefit.’”

Aside from user fees, the phrase “voluntary contribution” in the passage conveys to some authors that Smith also held faith in charity. Walter Clark (1903, 230) concluded succinctly that Smith approved of “education of common people in charity schools.” Jack High (1985, 317), paraphrasing Smith: “If we do in fact benefit from the additional education of others, voluntary contributions enable
us to capture these benefits without the necessity of government action.” Robert Heilbroner (1986, 312) comments in his condensed version of WN that education “may also be defrayed by general contribution, but better by fees and voluntary contribution.” West (1994), adopting the persona of Smith in a mock interview with the Minneapolis Federal Reserve, states: “I always wanted the parents, even poor parents, to pay fees covering some significant part of the costs, and if there was to be some non-parent support, I recommended voluntary contributions from the immediate neighbors. This was not therefore an argument in favor of state education as so many writers tend to believe.” Finally, my own work aims to rehabilitate the place of charity in his thoughts on education (Drylie 2016; 2020).

The thought of charity has crossed the mind of a few who offer a state interpretation, but they sell Smith short. Daniel Rauhut (2005, 29–30) implies that Smith missed the fact that “private agents can provide several of the things Smith lists as government duties.” Reisman (1998, 371): “The State is in and the market is out—or perhaps Smith simply threw in his cards without appreciating just how much the automaticity of interest could rise to the challenge.” And Gaston Rimlinger (1976, 230) claims Smith had “little faith in private charity.” Overall, few have noted or have taken Smith’s phrase “voluntary contributions” as a meaningful proposal. However, Smith does in fact identify charity as an option in the article, which he attributes to the English solution to the problem (V.i.f.55). A close reading will reveal that he in no way dismisses the option.

A pattern that emerges in the dissenting literature is that Smith, indeed, contemplated a role for government but constrained or denied it. These scholars turn to the abundance of criticism of endowments as evidence that Smith’s true message to the reader is to proceed warily and to resist the seduction of any apparent easy solution. Because of public benefits, government action may be looked at as just; however, government action will likely disappoint and end up being unjust.

Three distinguished dissenting interpretations

It is important to note that the dissenting literature has not strongly argued the points I make above, nor heavily substantiated them. My above citations of Smith far exceed evidence provided by any one of them. And while those who cite the passage tend to make the argument more forcefully, there remains a tendency toward terseness. In only three cases do scholars provide elaborate interpretations. Each makes contributions beyond points in my foregoing discussion.

Knud Haakonsen (2006, 20–21), who does not cite the passage, writes that government is part of the solution to “replace the moral community of spectators that is lost when people move away from the dependency relationships of
traditional society to the ‘anonymity’ of the wage economy in commercial society.”

But then he importantly defines Smith’s notion of governance as encompassing “the great number of public offices which were certainly of a civic nature but which were not offices of the state.” Here is a notion of governance which opens up a much more indeterminate solution set of possibilities than the set of mixed-mode options which have presented themselves so far. Haakonssen concludes that the funding for education will come “in part by society, and by the multiplicity of confessional groups that tend to arise from freedom of religion.” The politics to such ends should crucially be driven by “a wide variety of leadership roles in local communities.”

The other two distinguished interpretations do cite the passage and apply a similar historical perspective. First, E. G. West, who has investigated this subject more than any other has, wrote:

Smith was ultimately so heavily critical of the way in which any element of “subsidy” was in danger of being squandered, that one often gets the impression he believed they were typically non-productive, if not counterproductive. The penultimate paragraph of his long section on public works [the key passage discussed in this paper] deserves more attention than it usually receives. It shows Smith’s final balance of all the arguments reviewed earlier in the same chapter… It is relevant that Smith relies on voluntary contributions from the “neighbours.” These neighbors would recognize benefits to themselves of the prospects of the poor in near proximity to them having an education. With a free-rider problem partially resolved through proximity, education can therefore be left to these neighbors to provide subsidies to augment family education and expenditure. (West 1977, 15–16)

West is particularly keen on Smith’s “voluntary contribution” phrase because he believes Smith’s concept of the sovereign was not equivalent with today’s government, but rather represented a range of government and quasi-government roles. The sovereign was generally cast first as a “stage manager” and not a “leading actor,” a protector of property and markets, not a producer (West 1977, 18). The sovereign’s role would involve, then, the active enabling of both private and charitable markets, which at the time did require enabling (Drylie 2020).

The last example is from Craig Smith:

Without this local responsibility there is a real danger that the providers will become unaccountable and inefficient. The sort of government action that Smith is endorsing here is clearly not that of a remote eighteenth-century national government, but rather that of a local association accountable to its residents. Moreover, even in this case the service is to be organised in such a way that the individuals involved have direct relationships unmediated by a
state apparatus. Those who “receive the immediate benefit of such education and instruction” are reasonably to be expected to meet the expense, just as those who make use of the legal system will support the expense involved through court fees. It would seem that Smith thought that most of the activities covered by his heading of police would be maintained at a sub-national level and quite separately from the institutions concerning justice. (C. Smith 2012, 795)

These interpretations by Haakonssen, West, and Craig Smith have similar qualities. They eschew the cloying framework which is so common in the literature and which states that even this icon of the free market could admit its shortcomings when it came to important things like education. Instead, they reveal that a close reading of the text in historical context allows the article to be both internally coherent as well as coherent to the principles that run through WN. They cast Smith as a writer who is gifted, disciplined, and tending toward consistency in his thinking. They elaborate on the logical possibilities of the text—like those sensed by, say, a William Boyd (1932, 324), who only speculated that Smith’s choice to name the agent as “the public” is one that “leaves open the possibility of other educational authorities.” They resist the temptation to conceal complexities, and over-define the solution for the modern reader, as did Eli Ginzberg (2002, 102) who simply replaced the word “public” with “state” in the phrase, “The public can facilitate, encourage, and impose.” They resist the misdeeds of, say, an Albion Small (1907), who misquoted Smith’s subject of criticism throughout the first three quarters of the article as “private endowments,” when Smith in fact makes no distinction and used the phrase “publick endowments” (V.i.f.3). Regardless of one’s ilk, these well-known scholars’ efforts on this topic deserve as much attention as their efforts elsewhere have received.

Conclusion

There are many aspects of Smith’s article which could bring public education to mind. Smith recognizes the challenge of educational access and describes the problems of the poor; he identifies many private and social benefits which would derive from more widespread access; he specifies a market-failure rationale for government to intervene; and he makes a call to action. But Smith also provides a theory that public subsidization, as a specific kind of action, will be corrupting; and as one of his first French translators noted, Smith lays out “incontestable examples” of that corruption (Garnier 1843/1802, xxx).

The challenge for the reader has been to determine how to characterize Smith’s call to action, given his misgivings about endowments. The majority since
1893 have either lightly weighted or dismissed his anxieties about endowments and have read the text as providing a clear prescription of subsidization. Those who have dissented have been moved by the vigor of his criticism and have characterized his call to action as vague or constrained by his criticism.

The observed pattern in the literature suggests that literature does not constitute a body of knowledge that makes a strong case for either interpretative choice. The state view almost universally suffers, at a minimum, from omissions. Those who perpetuate the state view have rarely cited Smith’s final adjudication of the funding options, and have even regularly suppressed, ignored, and deflected the part which is inconvenient for the state view. A strong refutation of the inconvenient part is absent, and a recognition that others have dissenting views has almost never been put to paper. The state interpretation hangs a great deal on the claim that Smith’s views of endowments are irrelevant. On the other side, the dissenting view suffers from lack of voice. They have not recognized that the prevalence of state view in the literature necessitates that they provide more evidence and a stronger case. They have not cried out that there may be an injustice occurring in the manner in which many scholars position Smith within a political discourse. The result is that debatable points remain unchecked, and unresolved elements of the text remain unexplored.

I will conclude with an attempt to stimulate the debate which I feel is missing. I provide a series of additional inconveniences for the state interpretation. The dismissal of three-quarters of the article has allowed for the avoidance of these inconveniences. They also have not been substantively put forward, or addressed at all, by dissenting authors. Each elevates what I would expect to be a reasonable burden-of-proof to make the state case. I have elaborated on each of these in prior work (notably Drylie 2020).

• In the most famous of his purported advocacy quotations, Smith writes “The publick can facilitate this acquisition by establishing in every parish or district a little school” (V.i.f.55, my emphasis). Could Smith’s use of the term “the public” instead of “government” matter? A concept of the public as being distinct from the state did exist in Smith’s time. Moreover, Smith’s use of the term regularly reflects that distinction. Considering that immediately after this statement he identifies two ways to establish a school—the Scottish quasi-government model and

24. I borrow this burden-of-proof phrase from Charles Griswold (1999, 295), who writes, “A burden-of-proof argument suffuses Smith’s writing in political economy; the state may intervene in all sorts of ways, but those who would have it do so are required to show why it should in this particular instance, for how long, and in precisely what fashion, and how its intervention will escape the usual dangers of creating entrenched interest groups and self-perpetuating monopolies.”
the English charity model—a different interpretation is possible. Namely, Smith takes advantage of the ambiguity of the term *the public* and uses it as an expedient to broadly summon agents, both governmental and civilian, to consider this option.

- The Scottish quasi-government education system in Smith’s time was not on a strong foundation, but the extant charity system was thriving, broadly supported by those in the rising ranks, and well-known. Thus, his readers would need convincing to abandon charity for government. Smith makes no such compelling case. He never adjudicates clearly in favor of the state system over the merits of charity, but rather places them in close equipoise as the introduces them. In the “Conclusion of the Chapter” he even tends to favor charity, as the term “voluntary contribution” would have clearly conveyed charity in his time.

- Smith repeatedly portrays endowed schools as “corrupt” (e.g., V.i.f.17, 20, 30, 33, 45, 49). But he also specifically denies government any claim to success throughout his assessment of ancient schools, and he chastises historians who have given governments too much credit (V.i.f.39–45). He accuses them of fabricating “ingenious reasons,” being “predisposed” to see wisdom in ancient governance (40), and tending to “over rate” the role of the legislator (45). Therein, he asserts that there is a distinct bias or prejudice of thought on the matter—one which he cannot be cajoled to adopt (45). If Smith still wanted to recommend tax-financed endowments, Smith sets a high bar for himself to explain why society should replace extant options for the poor with one that is theoretically flawed, historically unproven, widely corrupt, and supported by prejudicial thinking. He does not, and likely had no intention to, make such a case.

- The idea of widespread schooling based on a tax fund was afoot in Britain when Smith wrote. It had been recently posed by the popular moralist John Brown (1765) and was passionately critiqued by the famous scientist and educational thinker Joseph Priestley (1765) as well as by the popular, outspoken reverend Samuel Parr (1828/1780). However, none had done justice to the topic, relying on theoretical arguments rather than comprehensive evaluations of endowments’ potential merit. Smith is almost certainly aware of these famous debates, and his article’s rigorous approach may serve to rectify their deficiencies. Perhaps most important to note, however, is that Brown’s inspiration for his provocative work was Montesquieu, who asserted in his 1748 *The Spirit of the Laws* that government should take a role in providing education (2001, 53–56). Montesquieu is generally treated
as insignificant in the historiographies on education, but he had an
important temporary impact in Britain through Brown’s work. Smith
mentions Montesquieu in his article (V.i.f.40) and rebukes him along
with other historians for their romanticism of government. In doing so,
Smith most definitely has entered into this contemporaneous debate,
and taken a side against government.

• In a few brief paragraphs, Smith’s possible solutions contain at least
13 examples of restraints and limits (V.i.f.54–56). The defining quality
of his solutions is their smallness. Even the idea of a school-per-parish
is restrained, as that such a model had already been shown to be
insufficient in providing widespread access in most Scottish parishes.
Smith did not make a push for an elaborate, complete, or expensive
solution.

• Smith never specifies that small schools, small incentive rewards, and
mandatory capability testing should all happen. They could be three
independent options. Additionally, the multiple calls for the state and
the public to “give attention” to education also never specify which
option. Given that Smith shows an appreciation for demand-side
encouragements instead of supply-side provisions in the article
(V.i.f.41, 42, 44, 45) and elsewhere in Book V, Chapter 1 (V.i.e.26,
V.i.g.3, V.i.g.14), it is credible to think that he may prefer incentive
rewards and capability testing. These demand-side alternatives to subsi-
dized schools, however, are almost never discussed in the scholarship.

• Smith shows a glowing appreciation for patience on the topic of edu-
cation (V.i.f.45, V.i.g.14, V.i.g.3). He shows the same appreciation
throughout Book 5, Chapter 1 (V.i.d.9, V.i.e.26). His standard for
market failure is that a private solution shows itself to never be possible.
This pattern, and his failure to cite urgency on the topic of the poor,
suggest that an unfortunate situation for the poor would not easily sway
him to seek immediate or dramatic change.

• In Smith’s time the primary goal of philanthropists was to convince
social, political, and academic leaders that the poor should have
education at all, many of whom saw widespread access as a threat to
the natural order of Britain. For a person of his place and rank, Smith
takes a radical position in support of the poor having education. But an
argument for education is not an argument for government provision
of this education. Historical context would suggest that today’s reader
should resist the urge to lightly treat this distinction.

• In Smith’s time, numerous writers juxtaposed the ‘dead’ giving which
had established endowments with the ‘living’ giving of charity. Despite
both being a form of subsidization, endowments fail because of a lack of ongoing scrutiny for quality and purpose, whereas charity succeeds through great attention to these. The strength of this juxtaposition can be surmised by its longevity into the late 19th century. In Smith’s final adjudication, he employs a juxtaposition as well—between “general contribution” and “voluntary contribution.” This juxtaposition benefits from the contemporaneous one, and, I would argue, efficiently aligns Smith with the larger thinking about how endowments differ from charity. Without Smith having to elaborate why he finds charity superior, his preference for charity be understandable to his audience. And, indeed, the initial responses indicate that his immediate audience saw him as a supporter of charity for schooling.

Quentin Skinner warns that “we can only classify the unfamiliar in terms of the familiar.” In doing so, we may end up with “conceptual muddles and mistaken empirical claims” (1969, 4–6). On the topic of education, today’s public-school models are familiar to writers. It is hard not to think that this familiarity makes certain passages appear as if Smith was prescient—and, if one favors the public-school model, worthy of praise. But a closer textual examination reveals there is much weighing against such an interpretation. That which has been presented as supportive of orthodoxy is not unequivocally so. As Smith was writing, there was an array of government reform initiatives being discussed surrounding the regulation of endowments and charity, and even, nascently, the possible provision by government. Smith enters into that discussion, providing an important study that was received by his immediate audience as providing cautionary lessons (Drylie 2020). Nathan Rosenberg (1960, 570) suggested that the result of giving too little credit to Smith’s interest in exact institutional details in Book V, Chapter 1 “has been a neglect of some of the most fruitful and suggestive aspects of Smith’s analysis and a distortion of the broader implications of his argument.” I second that view. And on reflecting on Alexander Gray’s comments at the start of this study, I am inclined to believe that on the topic of education, the familiar and the partial may have, indeed, displaced the unfamiliar and whole.
Appendix A:
List of publications addressing Smith’s position on school funding

State interpretation


First part only: Clay (1968)**, Bates (1973)**, Reisman (1976), Eze (1983), Novak

**Second part only:** (none).

### Dissenting interpretation


**First part only:** Billet (1978).

**Second part only:** Taylor (1972), West (1980), West (1990), C. Smith (2012).

**Notes:** * dissertation; ** master’s thesis; ~ readily inferable; ^ applied only to universities or religious institutions, but view of elementary expressed as taxpayer funded elsewhere.

### Appendix B

**TABLE 3. Contingency table of era vs. nature of the interpretation**

<table>
<thead>
<tr>
<th></th>
<th>Before 1976</th>
<th>1976 and since</th>
<th>Total</th>
</tr>
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<tbody>
<tr>
<td>State</td>
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<td>115</td>
<td>151</td>
</tr>
<tr>
<td>Dissenting</td>
<td>8</td>
<td>32</td>
<td>40</td>
</tr>
<tr>
<td>Total</td>
<td>44</td>
<td>147</td>
<td>191</td>
</tr>
</tbody>
</table>

**TABLE 4. Contingency table of era vs. decision to cite the passage at all**

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<th>Before 1976</th>
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<tr>
<td>Cite</td>
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<td>54</td>
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<tr>
<td>Total</td>
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### TABLE 5. Contingency table of discipline vs. decision to cite or suppress 2nd part

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<th>Total</th>
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</thead>
<tbody>
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<td>Cite</td>
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<td>33</td>
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<tr>
<td>Suppress</td>
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<td>9</td>
<td>20</td>
</tr>
<tr>
<td>Total</td>
<td>42</td>
<td>11</td>
<td>53</td>
</tr>
</tbody>
</table>

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### About the Author

**Scott Drylie** is an assistant professor and the director of the graduate program for Cost Analysis at the Air Force Institute of Technology in Dayton, Ohio. He is also an active-duty finance officer for the United States Air Force. He teaches economic principles, mathematical economics, and acquisitions cost estimating. His research focuses on the role of learning, expertise, and innovation in organizations and systems. He has a B.S. in Economics from Montana State University, an M.Ed. in Education, an M.S. in Cost Analysis, and a Ph.D. in Economics from George Mason University. His email address is scottdrylie@gmail.com.

*Go to archive of Intellectual Tyranny of the Status Quo section Go to September 2020 issue*
Republicans Need Not Apply:
An Investigation of the
American Economic Association
Using Voter Registration
and Political Contributions

Mitchell Langbert¹

LINK TO ABSTRACT

The American Economic Association (AEA) is the most influential professional association of economists in the United States, and by extension the world. It publishes eight journals, which place highly in standard rankings. For example, in the RePEc ranking (link), AEA journals are three of the top 10 and five of the top 20. Publication in highly ranked journals is a prerequisite to tenure. The AEA runs the Allied Social Science Associations Annual Meeting, which for professional economics is both the premier conference for researchers to show their work and the centerpiece of the academic job market. Those who oversee the AEA have a profound impact on the profession. Publication in AEA journals and executive roles in the AEA are coveted and important stepping stones in economists’ careers.

In this paper I collect data on voter registration and political contributions of AEA members. I focus on how these differ across levels of the AEA hierarchy. I find that the political skew increases up the AEA hierarchy (Figures 3 and 4). While the AEA membership has a 4:1 ratio of Democratic:Republican voter registrations and a 13:1 ratio of political contributors, the officers, editors, authors, book

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reviewers, and acknowledgees (those thanked in published acknowledgments) skew 13:1 in voter registration and 81:1 in political contributors. The AEA Boards of Editors, who oversee the journals, are 29:1 Democratic:Republican in voter registrations and 47:1 in political contributors. The AEA officers and editors are 8:1 in voter registrations and 67:1 in political contributors, while the authors they publish are 10:1 in registrations and 49:1 in political contributions.

The upper echelons of the AEA control the organization. They construct slates of candidates for AEA offices, appoint journal editors, organize the conference program, and so on. Mere members have little power.

In this paper I draw an essential distinction between AEA members and AEA ‘players.’ There is no selection in mere membership apart from self-selection. Anyone can join, and some Republican voters do join. The players, however, are elevated in one way or another by the organization. The player categories are officers, editors, authors, book reviewers, and acknowledgees (those thanked in published acknowledgments). The AEA is its players.

The players are largely devoid of Republicans. For AEA players, the Democratic:Republican ratio is, as stated above, 13:1 in voter registration and 81:1 in political contributions. The dominant professional association of economists in the United States is dominated by people on the political left.

Fourteen years ago, William McEachern (2006) published a study of AEA economists’ federal political contributions, finding a D:R contributor ratio of 5:1 among the AEA membership and 18:1 for AEA players. McEachern’s careful study revealed a hierarchical effect. For editors of American Economic Review, Journal of Economic Literature, and Journal of Economic Perspectives, the contributor ratios were 9:0, 5:0, and 2:0 respectively. For authors, the ratios were 9.5:1, 24:0, and 20:1 respectively. For authors in the conference journal Papers and Proceedings the ratio was 32:1. It is well established that the more highly ranked a university is, the more leftist its professors are (see Langbert et al. 2016, 441–442).

One question mark hung over McEachern’s findings: Are political contributions really telling? It is only a minority who make political contributions. It is known that the D:R ratio for federal political contributors is more skewed.

2. I computed the ratio for players in McEachern’s (2006) paper by summing the following numerators and denominators of the ratios he describes: editors, 9:0; authors, 19:2; Papers and Proceedings leadership, 7:1; Papers and Proceedings authors, 32:1; JEL editors, 5:0; JEL referees, 16:0; JEL authors, 24:0; JEP editors, 2:0; JEP advisory board members, 12:0; JEP authors, 20:1; acknowledges, 11:0; search committee members, 7:1; nominating committee members, 4:0; presidents, 5:2; top editors, 5:0; EC members, 4:2.

3. The reason for the discrepancy between registration and contribution ratios is unclear. One possibility is that contributions information is easily accessible online, and Republican economists are apprehensive about being canceled and excluded (including from the AEA journals and programs). Another is that academic Democrats are more enthusiastic than are academic Republicans. Another is the recent inclusion of contributions below $200 in the FEC database. Perhaps Democrats are more likely to contribute small amounts.
than that for voter registrations (e.g., Langbert and Stevens 2020). I update McEachern’s work but also provide data on an indicator that was not available when McEachern did his study and that largely removes any uncertainty: voter registration. McEachern’s results showed left domination of the AEA, but my results show that it has pretty much topped out, and the voter-registration data leaves little real doubt.

There are also imbalances for AEA members who work outside academia, in industry or government, but there is more ideological diversity among those who are interested enough in economics to join the AEA than among players such as those who publish in the AEA journals, a mostly one-party domain since McEachern’s study.

Data and methods

In 2006, in response to the Help America Vote Act, states began to publish voter registration data. Now, 30 states plus the District of Columbia have published registration information. I have obtained the registration data from the proprietary Voter Lists Online database of Aristotle, Inc. (link). As for political contributions, the Federal Election Commission (FEC) makes available federal contributions on its publicly available online database. It has broadened its reporting so that contributions below $200 are now available; in 2006, when McEachern published his study, the FEC listed only contributions above $200.

From late 2019 through August 2020, I collected data on the voter registration and federal political contributions of 6,066 person-roles: members, officers-editors, authors, acknowledgees, or editorial board members of the American Economic Association. There are, of course, accuracy issues. First, there are recording errors within the databases. Second, with respect to federal contributions, individuals’ names and employers may not be spelled accurately, so they are difficult to find, and nicknames like ‘Bob’ for ‘Robert’ or abbreviations like ‘univ’ for ‘university’ also make identification difficult. Third, with respect to registration data, multiple individuals with the same name in a given state can make identification difficult as can changes of address. The inclusion of employer and amounts. However, limiting my data to those who donate to either party in excess of $200, I checked the ratio of Democratic to Republican contributions: The resulting D:R ratio is 9.7:1, 80 percent greater than the ratio that McEachern found.

4. In their sample of 12,372 professors in eight disciplines in leading colleges in 30 states plus D.C., Langbert and Stevens (2020) find a voter registration ratio for economists of 3:1 but a contributor ratio of 17:1. There are almost no Republican contributions.

5. See the FEC website (link).
place-of-residence information in the FEC database and of place of residence, middle initials, and date of birth in the Voter Lists Online database facilitate identification and help to minimize the error rates.

Approximately 74 percent (16,000) of the AEA’s active members are listed in the association’s online directory while 26 percent (5,700) are not. Of those listed, however, many are students who are temporary U.S. residents. I limited my sample to the 30 states, plus Washington, D.C., for which there is registration data (see, e.g., Langbert, Quain, and Klein 2016, 423 n.4), and I excluded visiting students. Following McEachern’s research strategy I randomly sampled approximately 2,000 members (2,028, to be exact) or about one-eighth of those listed in the AEA directory. I used an online random number table to select an initial name in the directory for each state and then took every eighth name thereafter, discarding visiting students. As well, I determined whether the member is an academic, works in industry, works for government, or is a student. It is unclear whether the exclusion of those AEA members who elect not to be listed in the directory biases the sample.

In developing my data, I counted a human being multiple times for multiple roles. Thus, if someone is an editor in two journals, the person counts twice as an editor, or if a person publishes three articles, he or she counts three times as an author. As I explain below, I examined the effect of repeating individuals, and it is negligible. Hence, I look at person-roles rather than individuals. The method is fair because it shows accurately who has influence in the AEA.

**AEA members**

For voter registration, the Democratic-to-Republican ratio is 807:214 or 3.8:1 while for federal political contributors it is 336:26 or 12.9:1. The latter is more than double the contributor rate that McEachern found 14 years ago.

By type of employment, the registration ratios are 4.1:1 for academics (466:113), 6.1:1 for government workers, 2.5:1 for economists who work in

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7. I included in ‘industry’ those who work for not-for-profits, and I included in ‘government’ those who work for quasi-governmental organizations such as the World Bank and the Federal Reserve Bank.
8. A voter-registration study of professors in the social sciences in leading research universities (Langbert, Quain, and Klein 2016) found a Democratic to Republican ratio among economists of 4.5:1; a study of professors in leading liberal arts colleges (Langbert 2018) found a ratio of 5.5:1; a study of professors in field of industrial relations (Langbert 2016) found a ratio of 10:1. Using a broader sample of economics professors in leading colleges in each of the 30 states that publish registration information, Langbert and Stevens (2020) found a ratio of about 3:1.
industry, and 8.7:1 for students. For federal political contributors, the ratios are 17.8:1 for academics, 7.3:1 for government workers, 7.1:1 for economists who work in industry, and 20:0 for students.

<table>
<thead>
<tr>
<th>TABLE 1. Partisan affiliation of AEA members</th>
</tr>
</thead>
<tbody>
<tr>
<td>Party</td>
</tr>
<tr>
<td>-------</td>
</tr>
<tr>
<td>Democratic</td>
</tr>
<tr>
<td>Republican</td>
</tr>
<tr>
<td>D:R ratio</td>
</tr>
<tr>
<td>Registered, not affiliated/Contributed to both parties</td>
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<tr>
<td>Not registered/No contribution</td>
</tr>
<tr>
<td>Minor parties/Nonpartisan PACs</td>
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<tr>
<td>Moved from disclosure to nondisclosure state</td>
</tr>
<tr>
<td>Total</td>
</tr>
</tbody>
</table>

Notes: Minor-party registration among AEA members is as follows: American Independent, 2; Conservative, 2; Green, 2; Independence, 4; Independent, 2; Libertarian, 2; Working Families, 1. If Libertarians and Conservatives were added to the Republican total and Greens and Working Families to the Democratic total, the ‘left:right’ ratio of voter registrations would be 810:238 or 3.4:1. For AEA players, minor-party registration is limited to 17 Libertarian voters (12 of which are acknowledgees) and five Independent voters.

Of the 2,028 members, 469 or 23 percent are registered but unaffiliated and 502 of 2,028 or 25 percent are unregistered. Let’s call the unaffiliated and unregistered the ‘Uns.’ The Uns rate is higher for players because while I sampled members only from the 30 states plus D.C. that provide registration information and excluded foreign students I included all players, who are based globally, in the players’ samples. For instance, of 1,795 author person-roles, 391 (21.8 percent) are based in the U.S. but unregistered while 860 (47.9 percent) are in states that do not collect registration information or are not based in the United States.

One might imagine that among the Uns there are sizable pockets of economists who vote Republican (or Libertarian). I admit that possibility, to some extent: The high rates of Uns lend some uncertainty to my findings. However, the various studies, including studies that ask respondents how they vote, lead us to conclude that the Uns cannot much alter the picture, because the various investigations seem to confirm similar results (see the discussion in Langbert, Quain, and Klein 2016, 433–435). For example, the contributions data of the present study suggest virtually no Republicans.

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9. I could not identify the employers of 115 of the 2,208 members whom I sampled.
TABLE 2. Partisan affiliation of AEA members by employment
(VR = voter registration; Con = political contributors)

<table>
<thead>
<tr>
<th>Party/Employment</th>
<th>Academic</th>
<th>Government</th>
<th>Industry</th>
<th>Student</th>
<th>No employment information</th>
<th>Total</th>
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</thead>
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<td></td>
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<td>Con</td>
<td>VR</td>
<td>Con</td>
<td>VR</td>
<td>Con</td>
</tr>
<tr>
<td>Democratic</td>
<td>466</td>
<td>196</td>
<td>79</td>
<td>29</td>
<td>170</td>
<td>78</td>
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<tr>
<td>Republican</td>
<td>113</td>
<td>11</td>
<td>13</td>
<td>4</td>
<td>68</td>
<td>11</td>
</tr>
<tr>
<td>D:R ratio</td>
<td>4.1:1</td>
<td>17.8:1</td>
<td>6.1:1</td>
<td>7.3:1</td>
<td>2.3:1</td>
<td>7.1:1</td>
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<td>Minor party/Nonpartisan PACs</td>
<td>20</td>
<td>1</td>
<td>3</td>
<td>0</td>
<td>8</td>
<td>12</td>
</tr>
<tr>
<td>Registered, not affiliated; Donor to both parties</td>
<td>273</td>
<td>1</td>
<td>29</td>
<td>0</td>
<td>98</td>
<td>2</td>
</tr>
<tr>
<td>Not registered; Not a donor</td>
<td>232</td>
<td>895</td>
<td>74</td>
<td>166</td>
<td>88</td>
<td>329</td>
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<tr>
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<td>-</td>
<td>1</td>
<td>-</td>
<td>0</td>
<td>-</td>
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<tr>
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<td>1104</td>
<td>199</td>
<td>199</td>
<td>432</td>
<td>432</td>
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</tbody>
</table>

Note: The numbers in the “Not in disclosure state or not in U.S.” row are minimal because I preemptively excluded members whose 2019–2020 membership information indicated residence in a nondisclosure state or outside the United States. In the analogous tables that follow in this paper, (current) residence information does not accompany the person’s name, and I report findings in the analogous row of those tables.

AEA officers and editors

I compiled a list from the AEA website of 206 current and former AEA officers and editors going back to 2009. These include past and present presidents, vice presidents, executive committee members, committee members, and editors. For voter registrations among this group, I found a Democratic to Republican ratio of 73:9 or 8.1:1, with 57 residing in the 20 states that do not tabulate registration, 35 not registered, 3 in minor parties, and 29 registered but not affiliated with a party. For contributors to PACs and candidates, the D:R ratio is 67:0, with 139 not making political contributions. There are no Republican, neutral, or minor-party donors among the AEA’s current and recently past officers and editors. With respect to voter registration, the ratio for the officers and editors is more than double the ratio for the membership (8.1:1 versus 3.8:1). In light of McEachern (2006), the contribution data suggest that there has been movement toward the Democratic Party among both members and the officers-editors of the AEA relative to 15 years ago.
**Figure 1.** D:R ratios for federal political contributions among members of the AEA and officers and editors of the AEA, 2006 and 2019

![D:R ratios for federal political contributions among members of the AEA and officers and editors of the AEA, 2006 and 2019](image)

*Source of 2006 data: McEachern 2006.*

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**AEA authors**

I compiled the names of authors in the eight American Economic Association journals for 2018 and 2019. That includes the inaugural issue of *AER: Insights*. I took all of the authors for *American Economic Review* for issues 108:1 to 109:8; *Papers and Proceedings* for volumes 108 and 109; *AEJ: Macroeconomics* for issues 10:1 to 11:3; *AEJ: Microeconomics* for issues 10:1 to 11:4; *AEJ: Applied* for issues 11:1 to 11:4; *AEJ: Policy* for issues 10:1 to 11:3; *Journal of Economic Literature* for issues 56:1 to 57:2; the inaugural issue of *AER: Insights*; and *Journal of Economic Perspectives* 32:1 to 33:3.

*Papers and Proceedings* has the lowest D:R ratio for voter registration—104:14 or 7:4:1—but for political contributors it is 76:2 or 38:1. For *Journal of Economic Perspectives*, it is 7.6:1 for registration and 21:0 for contributors. For *American Economic Review*, it is 73:6 or 12.2:1 for registration and 45:0 for contributors.

Overall, the ratio among AEA authors is 9.7:1 for voter registration and 49:1 for political contributors. The 9.7:1 ratio is not far from the 11:1 ratio that I found for members of the Labor and Employment Relations Association (Langbert 2016).

The inaugural issue of *AER: Insights* is likely to have inchoate standards that early contributors will define. I find no Republican authors or editors associated with the inaugural issue.

Figure 2 compares D:R ratios for registration and contributions of academic AER members (see Table 2) and authors.
Effects of person-role duplication among officers, editors, and authors

Part of the explanation for the imbalance in partisan affiliation may be supposed to be the presence of a few highly motivated and productive academics who skew the rolls of officers and authors. To check, I used SAS’s Nodupkey option to remove the extra observations resulting from a person having multiple roles or multiple publications. There are 142 unique officer or editor individuals and 1,433 unique authors. The resulting D:R ratios for unique officer or editor individuals are 8.7:1 for voter registration and 42:0 for political contributors (versus 8.1:1 and 67:0). The D:R ratios for unique authors are 10.1:1 for registration and 48:1 for contributors (versus 9.7:1 and 49:1).

Acknowledgees and the informal organization

Organizational influence is often informal (Barnard 1968), and academic scholars recognize informal group dynamics by naming or acknowledging individuals who assist them in preparing a publication. The acknowledgees are often more senior academics who have guided the author, but they can also be students who labored on the scholar’s behalf or colleagues who read the paper during its preparation phase. Characteristics of acknowledgees may give hints as to the nature of influence networks and organizational politics.
Because many authors in the AEA journals give long lists of acknowledgees, I attempted to sample just enough articles in each issue so that the number of acknowledgees I collected roughly equaled the number of authors. I did this by picking articles from each issue using a random number generator until the number of acknowledgees approximated the number of authors.

With respect to registration, the D:R ratios for acknowledgees are 8.5:1 for AEJ: Macro, 39:1 for AEJ Micro, 25:0 for AEJ: Policy, 11.9:1 for AER, 1:0 for the inaugural issue of Insights, 20:0 for JEL, 18.6:1 for Papers and Proceedings, and 17.3:1 on average. I could not find acknowledgees in the issues of Journal of Economic Perspectives that I reviewed.

**Book reviewers and boards of editors**

The Journal of Economic Literature publishes book reviews. While book reviews garner reviewers at most a modest amount of publication credit toward tenure and other personnel decisions, reviews can impact the recognition the book author receives. The characteristics of book reviewers and boards of editors, who review papers, are likely to bias the kind of findings that are deemed worthy of publication (Langbert 2016). Of 37 book reviews in the Journal of Economic Literature, eight were written by registered Democrats; two were written by contributors to Democratic Party PACs and candidates; none were written by registered Republicans or contributors to Republican Party PACs and candidates.

The sample of book reviewers is small, but editorial boards, who serve as parallel gatekeepers for publication of articles in the journals, present a similar pattern.

AEJ: Macro has two registered Republicans and one Republican contributor ratio on its board of editors so that the registration and contributor ratios are 3:2 and 2:1 respectively. There are no registered Republicans or Republican donors on any of the other editorial boards, so the mean ratios are 28.5:1 for registration and 47:1 for contributors.

Figure 3 summarizes my voter-registration findings. There are 6,066 person-roles in the study. The D:R ratios for the political registration of the different categories are 73:9 for the AEA’s officers and editors, 363:21 for acknowledgees, 301:31 for journal authors, 8:0 for book reviewers, 57:2 for members of the editorial boards, 466:113 for academic members, and 807:214 for members. For players overall, it is 12.7:1. The left-leaning culture of the AEA is not much moderated by Libertarians or other third-party voters. For AEA players, minority registration is limited to 17 Libertarian Party members (12 of whom are acknowledgees) and five Independents.
Figure 3. D:R voter-registration ratios

Source of U.S. population ratio: Gallup, June 4, 2020 (link).

Figure 4 summarizes my party-contributions findings. The D:R ratios for political contributors in the different categories are 67:0 for the AEA’s officers-editors, 126.5:1 for acknowledgees, 49:1 for journal authors, 2:0 for book reviewers, 47:1 for members of the editorial boards, 17.8:1 for academic members, and 336:26 for members. For players overall, it is 80.7:1.

Figure 4. D:R contribution ratios


Conclusion

What are the implications and ramifications of my findings? I leave that to the reader and future discourse, except to say that my findings should put to rest any myth that professional economics in the United States does not lean to the left. It is like the rest of academe, just somewhat less extreme.
### Table 3. Partisan affiliations of authors in AEA journals

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<td>Con</td>
<td>VR</td>
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<td>VR</td>
<td>Con</td>
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### Table 4. Partisan affiliations of acknowledgees in seven AEA journals

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## TABLE 5. Partisan affiliations of AEA editorial boards

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Mitchell Langbert is an associate professor of business at Brooklyn College. He holds an MBA from UCLA, an MBA from the St. John’s University School of Risk and Insurance (formerly the College of Insurance), and a Ph.D. in industrial relations from the Columbia University Graduate School of Business. His research has been published in journals such as Human Resource Management Journal, Journal of Business Ethics, Academy of Management Learning and Education Journal, Benefits Quarterly, Journal of Labor Research, and Journal of Economic Issues. At Brooklyn College he teaches managerial skills, human resources, and business writing. His email address is mlangbert@hvc.rr.com.

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Mitchell Langbert is an associate professor of business at Brooklyn College. He holds an MBA from UCLA, an MBA from the St. John’s University School of Risk and Insurance (formerly the College of Insurance), and a Ph.D. in industrial relations from the Columbia University Graduate School of Business. His research has been published in journals such as Human Resource Management Journal, Journal of Business Ethics, Academy of Management Learning and Education Journal, Benefits Quarterly, Journal of Labor Research, and Journal of Economic Issues. At Brooklyn College he teaches managerial skills, human resources, and business writing. His email address is mlangbert@hvc.rr.com.

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Go to archive of Character Issues section
Go to September 2020 issue
Brazil is the largest country in South America and the fifth-largest in the world by area, sixth by population. It is noted for its soccer and popular celebrations. It is also known for its undeniable problems. And it is one of the most closed and regulated economies on the planet. The Economic Freedom Index published by the Fraser Institute rates 162 countries for the year 2017. In that listing, Brazil comes in 120th place (Gwartney et al. 2019, 9).

Brazil was under Portugal’s power for centuries, becoming a united kingdom under a traditional monarchy in 1815. It became a constitutional monarchy after its independence in 1822; a republic controlled by oligarchies after a coup d’état in 1889; a dictatorship in the 1930s; it tried to maintain a constitutional democracy between 1946 and 1964; and it was a military regime between 1964 and 1985. Then the current democratic period began, called the New Republic. Regardless of the period, the same kinds of challenges were experienced, varying only by degree. Economic problems like inflation, public debt, and excessive regulation are a constant in Brazilian history, appearing in all of its historical periods and political regimes.

Brazilian intellectuals have blamed Brazil’s troubles on several cultural characteristics. One of them is the prevalence of patrimonialism, a system “in which the leader organizes his political power as the exercise of his domestic management” (Medard 1991, 326). Brazil’s history is marked by many military coups, dictatorial cycles, and chronic economic statism. Nonetheless, a liberal tradition has never ceased completely. Brazil never fully succumbed to totalitarianism. In fact, it is possible to say, according to the writer Euclides da Cunha (1866–1909), that

1. Instituto Liberal, Rio de Janeiro, RJ 20031-010 Brazil.
2. Patrimonialism is a concept from the sociologist Max Weber (1864–1920). Unlike feudalism, this system is marked by centralization. Its origins in Brazil are related to Portugal, where, according to authors like Raymundo Faoro, feudalism didn’t exist.
liberalism led to the founding of Brazil as an independent nation by inspiring many of the leaders of Brazilian emancipation from Portugal (Macedo 1995, 117). Today liberalism continues to influence Brazil’s leaders.

This article provides a brief summary of the presence of liberalism in Brazil from the period immediately before its independence in 1822 to the present day. I explore at length the history of politics and liberalism, highlighting important liberals and then providing an overview of the main personalities and institutions currently dedicated to defense of liberal thought.

**Cultural challenges for liberalism in Brazil**

Before pondering the Brazilian liberal tradition, it’s worth mentioning some studies by famous Brazilian social commentators outlining the obstacles Brazil’s national culture puts in the way of effective liberalism. Among such commentators, the sociologist Gilberto Freyre (1900–1987), author of the classic *The Masters and The Slaves (Casa Grande & Senzala)*, occupies a distinguished position. His work identified the relationships among the main groups that initially constituted Brazil—the Portuguese, the Indigenous, and the Blacks, all part of a “landowning monoculture” (Freyre 1990, 12)—as the most significant foundation of Brazil’s social structure.

According to Freyre, Brazil's social arrangement was marked by a patriarchal organization, which subjected slaves, women and children to the authority of the owner of the land, although the landowner was himself subordinate to the Crown. The family was the “colonizing unity” (“unidad colonizadora”) of the country (Freyre 1990, 14). Within the latifundium system, the family configured a social complex that mixed popular Catholicism, nepotism, and polygamous patriarchalism.

Many of the intellectuals before and after Freyre saw numerous obstacles to advancing self-government as in the U.S. tradition. These included the large portions of land controlled by a few strong authorities with an intensely subservient hierarchy, despite the incomparable blending that resulted in the immense diversity of the Brazilian people.

On the other hand, authors like Raymundo Faoro (1925–2003) modernized Freyre’s thesis without necessarily denying it. Faoro emphasized that the manorial patriarchalism of the Brazilian colonial era exhibited subordination to the Portuguese metropolis, in other words, to a central power. That subordination increased in degree when the old administrative model of dividing the territory into hereditary captaincies and *sesmarias* gave way to governments located in the colony itself.

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3. System adopted by Portugal to promote settlement by donating land.
The central aspect of Faoro’s criticism, which appeared in his book *The Owners of Power (Os Donos do Poder)*, is that Brazil has a patrimonialist heritage, manifested throughout its economic history by the prevalence of state capitalism or politically oriented capitalism. Economic and commercial practices were to a significant extent linked to a bureaucratic social stratum embedded in the body of the state, which derives in part from the absence of feudalism since the time of Brazil’s connection with Portugal.

For authors like Faoro, the absence of feudalism meant that the nobility and the bourgeoisie never had “enough power to counter the unquestionable power of the monarch. Thus, the nobles, more than a tradition that was independent of the Crown, received the prestige from the Crown, being practically employees of the prince” (Rodríguez 2006, 42). The consequence was the formation of “a State stronger than its society, in which the centripetal power of the king, in the colonial period, and of the emperor, throughout the 19th century, or the executive, in the republican period, created a strong bureaucratic apparatus based on the feeling of personal fidelity” (ibid., 77). Hence, in Faoro’s view, “the bureaucratic status, founded on the patrimonial system of politically oriented capitalism, acquired the aristocratic content of the nobility of the robe and the title. The pressure of liberal and democratic ideology did not break, dilute or undo political patronage over the nation” (Faoro 2000, 379).

Dialoguing with Freyre’s view, the sociologist Sérgio Buarque de Holanda (1902–1982), in his work *Roots of Brazil (Raízes do Brasil)*, argues that there was a long-term development of a “factional spirit” in politics, stimulated by the authority of the landowner, and which was increased by the family and the relational mentality associated with the patrimonialist system. Such development seems to have removed Brazil away from the liberal principles of impersonality and separation between private interest and the state machine. The anthropologist Roberto DaMatta (b. 1936), in his turn, sees in Brazilian culture a synthesis of aristocratic values and tribalist ones, as well as some elements of modernity. All of this together hinders the absolute imposition of totalitarian proposals, but also encourages nationalization of economic affairs and obstructs the liberal agenda.

**The legacy of Pombal**

In addition to these theories, there is a specific historical fact that, according to some authors, reinforces the anti-liberal conditions existing in Brazilian culture. It is the political procedure of Sebastião José de Carvalho e Melo (1699–1782), the Marquis of Pombal, a Portuguese reformist leader during the reign of Dom João
According to Antonio Paim, Pombal absorbed from the British cultural and political example the importance of science for progress, but he did not appropriate the liberal-representative tradition developed in England. For Paim, unlike Faoro, this option historically taken by Pombal suggests that there need be no fatalism in Brazilian political and economic history and that this patrimonialist root can be modified if choices are changed.

Pombal’s policies meant, for both Portugal and Brazil, emphasis on a mercantilist model that supported politically oriented capitalism and the patrimonialist system mentioned above. At the same time, it gave a scientific and modernizing justification for this system, which would be explored by authoritarian and anti-liberal movements. Positivism would come to be touted as the supportive theoretical basis of the twentieth-century Vargas dictatorship. As Paim points out, “the modernization carried out by Pombal did not include the reform of political institutions. These remained attached to the monarchical absolutism” (Paim 2018, 33). The Pombaline adherence to mercantilism meant that Brazilian modernization devalued profit and wealth in general and was hostile to private entrepreneurs. This “gave to the state bureaucracy a great supremacy over the other social groups. The Portuguese State, which was typically a patrimonial State—in other words, part of the Prince’s patrimony, and not an organ devoted to serve the society—started to attribute the role of promoting modernization (predominantly economic) as something that should benefit that same bureaucracy” (Paim 2018, 34).

Short of men and resources while warring variously with the Netherlands, Spain, and France, Portugal banned for centuries the development of industries and use of the printing press in the colonies. Brazilian liberalism has had to deal with that legacy, obviously one full of unfavorable aspects. However, Pombal developed a certain economic modernization, albeit subject to significant criticism, as well as helped form an educated Brazilian elite who attended Portugal’s University of Coimbra after his educational reforms. These created conditions for national emancipation and the emergence of the first theoretical agenda of Brazilian liberals: the installation of a representative political system. Brazilian liberals initially dedicated themselves to installing such a system, drawing inspiration from Benjamin Constant (1767–1830), François Guizot (1787–1874), and others.

4. Dom João V, also known as Dom John V, or John V of Portugal, was a monarch of the House of Braganza, who ruled as king of Portugal during the first half of the 18th century. Dom is an honorific designation for a noble or a person of distinction.
Liberals up to 1822 independence

Liberal ideas penetrated Brazil before independence, which liberals supported. Through the secret work of Freemasonry in Brazil and Portugal at the end of the 18th century, Enlightenment thinkers with liberal tendencies influenced the nativist movements that broke out in different regions, such as the "Minas Conspiracy" (Inconfidência Mineira) in Minas Gerais, whose leader, Tiradentes (1746-1792), would later be treated by the republic as a national hero. Their doctrinal and proposed formulations, however, were somewhat diffused and did not have a significant impact on later political tradition.

Frei Caneca

An important figure is Joaquim do Amor Divino Rabelo e Caneca (1774–1825), known as Frei ('friar') Caneca. He was an intrepid pamphleteer from Recife, capital of Pernambuco, in the Northeast of Brazil. He associated himself much more with the democratic radicalism of Jean-Jacques Rousseau (1712–1778) than with the Scottish Enlightenment thinkers or the foundations of the British monarchy. Caneca stood side by side with the priests of the Seminary of Olinda, although his ordination was prior to the foundation of the seminary. They were immensely influenced by the Pombaline reforms, and they sought to unite religion with faith in science and its potential to promote progress. Paradoxically, the Brazilian Rousseauians in the 18th century, counterparts to the French who stoked the Revolution and in its most radical stages attacked the Church, were priests.

For Frei Caneca, each province could “follow the road that seemed better in each way; choose the form of government that seemed most appropriate to its circumstances; and be constituted in the most conducive way to its happiness” (Mug 1875, 55). This doctrine, imbued with Rousseau’s sources, was propitious in provoking revolutions; the victorious independence project was the one that gathered different interests around a political unity over the entire national territory—and it was built around the unifying symbol of the Crown and the emblematic figure of Dom Pedro I (1798–1834). 5

Caneca participated in the Pernambucan Revolt of 1817. Later, after independence, he was also part of the 1824 Confederation of the Equator, which was another insurgency that occurred in the Northeast of Brazil. In January 1825,

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5. Dom Pedro I, also known as Pedro I of Brazil, or Pedro IV of Portugal, was the founder and first ruler of the Empire of Brazil after independence from Portugal.
he was executed because of the 1817 Revolt. He defended his political platform in a newspaper called Typhis Pernambucano. The platform reflected regionalist ideals rather than a national purpose, as Frei Caneca favored the independence of the Northeast of Brazil more than the emancipationist project around the Crown in Rio de Janeiro. For him, the provinces should have had total autonomy and even been able to secede from the constitutional monarchy pact in order to found independent republics.

**Hipólito José da Costa**

Even before the independence process in Brazil, the English liberal formulation also influenced Portuguese American leaders. One of them was Hipólito José da Costa (1774–1823), who was born in the extreme south of Brazil. He lived in London and from there edited a famous newspaper called Correio Braziliense. He managed to prevail over the government censorship in Brazil and share the ideas that underpinned the British constitutional monarchy. Hipólito belonged to the elite formed at the University of Coimbra in the wake of the Pombaline reforms. Not only did he carry out important work of doctrinal and historical foundation for classical liberal thought in Brazil and Portugal, but he was a pioneer in defending these ideas within Portuguese political traditions. In Hipólito’s conception, the Portuguese monarchy had a legitimacy that went back to the founder of the nationality, Afonso Henriques (ca. 1109–1185), chosen by the Cortes de Lamego. He even pointed out an alleged superiority of the Portuguese crown over the legitimacy of the British monarchy. The latter in the course of its evolution only surpassed the Portuguese by ceasing to be an “absolute hereditary monarchy” (Paim 2019, 21) to become a mixed monarchy. Hipólito wished to avoid the disorder of the revolutions and the uproar he saw in Rousseauian thought. He endorsed the tradition of liberalism classically intertwined with the establishment of a government capable of perpetuating itself.

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6. Cortes de Lamego designates an assembly that is said to have met in Lamego and to have founded Portugal, and originated legal norms that would mark the history of Portugal.
Silvestre Pinheiro Ferreira

Before independence, which Hipólito supported at the end, as he died just at the beginning of the history of the Brazilian Empire, two more names should be mentioned. One is Silvestre Pinheiro Ferreira (1769–1846). Although he was born in Lisbon and was not a Brazilian by birth, Ferreira was a prominent figure in the government of the king of Portugal and Brazil, Dom João VI (1767–1826), father of Dom Pedro I. Shortly before Ferreira’s presence in a prominent position in the reign, the Porto Revolution in 1820 had broken out in Portugal. The revolution demanded the implementation of liberal and constitutionalist reforms, and also ended up with the return of the king, who was, until then, a refugee in Brazil, having escaped from the invasion of the Napoleonic troops in Portugal in 1808. At a very delicate moment of political tension, Ferreira theorized a transition from the traditional monarchy to the constitutional monarchy. His work, called Citizen’s Manual in a Representative Government (Manual do Cidadão em um Governo Representativo) was extremely influential in 19th-century Brazil in supporting the construction of the institutions of the Imperial State. Ferreira said:

If the jurists had evaluated the importance of this observation, they would have concluded without hesitation that the jurisprudence on representation cannot be other than that of the mandate. When it came to establish the rights and duties of the agents or representatives, whatever they were, it was in the nature of the interests that the reasons should be sought; but, losing sight of this simple idea or omitting the word “interests” and preserving that of a person, they fell into serious errors, especially when dealing with constitutional law and the rights and duties of diplomatic agents. (Ferreira quoted in Paim 2018, 73)

Ferreira said that a representative system—the main goal of Brazilian liberals in the transformation of the Portuguese monarchical regime into an independent constitutional monarchy—should be based on the representation of different interests in the parliamentary body. The emphasis on this theory was one of the factors that allowed, despite the cultural challenges of antiliberal content already mentioned, considerable political stability in the Second Reign of the Brazilian Empire, in which interests started to be represented by political parties.

7. Dom João VI, also known as John VI of Portugal, was king of the United Kingdom of Portugal, Brazil, and the Algarves from 1816 to 1825.
Viscount of Cairu

Finally, José da Silva Lisboa (1756–1835), the Viscount of Cairu, is worth mentioning. Born in the city of Salvador, Cairu is renowned as the first Brazilian economist. By the age of eight he already knew Latin grammar, dedicating himself to philosophical studies and studying music and piano. He was then sent to the University of Coimbra, to take legal and philosophical courses. Deeply religious, he studied the Scriptures and learned about scholastic thought. He became, as he always would remain, a forceful critic of Freemasonry, which he saw as a hotbed of revolutionary thought, and which had great influence in Brazil.

Cairu became secretary of the Inspection Bureau in the state of Bahia, responsible for inspecting and supervising everything that concerned the economic life of the region, particularly classifying sugars and tobacco. At the end of his life, Cairu demonstrated that he did not approve the existence of this type of public agency, with its interference in the economy. He pointed out in the Senate on May 10, 1827, that the bad situation of agriculture “induced El-rei Dom José, in the ministry of the famous Carvalho, to create the Inspection Tables at the main ports of this State, taking care that in this way he would repair the damages, imposing fees and sanctioning abuses” (Belchior 2000, 35).

Documents from the old archives of the Overseas Council in Portugal reveal that Cairu defended the awarding of prizes to inventors and introducers of new machines and factories, demonstrating his enthusiasm for Brazilian economic and technical development. In an official letter of March 28, 1799, Cairu proposed to award Joaquim Ignácio da Sequeira Bulcão, future Barão de São Francisco, for having been “the first owner who had adopted new cane crushing processes” (Belchior 2000, 33). In 1815, he assumed the role of supervising the works that were to be printed, a position in which he basically acted as a censor, given that he and his peers feared what they considered “incendiary writings.” Over time he became more accustomed to the English spirit in this regard, having praised British press freedom as a factor allowing the development of brilliant minds.

In the last decade of the 18th century Cairu became aware of the works of Adam Smith (1723–1790), and later those of Jean-Baptiste Say (1767–1832), which initiated the richest phase of his intellectual trajectory. Cairu’s conservative temperament reacted with reservations to the Liberal Revolution of Porto in 1820, which demanded the containment of the king’s powers. He feared a French-style development, which he considered the worst possible outcome. Later, having

8. “El-rei” Dom José is the king Joseph I of Portugal.
9. Barão de São Francisco (Baron of São Francisco) was a Brazilian title created by Dom Pedro II for Joaquim Ignácio de Siqueira Bulcão.
defended the maintenance of the union between Brazil and Portugal, he agreed with the widespread Brazilian perception that the Portuguese intended to adopt anti-Brazilian measures and worked for the foreign recognition of the new free country. As a congressman in the Empire, he fulminated against federalism and was opposed to religious freedom, and he also made certain abridgements of his economic liberalism—he supported the existence of trade guilds in the country, understanding them as restricted and capable of improving the arts. At the same time, Cairu also advocated a more just and humane treatment of slaves.

Many authors point out that Cairu’s theoretical influence encouraged Dom João VI to open Brazilian ports to trade with friendly nations, a milestone for Brazilian defenders of economic liberalism. Despite the apparent paradoxes of his mentality, he brought Smith’s economic ideas and the political thought of Edmund Burke (1729–1797) to Brazil. Cairu’s contradictions can mostly be justified by understanding that, under the influence of the philosophy of eclecticism, a French import that guided philosophical orthodoxy during the Empire, Cairu devised a synthesis among a variety of forces: classical economics and Smith’s liberal thought, aspects of state modernization and centralization from the Marquis of Pombal and from the Coimbrã instruction, and conceptual and rhetorical references of feudal and traditionalist atmosphere, echoing the sacredness of the Catholic religion and the emperor. His mentality was an understandable amalgam in a changing world and society, but all of it was touched by a key ingredient: the work of Burke, which Cairu translated into Portuguese, composing the famous Extracts of Edmund Burke’s Political and Economic Works (Extratos das Obras Políticas e Econômicas de Edmund Burke).

The penetration of Burkean temperance among the Brazilian elite and its introduction into the formal education of the future monarch of the independent country, based on Cairu’s efforts, demonstrated that the British tradition was not alien to the educators of Brazil. In fact, the constituents of the new nation, for the most part, wanted a foundation to be established that would provide cohesion and some degree of predictability in rules and relationships, indispensable to civilization, as Burke propounded.

10. For more information on eclecticism, see McClellan (2003).
Liberalism at the foundation of the Empire
(1822–1831)

Freemasonry and the Emperor D. Pedro I

Brazil’s independence is related to Portugal’s Liberal Revolution of Porto in 1820. In previous years, during the reign of Dom João VI, when Portugal was experiencing difficulties, Brazil was the center of the Portuguese Empire, because the Portuguese royal family, during the French invasion of Portugal in 1807, fled across the sea to Brazil, establishing Rio de Janeiro as the de facto capital of the Portuguese Empire for the duration of the Napoleonic Wars. Brazil had been elevated in 1815 to the category of United Kingdom to Portugal, gaining a relative sense of autonomy and unity, while being formally integrated into its former metropolis. Several developments, such as the opening of ports, took place in Rio de Janeiro, especially in the vicinity of the old Court. The Kingdom of Brazil was seen as more prosperous than the Kingdom of Portugal, its older ‘brother.’

The Portuguese liberal revolution forced the return of Dom João to Portugal and a call in 1821 for the so-called Cortes de Lisboa, which would function as a kind of constituent parliament. Upon leaving, Dom João kept his son and successor, Dom Pedro, as prince regent, commanding the kingdom of Brazil. Throughout the process, Brazilian representatives saw the intentions of Portuguese parliamentarians as dissolving any sense of Brazilian unity, fragmenting power among the provinces while still submitting some of them to the government of military junta. Even for Brazilian liberals, sympathetic to the general claims of the Porto Revolution (particularly those linked to Freemasonry in Rio de Janeiro), among whom leaders such as Joaquim Gonzãlves Ledo (1781–1847) and José Clemente Pereira (1787–1854) stood out, these proposals were seen as anti-Brazilian. The main national statesmen, such as the Viscount of Cairu and José Bonifácio de Andrade e Silva (1763–1838), known as the Patriarch of Independence, intended to maintain the union between the two kingdoms. However, the succession of events in the Cortes de Lisboa led them to produce a gradual movement of emancipation around the figure of the prince regent, transforming him into an emperor and converting the Kingdom of Brazil into an independent empire.
It is pertinent to verify the presence of liberal ideas in the circle of personalities that constituted the First Reign of the Brazilian Empire and directed the country to create the first Brazilian Magna Carta, the Constitution of 1824, which was in force until 1889. The central leader would be Dom Pedro I. The first Brazilian monarch was Portuguese, born in the National Palace of Queluz in Sintra, a district of Lisbon, in 1798. Although he was only nine years old when his family travelled to the new country, his firmly Portuguese origin was one cause of the tensions that disturbed his reign until a definitive impasse led to his abdication. Dom Pedro’s personality was considered very willful and, in 1823, he forcibly dissolved the Brazilian Constituent Assembly, from which, nonetheless, general aspects of the constitution emerged. He is usually characterized as a monarch who admitted the inevitability of the liberal agenda—an agenda he referred to generically as ‘new ideas.’

Dom Pedro was influenced by French liberals, such as Benjamin Constant, and the Neapolitan Gaetano Filangieri (1753–1788). Filangieri held that the old European feudal principles were responsible for the misery in Europe, due to the wasteful luxuries of the aristocrats and nobles. The remedy would be a sort of “orderly revolution,” provoked by the authorities themselves, so as to soften the laws and establish justice. The prince was also a reader of Edmund Burke, as well as of Viscount of Cairo, although scholarship is not a characteristic highlighted by his biographers (Sousa 2015, 115).

Dom Pedro recognized that the old monarchical institutions could not survive in the 19th century in the same form as before. The kingdom—be it the Portuguese or the Brazilian Empire—would need to find a new path. It would not completely abolish existing traditions and institutions, nor annihilate the power or the position of the royal family, nor would it cling to a past that was overcome and unforgiving. In the works of liberal authors like Constant, Dom Pedro found what he judged to be the key to the realization of his wishes.

The history of the Brazilian Empire is divided in three different parts. The First Reign (1822–1831) was a brief period of turbulence and tensions, but it also marked the consolidation of independence. It did not, however, definitively consolidate national unity, which would continue to face challenges through the Regency period (1831–1840) until the advent of the last period, called the Second Reign (1840–1889).
The differences between José Bonifácio and the Masonic group of Gonçalves Ledo were common since the beginning of the First Reign, when the latter began to press for an oath from the emperor that he would accept the constitution, whatever it may be, that would arise from the Brazilian Constituent Assembly. It was a situation similar to the discussions that preceded the departure of Dom João when the revolutionaries of Porto showed interest in the creation of a constitution, and forced the king to accept it. However noble the intention in Porto was to establish constitutional rules to limit arbitrariness, it was from that Constituent Assembly that the anti-Brazilian measures that triggered independence emerged. Averse to such risk, Bonifácio did not want the emperor, in face of all the institutional engineering that he considered necessary to consolidate the new country, to be held hostage to a political mentality that he considered abstract and agitating.

Freemason liberals believed that all national sovereignty should be created under the authority of the nation itself. Bonifácio, on the contrary, believed that Brazilian political authority revolved around Dom Pedro, as the symbolic representative of the homeland. Bonifácio, the Patriarch of Independence, thought a sort of pact had already been concluded between the nation and the emperor, which no constituent assembly could threaten, subordinate, or dissolve. The Constituent Assembly and Dom Pedro should have equal strength and legitimacy in the national regime under construction. Liberals could not, for example, proclaim a republic. Constitutional monarchy would already, in itself, be a limit that no member of the Constituent Assembly could question.

After independence, it was no longer a question of discussing the points of consensus between the forces that carried it out; but, instead, their differences. These, added to personal intrigues and gaps in temperament, were more significant than they were previously. We start with the social scope of Bonifácio’s vision. Although at first he did not want an immediate Constituent Assembly, he favored bringing an end to slavery. That was a horizon of thought that the leaders most concerned with institutional formalism, such as Gonçalves Ledo and other opponents of Bonifácio, did not consider. In general they preached the liberalism of the Americans or the English, but not the freedom for human beings whom they considered their property. The political elite was not prepared for the outlook of Bonifácio, who, in this respect, was ahead of many of his contemporaries.

Until the last years of the monarchy, Brazilian political liberalism coexisted with slavery and the country based most of its economic model on it. Gradually, laws would be approved to end this situation, but this contradiction followed Brazilian leaders all that time.
The Constitution of 1824 and the moderator power

Dom Pedro I, still concerned about the constitution that he expected the 1823 assembly to write, pointed out (probably under the influence of Bonifácio):

a wise, just and enforceable Constitution, dictated by reason and not by whim, which only lowers overall happiness, which can never be great, without having solid bases, bases that the wisdom of the years has shown to be true, to give a fair freedom to the peoples and all the necessary strength for the Executive Power. A Constitution in which three powers are well divided, so that they cannot be arrogated, rights that do not contend among each other, but that are organized and harmonized in such a way that will become impossible, even over the course of time, to be dangerous, and that will more and more compete hand in hand for the general happiness of the State. After all, a Constitution, which places barriers inaccessible to despotism, whether real, aristocratic or democratic, chases away anarchy and plants the tree of freedom, in the shadow of which must grow unity and tranquility, independent of the empire, that will become the haunt of the new and the old world. (Dom Pedro I, quoted in Sousa 2015, 464, my translation)

A philosophy that guided the construction of the first Brazilian constitution, embraced by the emperor, was to avoid a too metaphysical or “impracticable” constitution (Sousa 2015, 465), opposing excessive influence of Rousseau’s thought. The resulting constitution established a hereditary monarchy regime and, based on the work of Constant, it established four powers: the executive, the legislative, the judiciary, and the moderator.

The executive power remained in the hands of the emperor, who would appoint his ministers, executors of orders, and the presidents of provinces, who had a charge equivalent to current state governors. Later, in the 1840s during the Second Reign, without changing the constitution, the political system underwent an evolution toward parliamentary bias. The emperor was supposed to appoint a ministerial office under the command of a president of the Council of Ministers, with a function analogous to the prime minister in traditional parliamentary systems. This president would share with the emperor the exercise of the executive power; the ministers were responsible for their acts, while the emperor was legally free of responsibility.

The legislative branch was made up of the Chamber of Deputies and the Senate, with deputies elected by census vote (based on certain economic criteria) and senators appointed by the emperor, chosen from a list of the three with the most votes. The judiciary, by its turn, was also appointed by the emperor, with the judges having a lifelong position, suspended only by sentence or by decision of the
monarch himself. Voting was restricted to men over 25 with high income. At first, there were indirect elections, in which they chose those who voted for deputies and senators; later, the Saraiva law, in the final stretch of the Empire, established direct elections. The candidates, on the other hand, needed to have an even higher income and could not be freed slaves. They also needed to be Catholics, since Catholicism was the country’s official religion. The structure of the Catholic Church, however, was administratively subordinated to the government, in the system known as Padroado, triggering problems at the end of the Empire. Notable among them was ‘the religious question,’ which put Dom Pedro II (1825–1891) at odds with the clergy when he decided to punish priests who banned brotherhoods whose members were Freemasons. The constitution also determined the maintenance of the State Council, an organ composed of direct advisers chosen by the emperor. Rio de Janeiro was maintained as the capital of Brazil.

Regarding the moderator power, it was the object of the most intense discussions throughout the monarchical period. In France, Constant feared that the power of political oligarchies would break the constitutional rules, making it necessary to have a power different from all others to subdue them in the event of a conflagration. Political freedom would need to be constrained in the crisis by an instrument that would protect individual freedoms and the constitution from imminent dissolution. The separation of powers, inspired by the work of Baron de Montesquieu (1689–1755), would have to live with a power superior to the others, which would be that of the constitutional monarchs.

Constant believed that the moderator power was not to be confused with any of the others, nor with the executive; the king would have to be passive, without government, a chief and representative of the state:

The Executive Branch, the Legislative Branch and the Judiciary Branch are three springs that must cooperate, each on their side, with the general movement; but when these springs are disturbed and cross, clash and interlock, a force is needed to put them back in place. This force cannot be in one of the springs, as it would use it to destroy the others. It is necessary that it be outside, that it be neutral, so that its action is necessarily applied in all places where it is necessary that it be applied, and so that it is preservative, reparative, without being hostile. The constitutional monarchy creates this neutral power in the person of the head of state. This chief’s real interest is by no means that one of the powers overthrows the other, but that everyone support each other, understand each other and act in concert. (Constant quoted in Guandalini 2016, 31)

The inspiration of Constant, adapting his concepts and concerns about the French political context to the Brazilian context, is evident in the constitutional text of
Brazil’s monarchy. However, the Brazilian Constitution did not reproduce his concepts faithfully. First, it formally made the monarch the head of the executive branch, even though he only exercised executive power through the ministers. There is a subtle intrusion of a character more active in its function than what Constant would naturally have wanted. At the same time, legal immunity was given, in Brazil, to the person of the monarch and not to the moderating power. Hence, the emperor became legally immune for whatever he decided in the executive sphere; therefore ministers could be punished because they ratify the acts of the executive by signature, but the power-maker, their leader and mentor, the monarch himself, could not be.

**The Regency and the Second Reign (1831–1889)**

**Political groups in the Regency and the ‘moderate liberals’**

The temperament of Dom Pedro I provoked profound resistance in society. Some episodes were striking enough to reduce his popularity, such as the assassination of the Italian-born liberal pamphleteer Líbero Badaró (1798–1830), a staunch critic of the emperor, editor of the newspaper called *Observador Constitucional*. When Dom Pedro I abdicated the throne, on April 7, 1831, under strong pressure, the episode became known among that generation of politicians, including leaders such as Bernardo Pereira de Vasconcellos (1795–1850), Evaristo da Veiga (1799–1837), and Diogo Feijó (1784–1843), as a ‘liberal revolution.’

Despite not having political parties as in modern politics, Brazil was divided into three major political groups during the Regency period (from 1831 to 1840). This was the moment when, waiting for the future monarch Dom Pedro II to reach age 18, Brazil was governed by regents who did not have the moderating power, a turbulent experience in which governance resembled a republic. The first group was the so-called *caramurus* or restorers, who wanted the return of Dom Pedro I to power. With the death of Dom Pedro I, who fought against absolutism in Portugal in his last years, this group disappeared.

The second group was that of the so-called ‘exalted liberals’ or ‘radical liberals,’ like Antônio Borges da Fonseca (1808–1872) and Ezequiel Corrêa dos Santos (1801–1864), who led provincial revolts that eventually assumed separatist positions. Finally, there was the group of ‘moderate liberals,’ whose proposal was to establish liberal institutions of national scope under the aegis of the Brazilian
Crown. That was the original project of the founders of the Empire.

Dom Pedro II ascended to the throne in 1840 with the famous ‘Majority Coup’—the only coup d’état that took place under the imperial constitution—which made him officially emperor before the time prescribed by law. This inaugurated the Second Reign of the Empire. There were about five decades of relative institutional stability, in which the group of the so-called ‘moderate liberals’ dominated and established political orthodoxy.

**Saquaremas and luzias**

During the Second Reign the ‘moderate liberals’ divided themselves into two groups: the *saquarema* (Conservative Party) and the *luzia* (Liberal Party). As it can be pointed out, in general, both conservatives and liberals reflected, in Brazil, different tendencies of liberalism. To clarify the basic difference between them, it is worth noting two iconic examples: Paulino José Soares de Sousa (1807–1866), the Viscount of Uruguai, one of the most relevant saquarema theorists, and Aureliano Tavares Bastos (1839–1875), representative of the luzias.

In his book *Essay on Administrative Law* (*Ensaio sobre o Direito Administrativo*), Uruguai compared the administrative experiences of countries like France, England, and the United States with those of Brazil, a very young nation that needed to be inspired by other models and traditions to construct some of its institutions. He referred to his luzia opponents as adherents of the “so-called liberal opinion” because, in a note, he said, to the likely surprise of some unwary contemporaries, that his position is the one which was truly liberal (Uruguai 1862, 15). The theoretical references that Uruguai cited, which he used without moderation to support his positions, came from French liberalism, ranging from Benjamin Constant and Alexis de Tocqueville (1805–1859) to the French ‘doctrinaires’ like François Guizot. In Uruguai’s opinion, the French institutions of doctrinal liberalism, due to their originality and relative independence from a long historical formation process, were more applicable to the Brazilian socio-political context than British institutions. He developed his arguments by always comparing the constitutional and political thinking in force in other countries with the local legal debate, seeking to support what could and could not apply to the Brazilian context.
In the view of Uruguai and the saquaremas, some institutions clearly established by the Brazilian fundamental law were necessary to the Brazilian social state. These included the Lifetime Senate, the Council of State, the Moderating Power and political centralization, with the possibility of the monarch acting on the provinces through the provincial presidents appointed by the central power.

For Uruguai, the decentralizing reforms applied by the Additional Act of 1834 had made the central legislative branch overly hostage to the decisions taken by the provincial assemblies—created by the Act—hampering the political process. That necessitated the imposition of the Interpretative Law of the Additional Act, formalized in 1841 by the saquaremas. Uruguai also believed that the provinces, wishing for more power, had reduced the strength of the municipalities. The most important point was that the Empire’s elections would be affected by the lack of development of self-government. Above all, the elections would be compromised. Due to the scant communication between the regions, the lack of education of the Brazilians, and the nefarious power of local political influences, any opposing project for the country would effectively cease to exist.

The saquaremas believed that it was necessary to adapt liberal principles to the social, historical, cultural, and geographical circumstances of Brazil. This, in their view, demanded a political centralization capable of avoiding the power of the local oligarchies, guaranteeing the circulation of the parties in power, and protecting the liberties of the opponents. For Uruguai, at that time in history, the freedom of individual citizens—with which, as a liberal, he was concerned—was more threatened by local authoritarianism than by the central government and for this reason the central government should exercise greater scope and power.

On the other hand, luzias like Tavares Bastos were more directly influenced by the American democracy and the English monarchy. They generally defended the extinction of the Council of State and the Lifetime Senate, as well as the need for a ministerial referendum on the exercise of the moderating power. Some moved to republican positions, but the party was, in itself, monarchical. Their main banner was to combat centralization and defend the purity of application of the Additional Act of 1834, considered by them to be one of the most relevant and admirable legal instruments in Brazilian history.

For Bastos, centralization was a backward political practice. In his opinion, the work of the generation that fought Dom Pedro I could only be complete with the confederation of the provinces, the suppression of the Council of State and the union of the attributions of the Moderating Power and the Executive Power. Thus, the monarchy would become an entirely harmless institution. Saquaremas like Uruguai were, in his view, men without faith in the destinies of democracy or in the providential mission of the American continent. Since the beginning of his most important book, *The Province (A Província)*, Tavares had praised the Anglo-
Saxon countries as models that Brazil could aspire to. Also, he urged his party to combat the excessive policy of order and centralization and defend freedom.

Tavares Bastos’s conceptions defended the existence of humanity’s progress toward the greater exercise of freedoms. It also postulated that centralization, political or administrative, represented a nefarious obstacle to such progress. In his point of view, the U.S. exemplified a trend that was manifesting itself around the world toward decentralization. The trend would also advance in countries like France, which was further behind. Also in his view, the Interpretative Law of the Additional Act was a setback, a protest of an archaic mentality against the liberal advances achieved in America itself. For him, the deficiencies of the provinces in terms of transport and postal services, for example, stemmed from the suffocating power of the central state. Contrary to what the saquaremas said, in his view it was the centralization of power that manipulated the elections, preventing real democratic life in the provinces and in the municipalities. To the saquarema argument that the people were uneducated and thus not ready for democratic life, he opposed the argument that these people would remain uneducated under the aegis of a system he considered oppressive.

A real federation, as the United States, could emancipate the spirits and also mobilize the dispositions in Brazil. In Tavares Bastos’s opinion, based on the American example, all Brazilian provinces should adopt the absolute freedom of religion, freedom of the press, the right of assembly, and a sacred respect for private property. A real federation would consist of regions with autonomous political powers and even their own constitutions, without breaking up the union between them.

For Tavares Bastos, the problems observed as a result of the decentralizing franchises granted to the Brazilian provinces stemmed from the lack of experience with the representative system. So, in his opinion, it was saquaremas like Uruguai who had abdicated democratic and liberal evolution due to prejudice against people in the provinces.

Saquaremas understood that the local parties tyrannized the police and justice, so they held that these devices should remain under central control. They believed that the fears of José Bonifácio, who understood that Portuguese America ran the risk of splitting itself into small despotic republics, would be realized unless the empire and the emperor maintained a certain level of centralization. The luzias,
on the opposite side, understood that the central power was tyrannical and held that the provinces should be strengthened.

During most of the Second Reign, these currents of opinion fueled the political dispute, along with provincial and municipal political motivations that did not always reflect the great national debates. The aristocratic regime, the census vote, and the practice of slavery all led to an effort to establish representative institutions based entirely on the liberal tradition of the 19th century. There were anti-liberal traditionalist currents, but they were in the extreme minority.

**Liberals at the end of the Second Reign**

In the last decades of the Empire, saquaremas and luzias were affected by a supraparty movement which advocated the immediate abolition of slavery without payment of compensation to the slaveowners. Among the abolitionists, one luzia who would become a reference for Brazilian liberals, Joaquim Nabuco (1849–1910), stood out. Politically, Nabuco defined himself as an “English liberal.” “When I enter the Chamber,” he said, “I am entirely under the influence of English liberalism, as if limited to Gladstone’s orders; this is essentially the result of my political education: I am an English liberal—with radical affinities, but with whig adherences—in the Brazilian Parliament; this way of defining myself will be exact until the end, because English liberalism, Gladstonian, Macaulayian, will always endure, it will be the unavoidable vassalage of my temperament or political sensitivity” (Nabuco 1998, 177). Freeing slaves, making all the inhabitants of Brazil effectively citizens, became the cause of Nabuco’s life. He went to the Vatican to meet with Pope Pius IX (1792–1878) to ask for a manifesto against slavery. For him, abolition was a social force above the current parties, which envisaged a vast work to be done ahead of a Brazil without slaves.

The central understanding expressed by Nabuco was that slavery was more than an isolated fact in Brazil; it was an essential element of its formation as a society, the driving force of its backwardness and its infamies. According to him, in the United States, slavery resulted in a political division between North and South, a very violent Civil War, and a racial tension so profound that it almost completely encased the people in separated halves. In Brazil, on the contrary, a much greater dose of mixing and assimilation would have embedded slavery itself in the soul of the country with a much longer-lived naturalness.
The reforms that combatted slavery were carried out through gradual legislation, from the ban on trafficking to complete abolition. Most of these reforms were made during saquarema governments, including the last one, the Golden Law, responsible for extinguishing slavery, signed on May 13, 1888, during the ministerial office of João Alfredo (1835–1919), Nabuco’s political opponent, but who received his support in this cause.

Among the liberal monarchists of the end of the Empire, the figure of André Rebouças (1838–1898), an engineer and inventor who was influenced by his readings of Smith and Say, also deserves mention. From a practical point of view, Irineu Evangelista de Sousa (1813–1889), the Viscount of Mauá, was a pioneer in several ventures in Brazil, an enthusiast of free enterprise and the Industrial Revolution.

**Republican liberalism and authoritarian cycles**

**The Old Republic: Positivism and the power of the oligarchies (1889–1930)**

A combination of factors, including the popularization of the positivist philosophy of Auguste Comte (1798–1857) and a growing dissatisfaction in military circles, which started to be directly involved in politics, triggered a military coup on November 15, 1889. The coup deposed the monarchy, exiled the imperial family, and proclaimed a republic as the form of government.

Different currents of opinion shaped the republican elite of that period. Positivists, both orthodox and heterodox, were among them. Paradoxically, despite the purported anti-militaristic spirit of positivism, the doctrine played a prominent role in the mentality of the Brazilian armed forces. At the same time, the military’s meddling in politics led to the development of authoritarian currents such as the so-called Florianist ‘Jacobinism,’ composed of enthusiasts for President Floriano Peixoto (1839–1895). The third current was composed of the Republican liberals, many of them organized around the minority Republican Party after 1870. Despite the fact that the first two republican governments were military and authoritarian, constituting the period that became known as the Republic of the Sword (*República da Espada*), the liberals gained influence and attracted part of the elite of the traditional monarchical parties to integrate the new political class. Both Afonso Pena (1847–1909) and Rodrigues Alves (1848–1919), who became presidents of the Republic, were examples.

The influence of liberal thought proved to be substantial at the first republican constitution of 1891. Absent was any explicit positivism or attempt to
enshrine a dictatorship. Eliminating the titles of nobility and establishing the official separation between the Church and the state, the Republic transformed the provinces into states of a federation—at least nominally. In reality, many of these states, without much of a tradition of autonomy, gradually ceded their powers to the central power (Saldanha 1978, 109). The whole system only worked due to a pact that reinforced the position of the president of the Republic, a pact that was formalized by the policies of the second civil president, Manuel Ferraz de Campos Sales (1841–1913).

A ‘historic republican’—as Brazilian historiography labels one who supported the Republic during the last decades of the Empire or who took part in the coup that installed the regime—and an oligarchic liberal, Campos Sales defended presidentialism, but he had also been philosophically influenced by positivism, as had several liberals of his time. As previously seen, since Silvestre Pinheiro Ferreira, the Brazilian liberals of the monarchy had sustained the parliamentary representative system. Based on the work of Ferreira, in the Empire the parliamentary representative system served the interests embodied in political parties (although they were not exactly the same as modern parties). The positivists imagined that politics is completely subordinated to a scientific basis, which, in their point of view, happens likewise with all the other complex aspects of social life. In the positivist mentality, currents of opinion and political and partisan confrontations were seen as archaic and disorderly behaviors that should be replaced by ‘positive knowledge.’ Therefore, the positivism had weakened Republican liberalism.

Sales assumed the presidency with the aim of pacifying the political system after the Republic of the Sword and after the turbulent civilian government of Prudente de Moraes (1841–1902). For that, he wanted to implement financial measures which were considered unpopular, in order to clean up Brazil’s excessive debt. The solution was an agreement between the presidency and the state oligarchies, leading each to guarantee the position of the others. For that, the president had the support of the oligarchies through state representations in Parliament. Politics and representation took second place to management and its technical purposes. At the same time, arbitrary measures to restrain opponents were institutionalized, such as the Powers Verification Commission (Comissão Verificadora dos Poderes), which simply controlled the inauguration in Parliament of opposition parliamentarians, regardless of the outcome of the elections.

This period following the Republic of the Sword, called the Oligarchic Republic, was marked by the recurrence of emergency rule under a state of exception—a decree by which the president can suspend some liberties and the activity of the legislature and the judiciary—and of electoral fraud and armed interventions in politically weaker states (Paim 2018, 157). There was no party
diversification, the regime being basically a single party, merely regionalized. There was a Republican Party in each state, but it did not offer a clear programmatic differentiation of currents of opinion. Politically the Campos Sales liberalism had positivist influences, and the policies he enacted did not really favor the representative system. Economically, he had the Minister of Finance Joaquim Murtinho (1848–1911), who adopted a policy to contain the issue of currency and renegotiation of foreign debt to resolve the country’s disastrous economic situation. Murtinho was influenced by liberal economic thinking and believed that state intervention would bring more harm than good to society. The next president, Rodrigues Alves, carried out several public works, mainly in the capital of the time, Rio de Janeiro, and he resisted economic interventionism to help large coffee growers. Subsequent governments, in general, adopted this last policy, defined in the meeting known as the Taubaté Agreement, which represented, in reality, another episode of Brazilian patrimonialism.

Rui Barbosa (1849–1923) was probably the most important liberal in the Oligarchic Republic. This is so despite his having supported the republican coup and having had a very questionable performance as minister of finance in the government of Deodoro da Fonseca (1827–1892), when Barbosa adopted a policy of increasing the issuance of currency. But, if liberalism is considered a political movement, he inspired a good part of the Brazilian liberal leaders of the 20th century. Rui Barbosa became a recurrent opponent of the established regime, facing at the same time the oligarchic artifices of the groups that were in power and the militaristic thinking of other opposition groups. Barbosa was an authentic polymath, exercising functions such as journalist, diplomat, translator, and orator. For his performance at the Hague peace conference in 1907, he became known as ‘The Eagle of the Hague.’ He became a critic of the frequent application of the emergency rule of state of exception and the authoritarian instruments that the authorities used in Brazil, defending the legalism and the autonomy of the judiciary, as well as carrying out widespread electoral reform, including the adoption of the secret ballot, non-existent in the Oligarchic Republic. In his second electoral dispute, in 1919, Rui Barbosa was no longer restricted to discussing institutional and formal issues, bringing attention to the so-called social issues, such as regulation of the work of minors and protection against accidents at work (Paim 2018, 184).
Joaquim Francisco de Assis Brasil (1857–1938), in addition to being a historic republican, was another prominent liberal of the period. In the extreme south of the country, he assumed leadership of the fight against the dictatorship implanted in his state, Rio Grande do Sul, under the political guidance of Castilhismo, a local adaptation of the positivist thought based on the figure of Júlio de Castilhos (1860–1903). That application of positivism became the most successful doctrinal formulation of authoritarianism in the still young Republic.

It’s important also to add the name of the jurist João Arruda (1861–1943), who, after the death of Rui Barbosa, became an important liberal theorist in São Paulo in the 1920s. In the frontispiece of his book *From the Democratic Regime* (*Do Regime Democrático*), Arruda highlighted the motto of Patrick Henry (1736–1799), “the price of freedom is eternal vigilance,” which would become the motto of Brazilian liberals after the Second World War.

**Liberals under Getúlio Vargas’s rule (1930–1945)**

At the end of the 1920s, authoritarianism in Brazil was practiced by republican oligarchies, but, after has being combined with liberal formalism for a while, acquired different doctrinal proportions. As in the rest of the world, it strengthened new openly anti-liberal political currents, often rival currents. Despite this, the word *liberal* continued to enjoy some popularity. In Brazil, a coalition of political forces discontented with the current system created an electoral ticket they called the ‘Liberal Alliance’ to challenge the candidate of the dominant oligarchy. Unfortunately for liberals, the leader chosen to represent the Liberal Alliance was Getúlio Vargas, from Rio Grande do Sul, where the authoritarian trend then called Castilhismo held sway. The 1930 Revolution overthrew the oligarchic republic, but instead of calling for a new Constituent Assembly, Vargas became provisional president and began to govern arbitrarily.

In 1932, liberal currents that supported the 1930 Revolution and currents from the previous oligarchy joined themselves in São Paulo to claim a Constituent Assembly, provoking what became known as the Constitutional Revolution. Vargas repressed the revolt of São Paulo, but a Constituent Assembly was only formed in 1934, keeping him in power by indirect election. New elections were scheduled for 1937, with liberal currents gathered around the candidacy of Armando de Sales Oliveira (1887–1945), the governor of São Paulo who had links to the business sector. He was part of the liberal circles that had supported the revolution that brought Vargas to power, and he did not countenance authoritarianism. For Sales, communists and dictators of all kinds needed to be fought in the name of liberal

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11. An authoritarian political current derived from positivism that was born in Rio Grande do Sul.
democracy. His candidacy would be supported by the party called União Democrática Brasileira, founded to support his election. Unfortunately for his supporters, the plans were thwarted by a Vargas coup that dissolved Parliament, canceled the elections, and established, in 1937, the nationalist and statist dictatorship called Estado Novo.

The Estado Novo is the period of the greatest eclipse of the Brazilian liberal tradition. The Vargas dictatorship reinforced all the patrimonial and positivist aspects of the national political culture, promoting an industrialization developed by the state. Liberals were fully disorganized as a movement, persecuted and exiled, and the dictator assumed wide powers over the country, building a political machine that would survive his regime and keep his allies as occupiers of power. In the final stretch of the dictatorship, in 1943, part of the elite of the state of Minas Gerais signed the famous Manifesto dos Mineiros, a politically liberal demand for the end of the dictatorship, claiming that Brazil did not need Vargas’s authoritarian command to develop.

Also around this period, the figure of Eugênio Gudin (1886–1986) began to stand out. Gudin was the iconic supporter of economic liberalism in the mid-twentieth century, exerting great influence on the next generation of Brazilian economists. As minister of finance during the brief administration of Café Filho (1899–1970), between 1954 and 1955, Gudin defended fiscal responsibility and monetary balance. He was also a member of the Mont Pèlerin Society and represented Brazil at the Bretton Woods Conference. He was one of the very few Brazilian admirers of Ludwig von Mises (1881–1973) and Friedrich Hayek (1899–1992), although he did not reject the ideas of John Maynard Keynes (1883–1946). Gudin helped to create the first Brazilian university course in Economy, worked for foreign companies such as Light, and was an autodidact economist, an engineer, and even an amateur opera singer.

The economic debates between Gudin and Roberto Simonsen (1889–1948), defender of development through the state route, became famous. Simonsen was the practical winner of the debate, with the Vargas dictatorship and most subsequent governments adopting his formulations to some extent.

National Democratic Union and the opposition to Varguism (1946–1964)

When the Vargas dictatorship ended, the 1946 constitution came to govern a multiparty political system. Three parties were really relevant nationally. Two of them, Brazilian Labor Party (PTB) and Social Democratic Party (PSD), came from Getúlio Vargas’s political machine and maintained Vargas’s populism as the most powerful political current of the period. At first, in 1945, a large portion of
the opposition to Vargas joined the third party, the National Democratic Union (UDN), which even included a socialist current. Over time, the UDN had mainly become a haven for liberal and conservative currents opposing Varguism. Some smaller parties tended to support the UDN. One was the parliamentary party Partido Libertador, which had Raul Pilla (1892–1973), a doctor from Rio Grande do Sul and a supporter of Assis Brasil. Another one was the Brazilian Christian Democratic Party.

Despite the departure of the socialists, the UDN never had a single ideological point of view. It had internal wings and distinctions according to the region of the country. A portion of the UDN, known as *bossa nova*, was closer to economic intervention. Another wing, known as *chapa branca*, stood out for making pragmatic alliances with the Varguist currents. Its economic liberalism had limitations. For example, the state oil monopoly in Brazil was officially proposed by a Udenist, Olavo Bilac Pinto (1908–1985). The most forceful wing in opposition to communism and Varguism was known as “*banda de música*.”

Udenist liberals were influenced by tenentism—a political movement at the end of the Oligarchic Republic in which low-ranking soldiers organized diffuse rebellions against the established political system—and believed that solutions through force were necessary to dismantle the political-economic structure built by Getúlio Vargas and carried on by his political heirs. Udenist liberals eventually supported military initiatives considered to be coups against their opponents. In fact, the first presidential candidates supported by the UDN were military graduates of tenentism: Brigadier Eduardo Gomes (1896–1981) and Juarez Távora (1898–1975). While both were defeated, the latter stood out for his defense of increasing the power of municipalities.

Among UDN civilian leaders, there are names such as Octavio Mangabeira (1886–1960); Afonso Arinos (1868–1916); Milton Campos (1900–1976), who was governor of Minas Gerais; Pedro Aleixo (1901–1975); and Aliomar Baleeiro (1905–1978). However, the most charismatic and popular leader in the history of the UDN, considered a central character in the history of 20th-century Brazil, was Carlos Frederico Werneck de Lacerda (1914–1977).

A communist in his youth, Lacerda converted to Catholicism under the influence of intellectuals such as Alceu de Amoroso Lima (1893–1983) and Gustavo Corção (1896–1978). Later, other religious thinkers, especially the American bishop Fulton Sheen (1895–1979), would have an influence on him, including his use of the media to direct his speeches against everything that he considered a threat to civilization and the prosperity of the nation. Lacerda took a viscerally anti-communist stance, stating that in the doctrine he espoused in his youth there were elements incompatible with Western civilization, such as the nationalization of structures and their unrestricted subordination to the state as the
protagonist and leader of the whole society. He came to see in communist doctrine the danger of a dictatorship worse than the others, much more organized and, consequently, much more difficult to overthrow.

Communism was Lacerda’s greatest concern in international relations, so much so that he was always averse to the interests of the Soviet Union, openly supporting a democratic and western alliance with the United States against Soviet totalitarianism. In Lacerda’s mature phase, during the 1950s and 1960s, his greatest theoretical and political inspirations resided in Rui Barbosa’s anti-oligarchic liberalism. Lacerda had a conservative sympathy for the idea of a free ‘Christian civilization’ in the style of British prime minister Winston Churchill (1874–1965), and, in particular, in the party and ideological ensemble that brought Germany back after the Second World War: the Christian democracy of Chancellor Konrad Adenauer (1876–1967) and the German ordoliberalism (or ‘social market economy’) of authors like Wilhelm Röpke (1899–1966) and Ludwig Erhard (1897–1977). Lacerda preached the intention of making the UDN a grassroots party, capable of mobilizing public opinion to build an authentic liberal-democratic regime and defend the notional heritage of Western Judeo-Christian civilization. He promoted the decentralization of power, tolerance of foreign capital as a partner for national development, free enterprise, and the abolition of a tax that financed labor unions.

**The military regime (1964–1985)**

The tensions between Lacerda’s UDN and the Vargas parties, with the participation of the communist movement and the polarizing atmosphere of the Cold War, led to definitive friction in 1964, when President João Goulart (1919–1976) sided with the extreme left represented by some communist leaders and his brother-in-law Leonel Brizola (1922–2004) and broke the hierarchy of the armed forces, giving amnesty to rebel soldiers. The opposition political forces solved this problem with a coup, called by their defenders a ‘counter-revolution’ or ‘counter-coup,’ which deposed Goulart and had the support of most Brazilian liberals, particularly the UDN.

The initial intention was that a government led by military president Humberto de Alencar Castelo Branco (1897–1967), indirectly elected by the Parliament, would conduct elections for a new civilian president. In anticipation of these
elections set for 1965, the name of Carlos Lacerda emerged as one of the favorites for the presidency, which would allow the UDN to directly govern Brazil for the first time. The only time the party had won national elections was when it supported Jânio Quadros (1917–1992), who was not affiliated with the UDN and resigned in his first year of government. But the planned elections were cancelled.

The Castelo Branco military government paid attention to economists with a more liberal and sympathetic view of foreign capital, many of them influenced by Eugênio Gudin. That policy significantly reduced inflation. Some figures with a more liberal view became relevant throughout the military period, such as Octavio Gouveia de Bulhões (1906-1990), Mário Henrique Simonsen (1935-1997), and Helio Beltrão (1916-1997). However, in general terms, after the Castelo government, the regime suffered a political hardening and a progressive nationalization of economic conduct. The military nucleus, supported by civil circles, claimed the need to purge the country of communism and corruption. Instead of calling for new direct elections, Brazil adopted an authoritarian regime that became, especially after 1968, a dictatorship—although, unlike the Vargas dictatorship, there was regular presidential succession, always among the military, which meant that this dictatorship was not personalist. For Lacerda it did mean the loss of his right to run for office. And political liberalism again lost ground.

The New Republic (1985–present)

The end of the military regime and the beginning of the New Republic, organized around the 1988 constitution, led to attempts to found nominally liberal parties. An example, founded in 1985, is the Party of the Liberal Front, PFL (Partido da Frente Liberal) of Senator Jorge Bornhausen (b. 1937), who had been a member of the UDN. Another is the Liberal Party, PL (Partido Liberal), founded by Álvaro Valle (1934–2000). The Liberal Party ran businessmen Guilherme Afif Domingos (b. 1943) for president in 1989 and Flávio Rocha (b. 1958) in 1994. However, the first party changed its name, rejecting the designation ‘Liberal’ and adopting that of ‘Democrats,’ similar to the Democratic Party of the United States. The second one disappeared, merging with PRONA, a nationalist and anti-liberal party led by Enéas Carneiro (1938–2007). Such facts attest to the fragility and lukewarmness of liberalism in the first decades of the New Republic.

The most successful presidential candidate with a liberal platform was Fernando Collor de Mello (b. 1949), who in 1989 won the presidency in Brazil’s first direct elections since 1960. Collor initiated modernizing reforms but was involved in corruption scandals. In addition, he adopted interventionist economic measures, such as the confiscation of savings, which had a profound traumatic effect. Such actions induced an impeachment process, the first in Brazilian history.
The predominant political current in the New Republic was in fact social democracy.

The government of the social democrat Fernando Henrique Cardoso (b. 1931), who was president from 1995 to 2002, faced hyperinflation and a need to balance the budget. He had support from liberals to carry out important economic reforms. The Real Plan (Plano Real), which was successful in stabilizing the currency, and which started under the previous government—that is, under the brief presidential term of Itamar Franco (1930–2011) who became president as consequence of Collor’s impeachment—was carried out with the participation of very liberal economists, such as Gustavo Franco (b. 1956).

Until 1993, Brazil opened its economy rapidly. After that, the process lulled and then accelerated again from 1998 to 2004 during the government of President Fernando Henrique Cardoso. From the presidential term of Luiz Inácio Lula da Silva (b. 1945) onwards, during the age of the Workers’ Party, there was a considerable reversal, which was continued by Dilma Rousseff (b. 1947). Rousseff too faced an impeachment process; she was convicted and removed from the presidency. That seemed to promise to change the course of the economy to a more open system, but the fact is that Brazil is still struggling to recover from its recent past.

The most representative liberal leader of that period, who effectively fought what he considered to be statist and interventionist aspects of the 1988 constitution, and was also a disciple of Eugênio Gudin, was Roberto Campos (1917–2001). He became a politician after economists of the ‘Chicago School’ had advised the Chilean military dictatorship of Augusto Pinochet (1915–2006) and also in the period of the defense of free-market thinking at the international level by leaders such as Ronald Reagan (1911–2004) and Margaret Thatcher (1925–2013). Campos saw himself as a representative in Brazil of that cycle of intellectual and political defense of economic liberalism. He was a very peculiar case of a Brazilian parliamentarian who openly endorsed the theses of economists such as Ludwig von Mises, Friedrich Hayek, and Milton Friedman. Called by critics ‘neoliberal,’ or, in a pejorative Anglicist adaptation of his name, ‘Bob Fields,’ Campos ended his career as a champion of ideas like austerity in monetary policy, privatization of state-owned companies, reduction of economic regulations, and opening to foreign trade. Economist and diplomat, politician and thinker, Campos
had a long history of services rendered to the Brazilian state as an employee, taking part in the preparation of technical projects, in the creation of institutions, and in the role of minister in the Castelo Branco government. In addition, he was an ambassador before becoming a senator and deputy. He never managed to become a popular leader or create anything close to a mass movement in his surroundings and he had to put up with the aversion of socialists, social democrats, labor leaders, and other sectors of the national left. However, through his journalistic columns, television appearances, and speeches from the Congress floor, he managed to gather admirers of a more educated social group and succeeded in inspiring a generation of defenders of liberal ideas, in which the journalist Paulo Francis (1930–1997), a former leftist, can be included. A recurring statement of Campos was to point out the escapist tendency of Brazilian leaders, who would always hold foreign powers or the elite’s ‘hidden forces’ responsible for the country’s own failures. These trends would have prevailed, for example, in the João Goulart administration, to which Campos served as ambassador in Washington, making direct contact with American president John F. Kennedy (1917–1963). Both as an ambassador and as an economist, Campos established relations with the principal world leaders.

**Intellectual developments of liberalism**

Still within the New Republic period, in the exclusively theoretical field, the work of important liberal thinkers stands out. José Guilherme Merquior (1941–1991) was a literary critic, political scientist, diplomat, and writer who became famous for polemicizing against socialism and Marxism. Merquior was a disciple of the French intellectual Raymond Aron (1905–1983) and believed in a political agenda that he called ‘social liberalism.’ Although he recognized the importance of Mises and Hayek in denouncing socialist tendencies, he advocated Keynesian policies and believed that they had saved capitalism and liberalism. Merquior was a historian of liberal thought, particularly in his work *The Ancient and Modern Liberalism* (*O Liberalismo Antigo e Moderno*). The jurist Miguel Reale (1910–2006), in his mature phase, was another important Brazilian intellectual who considered himself an adherent of social liberalism, as did the professor, lawyer and politician Marco Maciel (b. 1940).

Antonio Paim (b. 1927) is a notable reference in the social sciences and in the historiography of Brazilian liberalism. Paim studied philosophy in the Soviet Union, when he was a communist sympathizer. He is the author of dozens of academic articles and reference books, as well as prefacing and collaborating on reissues of works by other authors. His classic *History of Brazilian Liberalism* (*História do Liberalismo Brasileiro*), re-edited in 2018, is an impactful work on the theme.
Among his pupils, Ricardo Velez Rodríguez (b. 1943) stands out; he had a brief stint at the Ministry of Education and developed studies on Alexis de Tocqueville, the influence of French liberals on the Brazilian elite, the problem of patrimonialism, and the nature of Castilhismo. Paim also lists the following Brazilian intellectuals who looked at different aspects of liberalism: Alberto Oliva (b. 1950), Francisco de Araújo Santos (b. 1935), Roque Spencer Maciel de Barros (1927–1999), João de Scantimburgo (1915–2013), Gilberto de Mello Kujawski (b. 1929) and Celso Lafer (b. 1941).

It is necessary to highlight two more names in the field of scholarship in recent decades. One is that of Ambassador José Osvaldo de Meira Penna (1917–2017). Echoing Edmund Burke, Meira Penna defended the old British Whiggism, heir to the freedoms and de-concentration of powers made possible by the Magna Carta of 1215. He contrasted its legacy with that of the French revolutionary mentality and Jacobinism, which provided the roots of socialism and exacerbated nationalism. The countries of Latin America would have been living in the shadow of the Jacobin terror or Bonapartist authoritarianism, he thought. Rather than institutionalizing freedom, as did the British or Scottish line liberals and the American founders, who would have reconciled order and freedom within a legal framework, these countries preferred Rousseau’s foolishness, which would condemn a tension between the despotism of the caudillos and the democratism of the agitators. In addition to Meira Penna, Ubiratan Borges de Macedo (1937–2007) should be named. He defined liberalism as a historical phenomenon rather than an abstract doctrine; he pioneered in the study of monarchical liberalism, as well as advancing the liberal approach to ‘social justice,’ using Hayek’s considerations.

In 2002, with the election of Lula and the rise of the Workers’ Party (Partido dos Trabalhadores), PT, to power, liberalism was again eclipsed, with leftist currents running strong. On the other hand, in recent years, adding the work of several organizations to the corruption scandals and the economic problems caused by the PT governments, different aspects of liberalism have gained wide popularity, especially among young people, using virtual means to disseminate their ideas. The phenomenon, which also encompassed conservative thinking, was popularly known as ‘New Right,’ for demarcating the reappearance of these ideas in a more ostensible and numerous way, after a period of relative ostracism.

In the New Right train, at least one party was identified with liberal ideals, called Novo (‘new’ in Portuguese). It was founded in 2011, supporting the engineer João Amoêdo (b. 1962) for president, who won 2.5 percent of the valid votes. In the same election, however, Novo won the election in the state of Minas Gerais, bringing to power Governor Romeu Zema (b. 1964), and parliamentarians who openly declare their adherence to liberalism were elected through other parties. Second, the economist Paulo Guedes (b. 1949), who was graduated from the
University of Chicago and claims to be an advocate of Chicago School economics, headed the Ministry of Economy in the presidential government of Jair Bolsonaro (b. 1955). Many other economists who see themselves as advocates of the Chicago School are working under Guedes’s command.

Politicians like Zema and Bolsonaro were elected, in part, as a result of a vast wave of rejection not merely of the left and the PT but also of mainstream politics because of corruption scandals revealed by an investigation called Operação Lava Jato (Car Wash Operation).

Liberalism in Brazil today

Context of liberalism in the New Republic

Since the 1980s, Brazilian liberalism experienced a phase of discredit in the period of the New Republic when national party politics became a dispute between the social democrats of the Party of Brazilian Social Democracy of Fernando Henrique Cardoso and Luiz Inácio Lula da Silva’s Workers’ Party. However, it also entered an unprecedented stage: the theoretical defense undertaken by institutes, organizations, and social networks. For the first time in the country’s history, institutes, study groups, and student organizations came into existence with the purpose of spreading the political and economic ideas of liberalism.

As early as the 1970s, there were liberals influenced by the ideas of Mises and Hayek, like the entrepreneur and engineer Henry Maksoud (1929–2014), who came to write a draft constitution for Brazil based on Hayek’s theory of demarchy. This is a system in which the law-making assembly is composed of a council of wise people, not elected by universal suffrage, as a method to guarantee the rule of law. Maksoud brought Hayek himself to Brazil several times and disseminated liberal ideas through television and a periodical, Visão magazine, which he acquired in 1974.

The Liberal Institute

The first organization created with the exclusive function of spreading liberalism was the Liberal Institute, which was founded in Rio de Janeiro in 1983 by businessman Donald Stewart Jr. (1931-1999), who was a convert to liberal thought due to his readings of Hayek’s work and translated Mises’s classic Human Action to Portuguese, though he was not a scholar. Since its inception, the Liberal Institute has functioned as a nucleus bringing together authors of different trends within
liberalism. Its first academic director was Og Francisco Leme (1922–2004), and the entrepreneur Salim Mattar (b. 1948) is one of its main supporters.

For Paim, it was thanks to the Liberal Institute that “at least in part, the intense mobilization that the Brazilian business community started to develop in recent years in favor of economic opening, privatization and the extinction of state monopolies.” Stewart’s effort via the Liberal Institute was decisive in the cycle of mobilizations to try to effect “the replacement of traditional Brazilian patrimonialism through which the State dominates the economy” (Paim 2018, 347).

The Liberal Institute, in the first instance, had the purpose of translating books, because Stewart was bothered by the lack of liberal literature in the teaching materials of universities. The liberal economics of Mises, Hayek, and Friedman was undoubtedly the largest benefactor of the effort, as until then it was known by only a few circles, such as that of Eugênio Gudin’s disciples. The Liberal Institute also resumed the links of Brazilian liberalism with the main liberal economic schools abroad, such as the Chicago school and the theory of public choice of James Buchanan Jr. (1919–2013). Stewart’s purpose was similar to Hayek’s in creating the Mont Pèlerin Society. In addition to bringing together and facilitating the exchange of ideas, he took steps to bring some of these ideas to a large audience that was unaware of them, as well as to leaders from different sectors of society, from the academic sector to the political sector. Like Hayek, Stewart believed that the evolution of society in more liberal directions had to begin with civil discourse and persuasion; literature and discussion were needed.

With their encouragement, liberal institutes emerged in different states, meeting in 1988 to establish a Declaration of Principles that reflected the goals that everyone should defend in order to recognize themselves as allies. Each institute, however would conduct the projects and initiatives that seemed to them most appropriate. Most of these institutes disappeared later, but the original Liberal Institute remains in operation. In 2013, the economist and political commentator Rodrigo Constantino (b. 1976) and the lawyer Bernardo Santoro (b. 1982) were responsible for starting a new stage in the Liberal Institute’s story by increasing its presence on the Internet.
Recent institutional efforts to support liberalism

Despite the disappearance of the other liberal institutes, similar initiatives were developed, especially in the 21st century, when intellectual and political resistance to Workers’ Party governments were organized. The Millennium Institute, founded in 2005, brings together intellectuals to promote the values and principles of a free society, such as individual freedom, property rights, market economics, representative democracy, the rule of law, and institutional limits to government action.

The Mises Brasil Institute, founded in 2007 and chaired by Helio Beltrão (b. 1967), son of the liberal economist of the same name mentioned earlier, became an institution of great renown, organizing a postgraduate course in liberal economic thought. Several intellectuals attended their circles, including economist Ubiratan Jorge Iorio (b. 1946), who has been a member of the Central Bank of Brazil and the Brazilian Institute of Economics of the Getúlio Vargas Foundation in Rio de Janeiro, in addition to being a researcher at the Brazilian Market Institute of Capitals. Historian Alex Catharino (b. 1974) is editor of LVM Editora, an initiative formed by individuals linked to the Mises Brasil Institute to publish books about liberal, libertarian and conservative ideas. Political scientist Bruno Garschagen (b. 1975) wrote a book that became a bestseller, *Stop Believing in the Government: Why Brazilians Don’t Trust Politicians and Love the State*, which criticized the paradoxes of Brazilian patrimonial political culture.

These institutes and several other movements and study groups are gathered in an association called Rede Liberdade, which is designed to coordinate their efforts. Below is an examination of the main members of the Rede Liberdade network that have not yet been mentioned.

- Liberalism in the state of Rio Grande do Sul also is promoted by the Atlantos Institute, which holds lectures and events free of charge and open to the public in at least five of the largest preparatory schools in Porto Alegre. There is also the Institute of Entrepreneurial Studies (Instituto de Estudos Empresariais, IEE), an institution founded in 1984 that has held the Freedom Forum, the largest liberal event in the entire country, since 1988. It receives national and international guests, including political leaders who do not defend liberalism, like candidates for the presidency of the Republic in election years. In 2007, the Atlantos Institute received the Templeton Freedom Award Grant as one of the most promising global institutions for the defense of freedom. It was also classified as one of the 150 organizations that most influence political, social, and economic changes in the world in
the 2013 ranking of “Global Go To Think Tanks” organized by the University of Pennsylvania (McGann 2014). Two leaders in the current liberal movement of that state are Roberto Rachewsky (b. 1955) and Winston Ling (b. 1955), influenced by the Objectivist philosophy of Ayn Rand (1905–1982).

• The Livres movement, which brings together some economists linked to the Real Plan—the strategy conducted under Itamar Franco and Fernando Henrique Cardoso’s governments that ended the hyperinflation. These include Elena Landau (b. 1958) and Pérsio Arida (b. 1952), and the movement is identified with trends ranging from social liberalism to libertarianism. Its motto is the expression ‘completely liberal;’ Livres members believe that other organizations dedicated to liberalism support incomplete or too conservative versions of liberal thought and should be more concerned with issues such as minorities.

• The Acorda Brasil movement participated in the demonstrations for the impeachment of Dilma Rousseff. It still exists, and it defends the rule of law, federalism, separation of powers, individual freedom, and entrepreneurship. Its co-founder is a descendant of the Brazilian imperial family who was elected federal deputy in 2018, Luiz Philippe de Orléans and Bragança (b. 1969).

• The Mackenzie University Economic Freedom Center, in São Paulo, holds important conferences on liberal thinking, particularly in the economic field, and brings in international guests.

• The ProLiber group aims to disseminate liberal proposals for Brazilian problems.

• Other members of Rede Liberdade who disseminate liberalism in their respective states, among congresses, publications, conferences, and leader training, include: Clube Farroupilha, in Rio Grande do Sul; Clube Ajuricaba, in Amazonas; the Democracy and Liberty Institute, in Curitiba; the Liberal Institutes of Alagoas, São Paulo, Triângulo Mineiro, Minas Gerais and Paraíba (all newer and independent of the original Liberal Institute); the Dragão do Mar Study Group, in Ceará, one of the oldest and most traditional in the country; the Libertarian Youth of Sergipe; Instituto Liberdade (former Liberal Institute of Rio Grande do Sul); the Instituto Liberdade e Justiça, in Goiás; the Carl Menger Institute, in Brasília; and the Leaders of Tomorrow Institute, in Espírito Santo.

• The various state branches of the Institute for the Formation of Leaders, which aims to train leaders in the business field, also belong to Rede Liberdade.
• In the student environment, Students for Liberty Brasil operates in the student environment. It’s one of the largest student organizations in the SFL network.

Unrelated to Rede Liberdade, there is the Instituto Liberal do Nordeste, one of the oldest among the young liberal institutes, and other local institutions, such as the Instituto Libercracia, in Pernambuco. Also unrelated to Rede Liberdade is the Movimento Brasil Livre (MBL), a national movement that led street demonstrations against the government of President Rousseff, publicly expressing an identification with liberal agendas and electing candidates linked to its coordination by different parties. And throughout the Jair Bolsonaro administration, the MBL has taken a position against President Bolsonaro.

Final considerations

In the academic and intellectual field, liberalism is far from being dominant in Brazil, even in the economic area. However, there are institutions that share experiences and initiatives with the purpose of promoting the dissemination of liberal ideas in a more consistent way. This cooperation among liberals is unprecedented in Brazilian history.

A native liberal tradition goes back to the circumstances that led to the foundation of Brazil as an independent country. The tradition coexists with cultural challenges that keep it from fully embedding in society and in the conduct of institutions. These challenges make Brazil, from time to time, experience cycles of intensification of authoritarianism and institutional ruptures. However, since the country officially appeared in 1822, there has been no historical period in which there were no active liberals, either politically or economically, fighting for the fundamental ideas of liberalism in the face of great obstacles.

The future of Brazilian liberalism depends, in part, on not losing sight of the efforts and contributions of that tradition. At the same time, the foreign perception of liberalism in Brazil will always be deficient if it does not incorporate awareness of the relative antiquity of liberalism in the country.

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**About the Author**

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Dispute on Method or Dispute on Institutional Context?
Foreword to the Translation of Carl Menger’s “Errors of Historicism”

Karen Horn\textsuperscript{1} and Stefan Kolev\textsuperscript{2}

As one of the archetypal disputes in the social sciences, the Methodenstreit (dispute on method) in German economics which unfolded during the 1880s has received substantial attention from multiple angles over the past 140 years. For decades it has constituted a fixture in German-language political economy. One reason for this, beyond the unusually vitriolic tone of the dispute, are the fascinating particular issues of methodology, morphology, and epistemology addressed in its course. Also, many scholars say it shows a dismal disposition to wage more or less abstract, ultimately fruitless methodological wars, instead of addressing pressing real-world problems in a pragmatic manner.\textsuperscript{3} It is sometimes said to reflect an obsession in the social sciences specifically German (and Austrian, for that matter). Indeed, for good or bad, there is a rich history of such disputes, making \textit{Streite} a trademark of the German social sciences.

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\textsuperscript{3} See, for example, Schumpeter 2006/1954, 782–783; Köster 2011, 121–141; Glaeser 2014, 135–152.
A series of disputes extends (at least) from the 1880s Methodenstreit to the Werturteilstreit (dispute on value judgments in academic inquiry) during the 1900s–1910s, to the Positivismusstreit (dispute on the nature of the social sciences as well as on the status of values) between the Frankfurter School and the critical rationalists during the 1960s, and to what was termed the ‘most recent Methodenstreit’ in 2009–2010 on the legacy of ordoliberalism in German economics departments. To this list may be added the meta-Streit over whether these disputes have contributed to the depth and the problem-solving capacity of the social sciences or whether they have instead congested the scholarly discourse with free-floating abstractions and thereby prevented more important applications of scholarly energy. Not very tender in his judgments, Joseph Schumpeter, in his *History of Economic Analysis*, deplores the “large amount of mutual misunderstanding” and considers the whole stream of literature that ran from this dispute as “a history of wasted energies, which could have been put to better use” (2006/1954, 782–783).

The main opponents in the Methodenstreit of the 1880s were Carl Menger (1840–1921) and Gustav Schmoller (1838–1917). Menger, one of the founding fathers of the Austrian School, is known for his contributions to the subjective theory of value and marginalism (e.g., Caldwell 1990; Hagemann et al. 2010). He began his career in journalism (Yagi 1992, 93–98), but then deepened his studies of political economy leading to the publication of his *Grundsätze der Volkswirtschaftslehre (Principles of Economics)* in 1871, which afforded him a habilitation at the University of Vienna. He sought a unified price theory based on a single principle, arriving there by combining an empirical behavioral theory of subjective evaluation and methodological individualism, considering social facts, processes, and institutions as unintended results of otherwise purposeful individual action. Taken alone, none of these elements were altogether new, and to some Menger thus “appeared a little old-fashioned and outdated” (Streissler 1990, 44).

In 1873, Menger was named professor at the age of 33, and in 1879 he became a full professor and held the chair of economic theory at the University of Vienna (Streissler 1988, 197). Starting in 1876, for several years he also served as a tutor of Crown Prince Rudolf (Streissler 1994). Menger’s lectures to the Prince drew especially from Adam Smith’s *Wealth of Nations*, and he expressed his affinities with Smith’s legacy in an essay commemorating the centenary of Smith’s death (Menger 2016/1891).
Menger was an erudite man, versatile in a multitude of fields, but nevertheless a shy, rather fearful person (Bonn 1953, 62–63). He probably never meant to engage in a vitriolic controversy or to distance himself from his colleagues in German political economy when he published his work, as he considered himself neither a rebel nor a revolutionary. He identified his approach as a “product of recent development in German political economy,” emphasizing that “the reform of the most important principles of our science here attempted is therefore built upon a foundation aided by previous work that was produced almost entirely by the industry of German scholars” (Menger 1976/1871, 49, his italics).

While his Principles received friendly but not overwhelming reviews (Streissler 1990, 39 n.23), it seemed to Menger that the importance of the new combination in his approach still escaped some of his readers, and so in 1883 he published a book which can be seen as a sequel, Untersuchungen über die Methode der Socialwissenschaften und der Politischen Oekonomie insbesondere (Investigations into the Method of the Social Sciences with Special Reference to Economics), focusing even more than before on methodology. Menger strongly insisted on distinguishing the historical and statistical study of economic forms both from theoretical economics and from practical fields such as economic policy and public finance. He thought that the historical study of economic structures, while useful, was only complementary to theory and could not seek to replace it (Menger 1985/1883, 41–43).

Gustav Schmoller, the influential leader of the Younger German Historical School and a proponent of an ‘inductive’ approach to economic thinking, then penned an unfavorable and in some parts personally insulting review of Menger’s Investigations (Schmoller 1883). Menger had disingenuously claimed that he wrote as a friend to the Historical School, but his books were “very obviously intended to vindicate the rights of theoretical analysis and to put the Schmoller school in its place—and a very secondary place it was!” (Schumpeter 2006/1954, 782). At least to some extent, Schumpeter says, “the quarrel was about precedence and relative importance” (ibid.). Simon Cook and Keith Tribe sum up the substance of the dispute as follows: “Menger argued, on the one hand, that theory was not susceptible to inductive elaboration—something which Schmoller in truth never even attempted—while Schmoller asserted, on the other, that it was, but never sought to demonstrate how” (Cook and Tribe 2016, 303).

At the time of the dispute, Schmoller had just moved to Berlin to teach at the famous Friedrich-Wilhelms-Universität (today Humboldt-Universität), after having held chairs at Halle and Strasbourg (Balabkins 1988, 18–44). He exerted much of his academic influence through the Verein für Socialpolitik, the

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4. On the Younger German Historical School see, for example, Rieter 2002; Peukert 2001. For a criticism of such labeling, see Backhaus and Hansen 2000.
main—and still existing—association of German-language economists, which he had co-founded in 1872 and chaired from 1890 to 1917 (Grimmer-Solem 2003, 171–202), as well as through editing the *Jahrbuch für Gesetzgebung, Verwaltung und Volkswirtschaft im Deutschen Reich*, an important journal (a quarterly, its name notwithstanding) on law, administration, and political economy which Schmoller edited from 1877 to 1917 and which in 1913 was renamed *Schmollers Jahrbuch* (McAdam, Kolev, and Dekker 2020). Schmoller, whose “petty bourgeois appearance could not always hide his passionate temperament and by no means harmless cleverness,” was so powerful that most academic appointments in political economy were based on his recommendations (Bonn 1953, 51, 53). Even in Austria-Hungary the Historical School had a firm grip on professorial positions (Blumenthal 2007, 66–75). Schmoller cooperated effectively with Friedrich Althoff, the near-almighty administrator of the Prussian academic system whose ‘Althoff system’ shaped the young Empire’s academic system (at least) from 1882 to 1907 (Backhaus 1993).

In response to Schmoller’s criticism, which upset him deeply, Menger prepared his second major contribution to the Methodenstreit, composed in the style of a collection of letters to a friend under the title *Die Irrthümer des Historismus in der deutschen Nationalökonomie* (*The Errors of Historicism in German Economics*, hitherto abbreviated as *The Errors of Historicism*). He submitted it to the press already in January of the following year (Menger 1884). This pamphlet, considered by some “a piece of masterly academic polemics” (Campagnolo 2016, 290), “fairly steamed with wrath and of course elicited rebuttal” (Schumpeter 2006/1954, 782). Schmoller himself, though, refrained from engaging in the dispute a second time in substance. In a short announcement in his *Jahrbuch*, however, he reprinted a letter he had sent to Menger, explaining that it was against his principles to engage in such personally offensive “literary armed conflicts” and that, instead of “destroying such a beautifully designed booklet,” he had sent it unread back (Schmoller 1884, 677; Bostaph 1994, 460).

This first battle did not decide the war of the Methodenstreit that had just started. In the decades to follow, the next generations kept discussing the issues which erupted in the early 1880s (Pribram 1983, 222–224), and the Werturteilsstreit during the 1900s–1910s is often called ‘the younger Methodenstreit’ due to the continuities in its agenda to ‘the older Methodenstreit’ of the 1880s (Backhaus and Hansen 2000, 312–313).

Beyond the German-language disputes, *The Errors of Historicism* has constantly been quoted in the burgeoning literature about the Austrian School since the ‘Austrian revival’ during the 1970s. But given today’s rather limited command of German in academia, it is one of those works that many have heard about but almost none have read. In light of the explosion of historiographical efforts related
to the evolution of the Austrian School, it is quite surprising that the text—under 100 pages in the German original—has not previously been fully translated into English. Only a notable English-language “digest” which “paraphrased and abbreviated” the book’s main arguments was produced by Albion Small (1924, 587–597).

In the process of creating and editing this first translation of *The Errors of Historicism*—an effort that is part of a larger project of the NOUS Network for Constitutional Economics and Social Philosophy—we have collected a multitude of observations, both inspiring and frustrating. In this foreword to the translation, we embed those observations within our reflections on the nature and value of the Methodenstreit.

Is *The Errors of Historicism* more than an underdog’s polemic?

The understanding that the Methodenstreit was unproductive may have taken root precisely because of the polemical character of *The Errors of Historicism*. One may therefore ask whether it was the very publication of *The Errors of Historicism* that derailed the dispute, so that a potentially useful controversy was led into a dead end. A look at Schmoller’s review of the *Investigations*, i.e., at the piece to which Menger reacted in *The Errors of Historicism*, is necessary at this point. While Menger’s *Investigations* was translated in 1963 on the 80th anniversary of its publication and have thus been accessible to English-speaking scholars for a long time, Schmoller’s review unfortunately remains untranslated to this day. The NOUS Network for Constitutional Economics and Social Philosophy is currently translating it for a forthcoming publication in the *Journal of Contextual Economics*—*Schmollers Jahrbuch*, which is about to reach the 150th anniversary of its founding.

In his review, Schmoller (1883) jointly discusses and contrasts Menger’s *Investigations* and Wilhelm Dilthey’s first important theoretical work, *Introduction to the Human Sciences* (Dilthey 1883). The text begins with Schmoller’s outright admission that in the case of Menger’s book he “cannot quite contain the polemic” since he feels that Menger’s “attacks are in part strikes at me personally” (Schmoller 1883, 975). Focusing his review on Book One of the *Investigations*, Schmoller portrays the book as an attack on “the scholarly division of labor” between the theoretical direction in economics and the collection of “descriptive material” (ibid., 978). After criticizing Menger’s “unworldly ivory tower naivete,” Schmoller utilizes again his notion of the scholarly division of labor to demonstrate how

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5. We are responsible for the translations of quotations from Schmoller in this foreword.
earlier generations in German political economy, embodied by Karl Heinrich Rau
and Friedrich von Hermann, were unable to found their theories on sound
theoretical foundations because of an unwillingness to put away “the blinders of
division of labor in specialized sciences” and to work, in today’s terms, along
interdisciplinary lines with professional psychologists (ibid., 979). He reads Menger
as pushing economics towards disembedding the study of the economy from its
institutional context—from “state and society, custom and law” (ibid., 980). This
turns out to be a crucial issue which we repeatedly sensed in the translation and
editing process of The Errors of Historicism. The dispute on method seems to be after
all a dispute on the meaning of concepts for demarcating what economics and its
sub-domains should be about.

Schmoller concedes that the institutional context does not matter “if theo-
retical economics is above all confined to the theory of value and price formation,
of income distribution, and of monetary issue”—but in his own conceptual
demarcation of economics, such a notion of economics “does not capture the
general essence of the economy” (Schmoller 1883, 980). As one of the “empiricists
digging in the dust,” Schmoller sees himself disparaged by Menger’s alleged
willingness to elevate his “set of secured deductive conclusions” to a status against
which “empirical reality can be put aside with contempt as something irrelevant”
(ibid.). Schmoller shares the aim of “a theory of the general essence and general
relationships of the economy,” but he is skeptical about the isolation methods tied
to Menger’s abstraction techniques (ibid., 981).

Schmoller (1883, 981) calls for a broad foundation of the social sciences,
with a special emphasis on “what is prerequired” before economic theory can be
adequately applied to its proper domain. Crucially for him, the complex inter-
relatedness of these institutions calls for a “collectivist” capturing of that inter-
relatedness in the institutional context. His perception of the difference existing
between him and Menger is probably best captured in the following juxtaposition:

This [the necessity of a collectivist view of the interrelated institutions] is
inaccessible to him [Menger] because he exclusively starts from the singular
observation of the single economy, always only thinks of exchange, value,
money, etc., but not of the economic organs and institutions which constitute
the bone skeleton of the economic body. (Schmoller 1883, 983)

Schmoller’s accusation concerning the institutional disembeddedness in
Menger’s approach culminates in reprimanding him for presupposing the institu-

6. Volkswirtschaft
7. Einzelwirtschaft
8. volkswirtschaftlich
tional snapshot of Western Europe as the context for his theory, thus sharing, “with the very old dogmatic economics,” “the great methodical error to assume the essence of his time to be the general essence of the economy” (Schmoller 1883, 983). Regarding Menger’s view of institutions, he is highly skeptical about Menger’s one-sided insistence on what later generations termed ‘methodological individualism’ and ‘unintended consequences’ of human action in the establishment and evolution of institutions—an inclination which Schmoller attributes to Menger’s alleged “Manchesterian antipathy against any conscious activity of the collective organs of society” (ibid., 986). The word Manchesterism for Schmoller and others at the time functioned much like the word neoliberalism does today, as an ill-defined pejorative used to cut down ideological adversaries.

Beyond economic theory, the debate between Menger and Schmoller also concerns the two other traditional sub-domains within German economics. While the terms vary, the sub-domains as captured by the three main courses during Max Weber’s tenure starting in 1897 at Heidelberg, one of the top universities in the German-language area, are demarcated as follows: ‘Allgemeine oder Theoretische Nationalökonomie’ (general or theoretical economics), ‘Praktische Nationalökonomie’ (practical economics, focusing on various domains of economic policy) and ‘Finanzwissenschaft’ (public finance) (Heilmann 2017, vii). Menger and Schmoller specifically disagreed on the extent to which economic policy and public finance can and have been, in Schmoller’s view, “elevated to the rank of theoretical sciences” (Schmoller 1883, 981), as opposed to being “only” arts. Arts, in Schmoller’s understanding, can only give practical advice on the basis of ideologies such as “free trade” or “state socialism” (ibid., 982). Schmoller insists that the distinction between of theoretical and practical economics is fundamental for Menger, while he himself considers that it can only be a gradual one. As far as Menger is concerned:

A complete system of the actual economic sciences will [...] comprise the following: 1. The historical sciences of economics: economic statistics and economic history. The former has to investigate concrete economic phenomena from the point of view of statics and within definite spatial limits, while the latter has to study them from an evolutionary standpoint and to combine them into a unitary, organic structure. 2. The morphology of economic phenomena, whose function consists in the classification of economic facts in accordance with their genera, species, and subspecies, as well as the demonstration of their generic form, i.e., the description of the common structures of different groups of homogeneous phenomena. 3. Economic theory, which has the task of investigating and establishing the

9. Volkswirtschaft
laws of economic phenomena, i.e., the regularities in their coexistence and succession, as well as their intrinsic causation. I have already called attention to the appropriateness of combining the morphography of basic economic phenomena with economic theory. 4. Practical or applied economics, which teaches us the principles and procedures by which generally determined economic aims may be most effectively realized in different circumstances and in the light of existing scientific knowledge. (Menger 1960/1889, 14)

Critical reviews are seldom a delightful read for an author. But ideally, with a fair degree of intellectual openness, they can prompt an author to think twice, to reconsider and improve. That is what academic discourse is all about, and this is essentially how intellectual progress can come about (Horn 2009; Vanberg 2010). However, for this competitive process to be productive, a certain degree of politeness in discourse is a prerequisite: Scholars are human beings, after all. In that sense, Menger's drastic reaction to Schmoller's critical review may have been triggered not so much by the analytical differences and methodological accusations but rather by Schmoller's concluding comments, which depict Menger as a person who is “a sharp dialectician, a logical mind, not an ordinary scholar,” but who “lacks as much a general philosophical and historical education as the natural width of horizon” (Schmoller 1883, 987). But does that imply, in turn, that The Errors of Historicism is nothing more than the polemical pamphlet of an underdog in Vienna wounded by the harsh, insulting review that his work has received from a star? While there is something to that, we do not think it is the full story. The Errors of Historicism deserves to be considered as one element of a chain of arguments that ultimately had a long-lasting impact upon the evolution of the discipline.

During the process of translating and editing, whose details we will describe in the next section, our group made a few observations about Menger’s content, style, and possible audiences which we would now like to briefly share. Our central impression is that after Schmoller’s review, the dispute was no longer primarily about methods in the sense of the textbook story about ‘induction versus deduction’ or something like that. Schmoller later acknowledged succinctly that these two approaches are “like the two legs of a walking man” (1898, 222). Rather, our impression is that what was at stake is the following: (1) the demarcation lines within the sub-domains of economics and (2) the separability of the results of economic theory from the institutional context. In other words, Schmoller accused Menger of an exaggerated emphasis on what has recently been called “isolating economics” focusing on the market process, while he saw himself as the true representative of a “contextual economics” focusing on the institutional framework around the market process (Goldschmidt, Grimmer-Solem, and Zweynert 2016).

The struggle seems to have been about defining the meaning of concepts and about demarcating theoretical economics, economic policy, public finance,
et cetera. In this reading, the dispute is very much—as explicitly noted above by Schmoller in the less polemical earlier parts of his review—about the division of labor within economics and across the interdisciplinary borders to neighboring social sciences like law, history, or geography. Two phrases are particularly recurrent in slight variations throughout Menger’s *Errors of Historicism*: a question whether, in Schmoller’s words, practical economics “can completely strip off the garment of an art,” and a question about “political economy and its parts, the understanding of the relationships between its parts and to their auxiliary disciplines.” Later, Schmoller would write solomoniically that “the demarcation of the subject matter and the systematization of each science depend on its inner condition and the practical purposes of its efficacy and teaching” (Schmoller 1949/1893, 15).

But to whom were these attempts at demarcation directed? From the names featured both in Schmoller’s review and in Menger’s *Errors of Historicism*, it is crystal clear that the struggle is not all-encompassing but concerns specifically the soul, and control, of German economics. It is still very much a national affair. English or French names hardly ever arise at this stage of the dispute. The notion of Austrian economics is non-existent in the texts, let alone that of an Austrian School—Menger very obviously craves to be considered a full member of the German tradition, nothing else. The aforementioned preface to his *Principles*, amounting to an almost servile bow, demonstrates this almost painfully: “Let this work be regarded as a friendly greeting from a collaborator in Austria, and as a faint echo of the scientific suggestions so abundantly lavished on us Austrians by Germany through the many outstanding scholars she has sent us and through her excellent publications” (Menger 1976/1871, 49). In his endeavor to attach himself closely to German economics, Menger neglects many Austrian-born authors or German-born economists teaching in Austria (Streissler 1990, 35). After attacking Schmoller, Menger’s wrath would turn against F. J. Neumann, Lujo Brentano, and Friedrich Kleinwächter, all of whom also took “the opportunity to present their methodological views, which in part are in direct opposition to my own” (Menger 1960/1889, 23).

As the Methodenstreit unfolded, however, rival endeavors at school-building came to clash. The Historical School had emerged decades earlier—at the very moment of the Methodenstreit it was already in the mode of transformation from the dominance of the Older Historical School earlier headed by Wilhelm Roscher, Bruno Hildebrand, and Karl Knies to the Younger Historical School that came to be dominated by Schmoller. Menger’s group, however, was only about to form in the early 1880s (Wasserman 2019, 37–48). In 1884, Schmoller was 46 years old, while Menger was 44—the ideal age of mid-career scholars to start building their networks of students. This is a background that may help the reader understand what may be the most frustrating part of the dispute as we experienced
it during the process of translating and editing; not so much the heat of the polemical parts, but the fact that the two opponents seem to be consistently talking past each other. Perhaps they never really tried to persuade each other but were talking to a third party: to those young scholars entering the scene of German economics who were in need of inspiration and orientation. Ex post, the vitriolic fireworks of the Methodenstreit might have served precisely as such a signaling device.

This conjecture of course runs the risk of attributing too much strategic stringency and purposiveness to what was going on in a rather chaotic manner. But at least by hindsight, the hypothesis of such undercurrents may seem corroborated by the interlocutors whom Menger mentions at the end of the preface to The Errors of Historicism. These are two Austrians and two Germans, three of them Menger’s juniors: the Austrians Eugen von Böhm-Bawerk (1851–1914), who had just received a professorship at the University of Innsbruck and whose capital theory and critique of Marxian economics would famously carry the Austrian School into the future, and Emil Sax (1845–1927), a professor of political economy at Charles University in Prague; as well as the Germans Wilhelm Lexis (1837–1914), who at the time of Menger’s writing was teaching Staatswissenschaften, or sciences of the state, at the University of Breslau (now Wrocław), and Heinrich Dietzel (1857–1935), a professor of political economy at the University of Dorpat (now Tartu) and later at the University of Bonn.

As far as school-building is concerned, over time Menger’s signals proved more successful than Schmoller’s. In the course of the subsequent Werturteilsstreit during the 1900s–1910s, Schmoller ended up losing the brightest minds of the next generation, most notably Max Weber and Werner Sombart, and this at several meetings of the Verein für Socialpolitik, Schmoller’s institutional stronghold: of all places, a crucial clash of the Werturteilsstreit took place at the Verein’s 1909 meeting in Vienna. In contrast, Menger bonded even more closely with his associates, most notably Böhm-Bawerk and Friedrich von Wieser. They became not simply faithful followers, but original contributors to his research program—sometimes also clashing with Menger, famously on issues of capital theory (Garrison 1990)—and pillars of what was seen as Menger’s school. They published explicitly under the brand of “Austrian” theories of value or capital in the very early volumes of English-language journals, and they engaged in post-Methodenstreit skirmishes with German economists (Caldwell 2004, 83–97; Kolev 2018).

Seen in this light, on the one hand, the Methodenstreit exerted both an immediate and also a long-lasting effect on the institutionalization of the school-like structures in German economics, which prevailed still in the interwar period (Eucken 2018/1938; Eucken 1940), and The Errors of Historicism played an important role in that. On the other hand, however, the pendulum did not fail
to swing back. Schmoller’s preoccupations with exact empirical socio-economic research and also with ethics are widely shared today, and his work can even be seen as an important building block for the ordoliberal program and the theory of the “social market economy,” aiming at a harmonious combination of free markets and social policy (Schefold 1996; Peukert 2000; Goldschmidt and Wohlgemuth 2008; Goldschmidt 2013; Zweynert 2013). Even though the aspirations of the Historical School did not bring a breakthrough, today’s empirical turn in economics amid the availability of big data and new econometric methods in “the age of the applied economist” (Rodrik 2015; Backhouse and Cherrier 2017) or the recent “state capacity” debate for the order of economy and society (Cowen 2020; Hammond 2020) are not too distant from its agenda. A parsimonious evaluation should therefore be in order: Both Menger and Schmoller have made an impact, both have contributed important insights to the discipline, and it is up to the modern researcher to heed—and balance—their calls.

‘Crowd-translating’ and its challenges: the project of the NOUS Network

Translating longer texts certainly does not feature among the activities which today’s economics profession holds in particularly high regard. And yet when we co-founded the NOUS Network for Constitutional Economics and Social Philosophy in 2015, we stipulated that a crucial mission of NOUS would be to make the insights of German-language political economy accessible to an international audience. This of course includes the promotion of original research, and currently we are conducting projects on the Colloque Walter Lippmann for the Journal of Contextual Economics—Schmoller’s Jahrbuch and on the ‘thinking-in-orders’ tradition for History of Political Economy. But given the rather limited command of German in academia, especially among younger scholars, we see making classic German-language texts accessible to be an important task of NOUS. Again, we are currently translating Schmoller’s review of the Investigations and his disparaging letter to Menger of 1884, soon to be published in the Journal of Contextual Economics—Schmoller’s Jahrbuch. In that same journal, another set of eight translations is currently being finalized, very much from the age which we discuss here, including articles by Schmoller, Georg Simmel, Ferdinand Tönnies, Walter Eucken, and a long article by Schumpeter directly relevant here, “Gustav von Schmoller and the Problems of Today” (Schumpeter 2018/1926).

When Dan Klein, a member of NOUS, approached us regarding a translation of Menger’s Errors of Historicism on behalf of Econ Journal Watch, this was
a perfect match. We were happy to be able to build upon a translation of the early sections of *The Errors of Historicism* conducted by Diana W. Thomas, another member of NOUS. And for what followed, the NOUS Young Affiliates (link) turned out to be a truly invaluable resource. Currently 80 in number, we incorporate young scholars into the existing greater academic network through a structured admission procedure open to students on the bachelor’s, master’s or doctoral level. And while ‘crowd-funding’ has become a common practice today, we conjectured that an experiment in ‘crowd-translating’ could also be worthwhile, and so approached our Young Affiliates. Ten of them enthusiastically volunteered to translate the Letters making up *The Errors of Historicism*, and painstakingly they produced excellent translations, notwithstanding Menger’s extra-long sentences, 19th-century lingual peculiarities, specifically Austrian expressions, and not easily tracked citation habits. To enable the reader to directly see particular subtleties in Menger’s language like the co-existence of synonymous terms like ‘Wirtschaftsgeschichte’ and ‘Geschichte der Volkswirtschaft,’ we provide the German originals in footnotes. Three of the Young Affiliates, Adam De Gree, Tessa Bohling De Gree (both from the U.S.), and Tim Tewes (from Germany), all master’s students of Philosophy, Politics, and Economics at CEVRO Institute in Prague, formed a first editorial council and worked through the translations as submitted by the Affiliates. From that fantastic manuscript produced in Prague, Karen Horn and Stefan Kolev finalized the work. Given the substantial efforts of the ten Young Affiliates, it would not be enough to merely list their names, and we gratefully accepted the possibility offered by *Econ Journal Watch* to produce a gallery of pictures of these promising aspiring minds in the domain of Philosophy, Politics, and Economics. Thank you all for helping make available an important text in the history of economics for the broader international academic community.

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DIE

IRRTHÜMER DES HISTORISMUS

IN DER

DEUTSCHEN NATIONALÖKONOMIE.

Von

DR. CARL MENGER,
O. O. PROFESSOR DER STAATSWISSENSCHAFTEN AN DER WIENER UNIVERSITÄT.

WIEN, 1834.

ALFRED HÖLDER,
K. K. HOF- UND UNIVERSITÄTS-BUCHHÄNDLER,
ROTHENHURMSTRASSE 15.
The Errors of Historicism in German Economics

Carl Menger
edited by Karen Horn and Stefan Kolev
translated by the Network for Constitutional Economics and Social Philosophy (NOUS)

Preface. [III]

The vagueness of the “historical school of German economists”\(^1\) regarding the goals and methods of research in the field of political economy, an ailment that was already obvious at the very founding of this school, has not been remedied even by the passage of almost five decades of development.

From the very beginning, the “historical school” was not the result of an immersion into the problems of our own science; unlike historical jurisprudence, it did not arise from the scientific\(^2\) needs of scholars who were immersed in the problems of their discipline. It implied from its inception a transfer of historical knowledge into our theoretical-practical discipline. External circumstances created it; [IV] it was not scholars in our discipline, but historians who originally founded it. The historical method was carried into our discipline from the outside.

The historical school has never been able to free itself from these defects of its origin. The external connection of solid historical knowledge with a careful but aimless eclecticism in our discipline constitutes its starting point and at the same time the peak of its development. Several very serious attempts were made along the lines described above, intended to bring history and political economy into a closer, more organic connection, but the elevation of our discipline from its backward condition promised by the historical economists has not been achieved; indeed, it seems today to be almost further away than it was when Hermann and Rau were teaching.

That the above-mentioned reform attempts, which were in part made with unusual ability, did not reach their goal was not a coincidence; they had to fail due

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1. [“historische Schule deutscher Volkswirte”]
2. [scientific]
to the error of recognizing history as the point of departure, and its connection with political economy as the pivotal point of the intended reforms. The erroneous hypothesis that the connection of historical knowledge with political economy as such implies a reform of the latter, i.e., the false dogma of historicism in the field of our discipline, was incapable of being the basis of a promising modification of the latter from the beginning.

The reform of a discipline can only arise from within, from the depths of its own spheres of ideas; it can only be the work of scholars immersed in the very own problems of their discipline. Political economy will be elevated from its current absorbed obscurity neither by historians, mathematicians, or physiologists nor by those who are blindly following their tracks. Reform of political economy can only originate from ourselves, from us professional peers who are at the service of this discipline.

What other disciplines and their representatives can offer and accomplish for us is a progressive immersion into their own problems, the perfecting of the results of their own research. To the extent that they are relevant for the development of our own discipline, we want to use these results carefully and with appreciation, the results of historical research as well as those of statistics, psychology, logic, and of the technical sciences. In the future, however, we will have to resolutely fend off the reformist meddling of other disciplines, the intrusion of viewpoints and methods alien to political economy into the latter, if German economics is not to expect new disappointments after another half-century.

The next and most important task to be solved in the field of political economy in Germany seems to be clearly delineated by the current condition of the discipline. Historians have set foot on the ground of our discipline like foreign conquerors to force upon us their language and customs—their terminology and their methods—and to intolerantly fight every research trend which does not correspond with their own idiosyncrasies. This state of affairs has to be stopped. The problems and paths of inquiry that derive from the nature of our discipline must be brought back to honor; this discipline must be liberated from its historicizing tendency and the biases of historicism. Once political economy in Germany has

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3. [Menger employs a number of terms for “economics,” most commonly Nationalökonomie and politische Ökonomie. We consistently translate Nationalökonomie as “economics” (also in the title of the book) and politische Ökonomie as “political economy.” When Menger employs other terms like Volkswirtschaftslehre or Wirtschaftswissenschaften, we have added a special reference. Appendix IV in Menger’s Investigations is particularly illuminating in that regard.]
4. [Ideenkreise]
5. [Versunkenheit]
6. [Fachgenossen]
7. [Erkenntnisswege]
8. [historisierende Tendenz]
rediscovered itself, its notion, and its methods, and as long as it further maintains for itself the spirit of universality—a spirit which makes use of the results of other research and those of other fields of knowledge for its own purposes, especially those of history and statistics—then we must not fear the further development of the discipline.

The following methodological letters are dedicated to the above purpose. They are intended to place under the light of criticism a scientifically most boggy field of historicism in German economics, which has been defended with extreme measures of intolerance and impropriety, i.e., historicism’s most recent outgrowth, to provide an appropriate answer for unqualifiable attacks which have not been provoked by anything, at least not anything in such a manner.

In this short essay, primarily intended as a defense, I have avoided the obvious temptation to treat the actual method of exact research in the field of theoretical economics. In the *Investigations into the Methods of the Social Sciences* I have sought to offer the proof for the justification—heavily contested by the historical school—of the above-mentioned direction of theoretical inquiry in the field of economics, reserving a detailed depiction of the respective paths of inquiry for a separate essay. The preliminary observations about this have nevertheless become the object of lively discussion among the evaluators of my methodological investigations—a pleasant sign of interest in the above-mentioned important branch of theoretical research in the field of German economics, despite the dominance of the historical school. I will now seek to speed up the fulfilment of my promise because the one-sidedness of our historical economists may only be countered in a decisive manner through complete clarity over the goals and paths of inquiry in exact economics. In this pursuit, I will also find the opportunity to consider the competent remarks by E. v. Böhm, Emil Sax, W. Lexis, H. Dietzel, and others which have been made to oppose specific parts of my argument.

Vienna, January 1884.
The Author.

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9. [*Begriff*]
10. [*unqualifizierbar*]
11. [*Erkenntnisstreben*]
12. *Investigations*, p. 62 [43]. [The numbers inside the brackets correspond to the pagination in the German original, the numbers ahead of the brackets correspond to the English translation *Investigations into the Method of the Social Sciences with Special Reference to Economics*, NYU Press, 1985. If Menger uses direct quotations from the *Investigations*, we reproduce their formulation in the English translation.]
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[On the final page of the book (p. 88), Menger lists ten errata. We have incorporated them into the text without comment.]
First Letter.

[Introduction: External motives for these letters. About the utility which could be drawn for the scientific discussion even from shallow criticisms by evaluators not sufficiently versed in the subject matter] [1]

You write, my friend, that the best response to the equally inconsiderate and challenging critique which my Investigations into the Methods [2] has received from the editor of the Berlin Jahrbuch für Gesetzgebung [3] is to overlook it with silence, which is the most effective answer to attacks of this kind.

Whoever read my book, even if cursorily, you suggest, would gather on their own to what extent the attacks by Schmoller are grounded in knowledge of the subject and impartiality, and would be able to base their judgment upon that. But his critique would receive the appropriate appreciation even by those who do not know my Investigations, as it originates from a man whose scientific erudition has long been acknowledged appropriately in academic circles, despite his constant reminders of the historical and philosophical [2] studies to which he has devoted himself, of the lectures on methods for which “he was just arming himself,” and more of the same. You say that reviews of the kind which Schmoller has handed over to the public for years without sufficient learning, filled with invective and obviously without taking any responsibility, are harmless for those knowledgeable readers which we first seek to reach with our scientific publications, and any response to them is beneath the dignity of a serious scholar.

Allow me, my friend, to nevertheless be of a somewhat different opinion in the above matter. I do not want to debate whether such reviews are harmful or harmless for the authors of the reviewed contribution. Consider them harmless for the author or even as amusing incidents of academic life. From that it does not seem to follow, however, that one should leave them completely unacknowledged. What may be harmless for the author of a piece, may still be harmful for the subject he is advancing; and even if that were not the case, why should we disdain to use for a matter so close to our hearts what does not harm it? Critiques by knowledgeable authors are useful to us in the sense that they correct and instruct us, and thereby

deepen the scientific discussion. Why should critiques of the kind that Schmoller publishes merely be harmless and not also provide utility, if not of course of a completely different kind?

[3] Every scholarly piece has a certain intellectual level below which the author descends only reluctantly. Not all formulas are explained in mathematical writings, the knowledge of positive law is required in legal writings, and generally much ability and knowledge are presupposed in scientific writings. Here lie, painfully recognized by every reasonable author, the limits for the understanding and the dissemination of his ideas. Shallow attacks directed at us by ignorant critics do, however, offer us the welcome opportunity to push back those limits, and that all the more effectively the closer the evaluator is to the less knowledgeable readers and the more ruthlessly he believes himself to be acting against us.

In such reviews objections are raised against the results of our research, objections which the author also had in mind but which he refrained from responding to, due to the erroneousness of the objections being obvious to the knowledgeable expert. When such objections are raised by a critic with strong emphasis, as is usually the case, then we are in a position to engage with them without violating the respect we owe the readers of scientific writings. Objections and attacks of the kind described above thus offer the opportunity to raise our ideas to the level of general intelligibility, which is not otherwise typical in scholarly writings and superfluous for the actual scholarly audience, yet which is not without utility with respect to a certain portion of the readership of scientific writings.

[4] But critiques of the kind I am talking about here do us another, incomparably greater service. Objections are raised in them that are so far away from the spheres of thoughts of serious scholars that no one who has some learning in the subject, and least likely the author himself, would ever come to think of them. These are, however, objections that usually arise from a strange harmony of thoughts in the minds of all superficial readers of scholarly writings not sufficiently acquainted with the materials treated.

It is through critiques of this kind that we thankfully arrive at knowledge of the greatest misunderstandings that our work is subjected to in certain circles of readers and, in this way, obtain the opportunity to counter them effectively. Critiques of the category I am speaking of here play the role of a certain figure in Italian comedy, who seems to inhibit the plot through its partially misleading, partially ill-natured interjections, but actually promotes it in an equally effective and exhilarating manner.

Indeed, it is not common that a well-known author of manifold laudable merits assists an author who is anxious about the distribution of his ideas by taking

15. [Gedankenkreise]
on a role of such secondary nature in the scientific discussion; it is nothing short of a stroke of luck when our opponent is a scholar who, because of the external means of power he unifies in his hands and because of the manner in which he uses them, is praised by the small and fearful, and wisely left uncommented-on by the stronger. [5] Thus, our interest in promoting our scientific endeavors is joined with our interest in cleansing the literature of the influence of a shallow kind of 

[Revision not numbered]

Thus, our interest in promoting our scientific endeavors is joined with our interest in cleansing the literature of the influence of a shallow kind of reviewerdom\[16\] not qualified for the high tasks of scientific critique.

And I should let this opportunity pass by completely unused, which is offered to me by the editor of the Berlin *Jahrbuch* in such an unintended manner, an opportunity to remove a series of misunderstandings and errors about the foundational problems of our discipline, and perhaps even to repair some other “historically evolved”\[17\] obstacles to an appropriate scientific discussion in the field of economics in Germany?

**Second Letter.**

*Continuation: About disfigurations in the field of scientific criticism and how to counter them* \[6\]

With kind concern, you alert me to the fact that a dispute with Schmoller would not merely have a scientific side, but also a completely different one. According to you, there is no other scholar in Germany, or anywhere else, who would be as ruthless in his choice of means when it comes to fighting an opponent. You say that I should be prepared for every possible and impossible disfiguration of my words, and that Schmoller is a master of a very personal and vulgar style of writing, of which I have just received downright frightening samples—as an aside, this may be the only mastery one can attribute to this man regarding his German.

You are right, my friend, when you consider a scientific discussion with Schmoller as not merely scientific;\[18\] seeing that this man is more than known for his pronounced tendency to misinterpret the opinions of others, and as a representative of impropriety in the field of scientific polemic.\[19\] [7] Truly, not

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16. [Recensententum]
17. [“historisch geworden”]
18. [scientifich]
19. In his reviews of my writing, Schmoller does not limit himself to swearwords like “unworldly ivory tower naïveté” [weltflüchtige stuhnglehrte Naivetät], “scholastic thought exercises,” “blinders of scientific effort,” [7n.] “abstract schemes,” “mental tuberculosis,” and more of the same, but intimates to me, apparently in order to strengthen the force of his arguments, that I would be “immediately thrown out” of every group of exact scientists due to my methodological views (*Jahrbuch*, p. 243). The specific part of his critique, which provides proof that Schmoller earned his spurs in his scientific career in tradeschools.
without some hesitation do I take steps towards fighting off this side of his attacks against me. [8] There are situations, however, in which it would be a betrayal of one’s own cause to remain silent. I would be more than happy to leave this unedifying business I have to complete here to someone else, if it were easy to find such a person, given the kind of critique Schmoller is formulating in the field of our discipline. It is exactly what you list as a reason for me to remain silent in the face of Schmoller’s attacks that must be an additional motive for me to raise my voice against him.

“One should grant anyone’s unearned praise”—says Lessing—“...but when such a precariously famous man will not be [9] content with his unearned honor, when the will-o’-the-wisp, which has been allowed to rise to the prominence of a meteor, now wants to scorch and burn and spread noxious fumes all around, who can refrain from indignation? And what scholar, whose circumstances allow for it, is not obliged to attest to his indignation?”

Well, my circumstances allow me to counter Schmoller’s misunderstandings, disfigurations, and improprieties in the field of economic critique.

I just beg you, my friend, to not see this as proof of even the slightest sort

[Handwerkervereine] (e.g., Schmoller: Zur Geschichte der deutschen Klein gewerbe, p. VI), not without merit for his style of writing, reads as follows: “The chemist can dare to abstract from the physical characteristics of a chemical object, but he would be immediately thrown out of the laboratory if he studied the atmosphere and said, according to Menger’s principle of isolation: ‘I am only considering nitrogen, because it is dominant.’” Those who merely know the elements of logic know that the method of isolation presupposes the isolation of only those moments of a phenomenon that are accidental, and those who have read my book know that nowhere in it do I further the nonsensical opinion that the method of isolation assumes the isolation of essential moments of a phenomenon. Schmoller’s comment is therefore not just improper and almost crude, but in addition completely inappropriate. I dare to make this remark even at the risk that Schmoller could, in a careless moment of forgetting that he is currently a member of one of the most illustrious scholarly associations, roll up his sleeves and get ready to present his dreadful arguments “immediately.”

By the way, I take it for granted that it is well known how Schmoller practices the disfiguration of the opinions of others and extremely inappropriate expressions not just against me, but quite habitually. Prof. Treitschke felt compelled, almost ten years ago, [8n.] in an open letter to Schmoller (“Der Sozialismus” und seine Gönner. Berlin 1875, pp. 102ff.) to point to the fact with numerous references that Schmoller’s polemic is “abundantly graced with personal attacks” which compelled him (Treitschke), against his inclinations and custom, to send some personal remarks ahead of his rejoinders.” And these remarks culminate in the fact “that Schmoller hurts at almost all of his opponents remarks which do not further communication.” Treitschke makes the following remarks with respect to Schmoller’s love of truth: “Like you, I would have to fill ten pages if I wanted to prove how you are exaggerating my claims here, how you are turning them on their head there, how you are portraying conditional claims as unconditional, how you are angrily holding up my own thoughts against me, as if I had opposed them, and how through such dialectic tricks you create an image of my work in which I do not recognize a trace of my true opinion.”

The fame of being the pinnacle of Schmoller’s abusive style of writing falls, however, most probably to his critique of my Investigations.


21. Lessing uses a different term here.
of heroism, because on the one hand I am of the opinion, that my “unworldly ivory-tower naïveté” can stand up to a pushiness as worldly and uneducated as it can be in the field of science, and I believe on the other hand that I have several other reasons not to fear my opponent too much. Men like Schmoller only have the ability to rise to the surface as a result of the downright desolate state of a science. Only when the leaders of a discipline are not quite sure of their ground, when serious doubts regarding their own fundamental views worry them, and when they require the leniency of inferior minds in more than one respect, are those lower minds capable of organizing the kind of half-obnoxious, half-ridiculous terrorism against the representatives of other opinions, terrorism which is currently being exercised in some of the journals in our discipline. However, I do not demand leniency from these men; I have neglected nothing to avoid even the appearance of asking for Schmoller’s leniency. What reason could I have then to fear him?

Perhaps that he could prove that I committed errors? I wish this were a threat, a most genuine threat; how thankful would I be for any instruction, if such could be found in the work of writers of his kind, writers who, as I have shown, commit errors page by page, which—but I do not want to adopt the same tone as my opponent.

Or should I recoil from the fact that Schmoller may disfigure and misinterpret my opinions? I admit that such an experience does not please an author. An erit, qui velle recuset os populi meruisse? How easy is it to take a portion of the loyal reward of honest work from an author through such “reporting”? How easy? Indeed! However, this only happens when we cede the field to the heroes of such endeavors and do not assert our right to objective reporting.

What is the editor of a scientific journal that we should quietly stand by as he disfigures the results of our scientific investigations? What else is the editor of a scientific journal if not a man who is in service to the truth and to the scientific requirements of the readership of his journal, a man who has assured of honest and unencumbered reporting and who acts in violation of his duty when he, instead of keeping his promises, disfigures the truth? And against someone like this should there not be any means of defense? No defense against the abuse of scientific outlets, the existence of which the scholarly world, and only it, makes possible through its intellectual and material support?

The means is just as simple as it is effective. It is important to expose the disfigurations of the results of our scientific investigations, rather than to silently tolerate them. If we expose the disfigurations in many cases, our readership will,

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22. [Streberthum]
23. [Translation: Lives there, who would not at applause rejoice, and merit, if he could, the public voice? (The quotation is from the Satires of Persius.)]
instead of trusting the reporting of certain reviewers blindly, get in the habit of reassuring itself of the soundness of particularly conspicuous assertions of the reviewers. In doing so, the spell of those men who disfigure the opinions of others instead of offering objective reviews is broken with one strike. If everyone does their duty in the aforementioned manner, we will soon not have to fear the likes of Schmoller. Instead, they will soon find themselves forced either to put down the critical pen or to be particularly conscientious in their future reporting. Once mistrust of such critics has awakened, they cannot help but be particularly conscientious in their reporting out of mere self-interest. What more cruel punishment can you think of for men such as these but to force them to be objective in their criticism?

Third Letter.

[About the different directions of inquiry in the field of economics] [12]

Both the historian and statistician as well as the social theorist deal with societal phenomena; how is their scientific activity distinct? What is the difference between historical and theoretical social sciences? This question, in itself important for the philosophy of science, had taken on particular importance for me. In addition to some other errors, which I will address in more detail below, opinions had appeared in the more recent economics literature in Germany which lacked any rigorous distinction between historiography and statistics on the one hand and theory on the other. A school of economists had emerged, which had earned recognized merits in the fields of history and statistics of the economy acknowledged by no one more readily than I, but which also often confused the above-mentioned disciplines with theoretical economics, and as a result of treating it like a historical discipline almost put into question the independent significance of theoretical economics.

[13] I had made it my task to counter this one-sidedness, which had become detrimental for the development of the theory of the economy. Not that I misjudged or ever underestimated the utility and significance of historical and statistical research in the field of the economy as such, or as auxiliary disciplines for theoretical economics; to the contrary, I have acknowledged the importance of this

24. [Wissenschaftslehre]
26. [Theorie der Volkswirtschaft]
kind of inquiry in the field of economics unmistakably without any restraint. What I have criticized about the aspirations of this large group of German professional peers who, known under the collective name of the historical school of German economists, 27 have taken on such a prominent place in the more recent economics literature in Germany, was the one-sidedness with which this group has devoted its mental capacity partly only to historical and statistical studies, i.e., to auxiliary disciplines of political economy, while at the same time they most regrettably have neglected the theory of our discipline, which is in such urgent need of reform, and partly they even seem to treat theoretical research in the field of the economy with misguided contempt, as if only historical research in economics were justified.

The historical school of German economists gave cause for several concerns in another related matter as well. Some of its distinguished representatives lacked any more serious separation of the theoretical and practical disciplines of the economy. [14] The boundaries of the above-mentioned fundamentally different directions of research were misunderstood, and these misunderstanding was even characterized as epoch-making progress for our discipline, not only in most of the recent doctrinal systems, 28 i.e., in the practice of depiction, but also in foundational methodological discussions.

In a third regard I also saw an error in the methodological principles of the historical school. Even those followers of the school who do not outright deny the independent significance of theoretical economics, who acknowledge the legitimacy of a science of the “laws” of the economy in addition to historical-statistical studies and socio-political research, even those followers of the historical school of German economists did not seem to be fully free from a gross one-sidedness in their conception of theoretical economics, by admitting legitimacy not to all directions of theoretical research but only to some that are in close connection to historical-statistical studies (like philosophy of economic history etc.), 30 while unjustifiably disregarding all the others, among them some of the most fundamental significance. 31

[15] To me, the historical school of German economists seemed to have lost the conception of political economy and its parts, the understanding of the

27. [historische Schule deutscher Nationalökonomen]
28. [Lehrgebäude]
29. [Wirtschaftsgeschichte]
30. See on this point the appropriate remarks of H. Dietzel, ibid., pp. 31ff.
31. “Both directions (the historical and the organic) but in particular the historical, gained ground quickly in Germany and today they dominate the German science almost completely. The manner in which they execute their domination, one can no longer deny, is not very tolerant. Every direction of inquiry that diverges from the dominant tradition is misjudged or ignored as ‘abstract,’ ‘ahistorical,’ or ‘atomistic.’” E. v. Böhm-Bawerk in Zeitschrift für das Privat- und öffentliche Recht der Gegenwart. Vienna [1884], Vol. XI., p. 209. [Menger erroneously puts the publication year as 1883.]
relationships between its parts and auxiliary disciplines but most importantly the overview of the different legitimate directions of research in theoretical economics—in short, the historical school of German economists lost its insight into the system of tasks whose solution is incumbent upon research in economics. One group of its representatives occupied themselves exclusively with the history and the statistics of the economy, in other words disciplines auxiliary to political economy, while they fancied themselves to be working, directly or indirectly, on the expansion of the discipline. Another group was busy with the solution of practical, especially socio-political problems, while believing themselves to be reconfiguring economic theory. Finally, yet another group was exhausting its mental capacity in the pursuit of certain particular directions of theoretical research which stand in the closest connection to historical-statistical studies, while rejecting all other directions of theoretical inquiry in the field of economics as misunderstanding the true goals of economic research.

To fight these aberrations of a notable portion of German economists seemed to me all the more important, since the same underlying misjudgment regarding important goals of political economy [16] was perniciously affecting the development of our discipline, which was in need of reform especially in its theoretical part. I believed to observe that as a result of the errors described above, i.e., since the founding of the historical school, theoretical research in economics in Germany was generally underappreciated, and in some parts no longer practiced at all, to the great detriment of our discipline.

The path I had to take to fight the one-sidedness and errors of the historical school did not raise doubts for me. The error of the above-mentioned German economists lies in their views of the nature of political economy and its parts, of the relationships between the parts as well as their relationship to certain auxiliary sciences of political economy, and finally in their one-sided doctrines regarding the nature of theoretical inquiry in the field of economics. However difficult and comprehensive the investigation turned out to be, the nature of the above-mentioned disciplines and their position amid the sciences had to be generally clarified before I could refute the errors of the historical school that were so detrimental for the development of political economy.

It would mean to repeat a significant portion of my deliberations on this issue in a way which would extend beyond the scope of this piece, if I wanted to treat the above-mentioned fundamental questions, which are so important for research in the social sciences in general and political economy in particular, in great detail here anew. [17] What I intend to do here is to counter the attacks on my Investigations by some well-known representatives of the historical school of German economists. Only the most recent results of my research, and only to the extent that they have become part of scientific discussion, shall be here summarized in a few words.
It is the acts, the fates, and the institutions of certain states and peoples that the historian and the statistician have to research and describe, the former from the perspective of development, the latter from the perspective of state; the theorist in the field of state and social phenomena, on the other hand, has the task to make us aware—not of concrete phenomena and concrete development—but of “manifestations” and “laws” of the respective human phenomena; the researcher in the field of practical state and social sciences, however, is supposed to teach us the “principles” of expedient intervention in state and social conditions, i.e., the principles by which certain goals, for example the maintenance of the economy and the administration of the government budget, can be realized in the most expedient way possible.

Along these lines, I said that the historian and the statistician have to research and describe the concrete phenomena of human life and their concrete relationships in space and time (the former from the perspective of development, the latter from the perspective of state!), whilst the theorist has to research and describe the manifestations of human life and the laws of the latter phenomena (the types and typical relationships between human phenomena). [18] The one working in practical state and social sciences, however, has to research and describe the principles of expedient intervention in state and social phenomena.

I did not stop with this classification and its application to economics. The central errors of the historical school of German economists concern their views regarding the nature of theoretical economics, their one-sided inclination towards particular directions of theoretical research that are closely connected to historical studies. While I initially set myself the task to describe the outlines of the complete system of problems which the human mind has to solve in the social sciences in general and political economy in particular, the narrower task then came up to determine the system of legitimate directions of theoretical research in the field of the economy. Along these lines, I elaborated that there are two main directions of theoretical research. Both have the purpose to determine the manifestations and laws of economic phenomena. The first (the empirical) is supposed to determine the manifestations of the real phenomena of the economy “in their complete empirical reality” as well as the observable regularities in the sequence and the coexistence (the “empirical laws”) of economic phenomena, whereas it is incumbent upon the other (the exact direction of theoretical research) to trace the real phenomena of the economy back to their most simple and strictly typical elements in a manner analogous, although by no means identical, to the exact natural sciences. [19] In addition, the purpose of this latter direction of theoretical research is, on the basis of the method of isolation, to outline the exact laws, by which the more

32. [Zuständlichkeit]
complicated phenomena of the economy develop from the above-mentioned elements, in order to provide the theoretical understanding of social phenomena not in “their full empirical reality” but of their economic aspects.

I have taken great care to prove the legitimacy of this latter direction of theoretical inquiry in economics, which has been zealously disputed by the historical school of German economists.

Now I do know that I can only offer my readers a highly incomplete picture of my investigations by summarizing the results of only a portion of them. After all, the main value of scientific results lies in their genetic development and methodological justification. However, even the schematic form in which I am recounting them here will be sufficient, I believe, to allow my readers to estimate the value of the attacks which my Investigations into the Method of Research [sic] have experienced from a portion of German economic criticism.

**Fourth Letter.**

*That political economy and the history of the economy are sciences which have to be strictly distinguished* [20]

The contrast between the historical and the theoretical social sciences, as I have characterized it in my last letter and as I have further elaborated in my Investigations into the Method of the Social Sciences, is not contested by Schmoller but acknowledged in his manner. He admits, that the separation of the directions of inquiry from which I start is—legitimate? No! This phrase is obviously missing from Schmoller’s peculiar reviewer argot—“of a certain legitimacy.” “But this contrast should not be understood as an unbridgeable divide.” [21] “The science of the particular”—Schmoller would “prefer to say” descriptive science—“provides the groundworks for the general theory; these groundworks are the more complete, the more the phenomena are described in all their essential characteristics, changes,
causes, and consequences. The complete description presupposes, however, a complete classification of phenomena, a complete concept formation, a correct classification of each according to the observed types, and a full overview over the possible causes. So, every complete description is a contribution to the assessment of the general nature of the respective science.”

“Of the general nature of the respective science!” What is that supposed to mean? What is “the general nature of a science”? Maybe Schmoller means the recognition of the general (the manifestations!) in any field of research? But I do not want to bother him too much with such questions. Meanwhile, what is the aim of the above-mentioned explanation with its strange terminology?

If Schmoller aimed to say in the explanations above that historical studies for theorists, and likewise the knowledge of economic theory for historians, are relevant, and thus that all progress in the field of historiography benefits theory and likewise [22] that all progress of the latter [viz., theory] benefits history, he is right, absolutely right, and the only question that could arise is, why does Schmoller like to cover such a self-evident truth in such incomprehensible language? Schmoller will not be burdening his readers to take his aforementioned sentence, even if it was put in still stranger language, for a new truth that yet has to be proven, or make them believe that it is unknown to me?

I have indicated (in my Investigations) that theoretical research in the field of the economy finds a very valuable empirical foundation in the history of the economy, and have emphasized that a more highly developed theory of economic phenomena is unthinkable without the study of the history of the economy. Also for the practical sciences of the economy (economic policy and public finance) I have stressed the meaning of historical studies in an unmistakable manner. I have explicitly labeled the historical sciences of the economy as auxiliary sciences of political economy and likewise I have labeled the latter as an auxiliary science of the former.

So what is Schmoller’s intention with the above remarks in this lecturing tone?

What is his intention with this in a critique of my book? [23] Probably he only wanted to tell his readers of his opinion that these trivialities, which he recites in a half-incomprehensible language, are unknown to me? He wants to lecture me about things of which I, for the sake of the humor which lies in certain pretensions of historical economists, have proven that they have been repeated, and are still

38. [Geschichte der Volkswirtschaft]
39. Investigations, p. 117 [123].
40. ibid., p. 163 [187].
41. ibid., p. 45 [18].
42. ibid., p. 163 [187].
repeated, again and again by authors of “practical philosophy” since Plato and Aristotle!

Yet even if the statements above were original, if the patina of two millennia did not lie on them, what do they have to do with the question of the boundaries between history and theory in the field of the economy? That the history of the economy, and not only this but countless other disciplines as well, can be labeled as auxiliary sciences of theoretical economics, and that their every progress benefits theoretical economics, yes, that even all sciences are somewhat related to one another—who would deny this, who has ever denied this? Only a very unknowledgeable evaluator would be capable of drawing the conclusion that there are no fixed boundaries at all between the individual disciplines, and that especially the historical sciences of the economy and theoretical economics may be confused with one another. Only against that, against those mistakes which our historical economists have fallen for in this respect, have I taken my stand.43

[24] No unbridgeable divide separates history from theory of the economy;44 as little separates them as anatomy from physiology, mathematics from physics and chemistry; between theoretical economics and history of the economy, and yes, between sciences in general, there is self-evidently no such unbridgeable divide as between transcendental philosophy and a Great Dane;45 yet, in every case a very specific boundary exists, insofar as it is possible for one to exist, between the sciences. The physiologist pursues scientific goals other than those of the anatomist, even though he engages with the results of anatomy for his purposes; the physicist pursues goals other than the mathematician, even though he uses the results of mathematics for his purposes; and the goal which the theorist in the field of the economy sets for himself is quite different from the one that a historian in the field of the economy sets for himself, even when he carries out historical studies for his purposes. “It is concrete acts, destinies, institutions of definite nations and states, it is concrete cultural developments and conditions whose investigation constitutes the task of history and of” (historical!) “statistics, whereas the theoretical social sciences have the task of elaborating the empirical forms of social phenomena and the laws of their succession, of their coexistence, etc.”46

Here, with regard to the tasks and goals of research, there exist strict boundaries between the above-mentioned disciplines exist, [25] which must not be blurred unless one opens the doors for confusion and the shallowest dilettantism. What I reproach the historical school of German economists with is not that they

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43. ibid., pp. 41 [11ff.]
44. [Theorie der Volkswirtschaft]
45. [dänische Dogge]
46. Investigations, pp. 42ff, [12ff].
practice the history of the economy as an auxiliary science for political economy, but that a part of its followers have lost sight of political economy itself while conducting their historical studies.

**Fifth Letter.**

**[Why Schmoller strives to blur these boundaries] [26]**

You are asking me, my friend, why Schmoller does not wholeheartedly admit the obvious statement that the historical sciences of the economy (the history and the statistics of the latter) are merely in an auxiliary relationship to political economy, why he instead attempts to blur the boundaries between these fields of knowledge as far as possible? The explanation for this, or to use Schmoller’s noble language, the explanation for his aversion “against the blinders of the scientific division of labor” is pretty obvious. No reasonable person denies the importance of historical studies for research in the field of political economy. Also, nobody denies the utility that history of the economy as such has for the understanding of economic phenomena. This, however, is not enough for the editor of the Berlin *Jahrbuch*. He wants to continue to conduct his historical-statistical miniature painting, and yet not give up the pretension of being considered a representative of political economy and in particular of the theory of the economy. Hence his aversion to the “blinders of the scientific division of labor,” but in truth against any appropriate determination of the boundaries between the history and the theory of the economy. [27] Hence also his tenaciously held opinion that the history of the economy is the *descriptive part* of political economy, although it is not at all a part of political economy, but an auxiliary discipline of the latter. To cross this difficult-to-bridge divide, he postulates the theory of the not-at-all-unbridgeable divide between writing history and doing theory in the field of the economy. “The opposition of the above sciences must not be understood as an unbridgeable divide.” With this, the question of the boundaries between historical and theoretical sciences is settled! Settled entirely in Schmoller’s sense!

*Bienheureux les Ecrivains*—I want to proclaim with Balzac at this point—*qui se contentent si facilement.*[^49] For Schmoller to be able to proceed undisturbed in his historical-statistical micrography, historically evolved and generally recognized scientific classifications are to be overturned; for him to continue to dedicate

[^47]: [Kleinmalerei]
[^49]: [Translation: Lucky are the writers who can be so easily satisfied.]
[^50]: *Mikrographie*
himself undisturbed to his Strasbourg historical walks, and still to be considered a representative of political economy, all scientific categories are to be turned on their head! No mistake! That would be worthwhile! And therefore, once again: Someone who utilizes findings of historical research for the purpose of conducting research in the field of political economy is certainly a political economist, but someone who inquires into the history of the economy itself is in this function a historian of the economy. [28] As an aside, one is obviously a scientific historian only insofar as one is familiar with the sources and the methods of historical research. That is how it is, and hopefully it will stay that way, even if it thus became clear that Schmoller fundamentally misjudged the task of political economy.

**Sixth Letter. [29]**

[The overestimation of historical studies in the field of political economy. Its causes and their disadvantages]

Even if Schmoller had admitted without hesitation the fundamental difference between the historical sciences of the economy on the one hand and political economy on the other, and in particular the difference between the historical sciences of the economy and theoretical economics, instead of obscuring an obvious truth by all kinds of evasions, the difference would still have become apparent between my views and his on the relationship of history to political economy.

There can be no reasonable doubt among fairly knowledgeable evaluators that history and statistics of the economy are merely auxiliary disciplines of political economy in general, and its theoretical part in particular, so that history and statistics of the economy have to be strictly distinguished from them; nor can there be any doubt that the historical sciences of the economy are important not only in and of themselves, but also in the above sense, i.e., as auxiliary sciences of political economy. [30] There is no auxiliary science whose harnessing is not of some importance for the purposes of research in the discipline to which it stands in the relationship considered here. This can already be found in its acknowledgement as an auxiliary science of the respective discipline. Just as no one can deny the character of the historical sciences of the economy as auxiliary sciences of political economy, no one can deny their importance for research in the field of the latter.

51. [wissenschaftlicher Historiker]
52. [Volkswirtschaftslehre]
A fundamentally different question, however, is the degree to which different directions of research into a specific field of the phenomenal world are warranted. No reasonable person would doubt that in this regard there is the possibility of understimation, but also of exaggeration.

Now I am well aware that among all the tasks of scientific discussion none is more difficult than to determine the correct limits of scientific endeavors. By its own idea, all science is infinite; every exaggeration of a direction, no matter how one-sided this exaggeration might be, has its utility and is therefore, from a certain point of view, justified. Nobody thinks of claiming that even the most one-sided commitment of the representatives of our science to historical studies has no indirect utility whatsoever for research in the field of political economy. [31] As I said before, all this is not questioned by anyone who has even some degree of experience in scientific research.

Equally uncontested, however, is the fact that the number of scholars in every nation and every period is a limited one, and that the infinity of scientific tasks is not met with an equally unlimited ability to solve them. Thus every exaggeration of particular directions of research, though justified, is synonymous with an equally one-sided neglect of others; in this sense, the almost exclusive commitment of many German economists to historical studies ought to be regarded in all circumstances as a pernicious one-sidedness, even if the “history of the economy” were indeed a part of “political economy.” Under the above assumption, this commitment should be designated as a one-sidedness, a pernicious one-sidedness at that, because it is necessarily accompanied by a similarly one-sided neglect of theoretical research in our discipline, while it is precisely theoretical economics that, because of its backward condition, needs to be urgently reformed.

The history of the economy, however, is not a part, but an auxiliary discipline of political economy—a useful, an indispensable auxiliary discipline, but only an auxiliary discipline. [32] Thus the almost exclusive commitment of erudite German economists to the treatment of the same is such an obvious one-sidedness that it is inconceivable how an opposition of opinions even emerged here.

Do you believe that after what was said here there can be any reasonable doubt about my position on the above question? Certainly not for someone who cares about the truth.

My friend, as we fight the one-sidedness of the historical school of German economists, let our rivals still complain about our not recognizing their merits in the field of history and our ignorance of the importance of the latter for our discipline. Henceforth, no sensible and unprejudiced evaluator can be doubtful

53. [Erscheinungswelt]
54. [gelehrte deutsche Völkwirthe]
about the fact that by making such assertions, Schmoller merely attempts to evade the actual subject of discussion.

What I fight is the aforementioned one-sidedness of the historical school; what I advocate is the reinstatement of all legitimate directions of human inquiry in the field of the economy. It is not me who wears the “blinders of the scientific division of labor.”

“Someone who lets Menger’s account work on them in an unprejudiced manner, and in particular not as a representative of a one-sided direction, will derive from his account the full appreciation of the mutual dependence of all directions of research as a result of the predisposition of our mind.”

[33] By contrast, someone who follows Schmoller’s literary activity in an equally unprejudiced manner will, from the half-dozen writings that he has thus far published about the development of the conditions in Strasbourg’s trades, surely have gained no less than the impression of universality.

Seventh Letter.

[About the opinion that economic history has to be fully researched before one can proceed with reforming political economy] [34]

What I fought against in my Investigations was not the repeatedly emphasized indirect utility of historical studies for research and teaching in the field of political economy, but the confusion of theory and historiography, the one-sided devotion of a substantial part of the German representatives of our discipline to the treatment of an auxiliary science.

What is the reason for this one-sidedness? Why, you ask me, has the above-mentioned error emerged, which has become so pernicious for the development of political economy in general and for its theoretical part in particular?

I do not want to speak exclusively about Schmoller and his like-minded professional peers. I have already spoken about the specific causes of the historicism of these writers. However, the conditions hinted at here are only coincidental after all; such a widespread phenomenon as historicism in the field of the German economics can only be the result of much more universal causes.

[35] The one-sided overestimation of historical studies on the part of some of

55. Investigations, pp. 28ff, [XVIII ff.]
56. [auf sich wirken lässt]
57. E. Sax, Das Wesen und die Aufgaben der Nationalökonomie. Vienna, 1884. p. 32.
58. [Entwicklung der Strassburger Gewerbeverhälttnisse]
our German economists is indeed rooted in a series of errors about the nature of political economy and the relationship of historical studies to this discipline; a series of false fundamental assumptions which prevail among our historical economists and which, however, at first glance are able to give one-sided historicism in our science the appearance of justification.

Here I would like to commemorate the widespread opinion among German economists that the path to a reform of political economy, or at least the next step towards it, is the exploration of economic history.

“When science temporarily proceeds in a predominantly descriptive fashion,” Schmoller writes⁵⁹ “this by no means implies a neglect of theory, but attention to the necessary substructure … That such work temporarily prevents part of scientific energy from further work on theory is the nature of the scientific division of labor.”

I hope I have already emphasized in a more than sufficient manner that history and statistics are important auxiliary sciences of political economy and, in this sense, a “substructure” of the latter. However, from the importance of history and statistics as auxiliary sciences of political economy, even if this importance were exaggerated in the most one-sided way, the consequences drawn by Schmoller do not follow at all. [36] If the historical sciences of the economy are important, even indispensable, auxiliary sciences for theoretical economics, it can only be reasonably concluded that research in the field of the latter should collect the results of historical research and statistics, and use them for its purposes. It would follow from this that the political economists would have to collect and use the historical and statistical material researched by historians and statisticians most diligently and carefully for their purposes—for determining the “laws” of economic phenomena, etc.

Never have the historians of all peoples paid more attention to cultural history and to cultural statistics in general, and to the history and statistics of the economic side of the life of peoples in particular, than they do in our time; never before has the volume of historical-statistical material to be dealt with by the theorists in the field of the economy been greater; never have the circumstances been so favorable, even for those branches of economic theory which are predominantly based on the results of history and statistics, as it is in the present. Indeed, social philosophers today lack historical-statistical material for theoretical research in the field of the economy less than ever before—even those who cultivate the above-mentioned branches of economic theory.

[37] That is not at all the motivation for the historical economists of Schmoller’s strict observance. Not the utilization of the results of historical

research for political economy, but historical research itself, especially historical-statistical miniature painting in the field of the economy, is what traps the spirits of the above-mentioned group of scholars who do not want to renounce their claim to be political economists. They do not want to let go of their historical micrography—there would be nothing to object to that; they want to be considered to be working in the field of political economy, not in the field of auxiliary sciences—one could even be silent about that; but they want once and for all—or at least for uncounted generations—exclusive, or the almost exclusive, control of economic history in the field of political economy—and every prudent person has to repudiate that!

In order to cloak the completely untenable point of view mentioned above in the semblance of a certain justification, the history and statistics of the economy must be stamped as the descriptive “part” of political economy, whereas in reality they are no parts at all, but only sciences auxiliary to political economy; for the same purpose, the idea must be maintained that only this “descriptive part” of economics must be worked out, at least at first or predominantly.

“In the future,” says Schmoller, “a new epoch will come for economics, but only through the utilization of all the historical-descriptive and statistical material that is now being created;” [38] in the meantime “it is no neglect of theory, but the necessary substructure for it, when in our science one proceeds in a predominantly descriptive manner.”

A. Wagner and H. Dietzel rightly protest “against this bill with a somewhat long expiration date,” 61,62 and E. Sax just as correctly observes that “it is quite a distorted thought to deny our time the vocation of obtaining a satisfactory theory of the economy until an incalculable number of studies in the field of economic history have been completed.” 63 However, it seems to me that Sax is still far too optimistic if he wants to calculate the required time period in generations. If economic history were to be completed in the spirit of Schmoller’s historical micrography—just think of the meat prices of Elberfeld! of Pforzheim! of Mühlheim! of Hildesheim! of Germersheim! of Zwickau! etc.—before one can resume work on theoretical economics, only eons would suffice. Just like astronomers had to introduce the concept of light years into their science in order to calculate their enormous distances, we economists would also have to begin to calculate at least in ages of solar systems in order to get even an approximate concept of the time periods that would be necessary to gain a complete historical-

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61. [“gegen diesen Wechsel mit etwas langer Verfallszeit”]
63. E. Sax, p. 3.
statistical foundation for theoretical research in Schmoller’s sense.

[39] It should also be taken into account that the historical material to be researched constantly renews itself as a result of the fact that economic history does not stand still, and even multiplies in a certain sense as a square function\textsuperscript{64} with regard to the upswings of the economic side in the life of a people, while the genuine Schmollerian historiography would at most be able to follow it in arithmetical progression, and thus Schmoller’s adventurous thought only seems even more adventurous.

But even if we disregard the special type of Schmoller’s historiography in the field of the economy, there still remains so much naïveté in the above thought that it will be difficult to take it seriously. Schmoller’s thought is as unqualified as that of a historian or statistician who would advise his professional peers to leave historical and statistical studies for periods on end, and to meanwhile dilettantishly dabble exclusively, or at least predominantly, in the field of social philosophy—namely for the reason that theoretical social sciences are important auxiliary sciences of historical research, but the results of the former are still deficient! According to Schmoller, the current backward state of the sciences of the economy should actually be an invitation for historians and statisticians to deal with theory, and for theorists with history and statistics! Of course! [40] Just do not work in your own field of research! This is much too common, and by the way much too arduous and difficult, while dilettantishly dabbling in foreign fields is just as noble as easy. Nothing in the world is more convenient than this “ut aliquid fecisse videatur”\textsuperscript{65} in the field of a science.

And even to that nobody would object if Schmoller did not appear with the strange pretension that his historical micrography is, whether at all or at first, the principally justified direction of research not in the field of historical sciences of the economy—one could even argue about that—but in the field of political economy!

Schmoller apparently has—I do not know why—no idea how much needs to be done in the field of political economy, even on the basis of our current auxiliary tools, and how much more important it is for our science that the material researched by historians and statisticians is used for the purposes of theory and practical sciences of the economy, rather than that new material is unearthed (by the representatives of our science!) about any peculiarities of the economy, for example about Strasbourg meat prices or certain clothier guilds.

By the way, does Schmoller seriously believe that a theorist who takes his task seriously will turn to the results of his research for historical or statistical instructions? I will not speak here about whether Schmoller’s scientific tempera-

\textsuperscript{64} [in quadratischem Verhältnisse]
\textsuperscript{65} [Translation: “Showing that something has been done.”]
ment is particularly conducive to unbiased historical research and objective
depiction of history. [41] In fact, I would like to describe his historical works as
quite respectable. Schmoller, however, will probably not indulge in the belief that
these have the guarantees of reliability that the theorist demands of historical and
statistical works—guarantees of reliability that only professional historians and
statisticians in the field are able to offer. Schmoller’s historical and statistical works
are in any case very worthy achievements; however, our praise of the author could
be much more unrestricted if these works came from a chamber of commerce
secretary, from an editor of a trade newspaper, or from the history club of some
Prussian provincial town. Historical and statistical works of such provenance are
used by the theorists from the outset with the precaution that corresponds to the
guarantees of their reliability and the expertise of their authors. However, that a
professor of political economy almost exclusively brings to light almost exclusively
such valuable work in fields whose methods he does not master completely, is
at any rate an unusual phenomenon; it would be close to ridiculous, however,
if Schmoller, for the sake of his above-mentioned works, seriously considered
himself a historian.

Truly, Schmoller’s example is not so tempting that any representative of
political economy could be induced to leave his own field of scientific research to
devote himself to dilettantism in the field of historiography!

Eighth Letter.

[About the opinion that economic history is the
exclusive empirical foundation of research
in the field of political economy] [42]

I believe that I would not exhaust the objections of the historical school
against my position in the question of the relationship of political economy to
the historical sciences of the economy if I did not consider a peculiar form of
historicism in our science, which has contributed, to no lesser extent than the form
caracterized in my previous letter, to the overestimation of historical studies and
to the one-sided devotion of German economists to the latter: I mean the widely
held view among German economists that history is the exclusive empirical foundation,
both of theoretical economics and of the practical sciences of the economy. To clarify this
erroneous view, both in terms of theoretical economics and the practical sciences

66. [sehr wackere Leistungen]
67. [Volkswirtschaftslehre]
of the economy, seems all the more important to me since the doctrine in question on the issues of methodology of our science is of crucial importance for the whole position of the historical economists.

[43] The supporters of the above-mentioned opinion seem to me first and foremost to overlook—to start with historicism in the field of theoretical economics—that in addition to history, common life experience (knowledge of motives, of goals, of circumstances determining success, and of the successes of individual economic activity) is a necessary basis of theoretical economics. The complicated phenomena of the economy are predominantly the result of the contact of individual economic endeavors, 68,69 so that the understanding of these and their interrelationships is a necessary condition for understanding the complicated phenomena of the economy. However, the history of the economy does not offer us the knowledge of individual economic processes 70 [44] or even less of their psychological motivation; indeed, for reasons which I have considered in detail elsewhere, it cannot grant us such knowledge at all.71 Only those who completely misunderstand the nature of historiography can describe history as the exclusive empirical foundation of theoretical economics.

Even less can history be described as the exclusive empirical foundation of the practical sciences of the economy; rather, it is self-evident that even a thorough knowledge of the past of the peoples would not per se allow us to identify the principles of expedient intervention in the economy, and of expedient action in it. The economic life of the peoples incessantly brings to light new tasks of economic policy and the administration of public finance, 72 which cannot be solved exclusively on the basis of studies of the past but only on the basis of recognizing the respective exigencies of political life that go far beyond mere historical and statistical knowledge, the changing view of the tasks of state action, the state of

68. [des Contactes individualwirthschaftlicher Bestrebungen]
69. Investigations, pp. 193ff. [232ff.]
70. Theoretical economics has not only to study the general nature and the general connection of those phenomena of the human economy, such as market prices, prices of bills and stocks, currency, banknotes, business crises, etc., which are phenomena of the “economy”—which is the result of the contact of individual economies connecting towards a higher unity, by the transport of goods or by the state activity directed towards the care of this organism of individual economies (Investigations, pp. 194ff. [pp. 233ff.])—but also the nature of the singular phenomena of the human economy and their connection to the phenomena of the “economy” in the above understanding of the word. Economics must also, for example, explain to us the nature of “individual needs,” the nature of “goods,” and even the nature of economic phenomena such as “use value,” which are quite subjective, and real only in the individual. How could it draw the knowledge of the nature of these phenomena and their connection to the phenomena of the “economy” exclusively from history? The opinion that history is the exclusive empirical foundation of the social sciences is a striking one-sidedness (Investigations, pp. 116ff. [121ff.]).
71. ibid., p. 117 [122].
72. [Aufgaben der Volkswirtschaftspflege und der Finanzverwaltung]
the art in technical sciences, etc. [45] The historian who is “a prophet facing backwards” cannot be the sole authority in the field of practical economics. Historicism in the above sense is also a striking one-sidedness with regard to economic policy and public finance. If theoretical and practical economics are to some extent supposed to be in line with life’s requirements upon science, then historicism is completely untenable and can only be explained by the errors of our historical economists regarding the nature and tasks of political economy.

Whoever, like the historical economists, regards theoretical economics as a “science of the parallelisms of economic history,” and who sees in practical sciences of the economy only a systematic representation of the economic goals pursued by the main civilized peoples of the past, of the measures taken in the past to achieve those goals and resulting successes, that person may indeed find his satisfaction in historical studies. Whoever, in contrast, recognizes in these endeavors of our historical economists only particular, albeit highly appreciable branches of research in the field of political economy, whoever recognizes theoretical economics as the science of the manifestations and laws of economic phenomena, and whoever recognizes the practical sciences of the economy as the sciences of the principles of expedient economic policy and of proper regulation of the state budget, that person will be able to describe history and statistics of the economy as important auxiliary sciences, but never as the exclusive empirical foundation of research in the field of political economy.

By devoting themselves almost exclusively to historical studies, our historical economists, especially our neo-historical economists, thus not only fall into the one-sidedness of putting an auxiliary science in the place of that science which they are primarily responsible for. Instead of studying the “laws of the economy” and the “principles of expedient action in the field of the economy,” they collect empirical material for identifying the above-mentioned scientific truths. Yet their one-sidedness is much greater. They deal only with one of the numerous auxiliary sciences of political economy and, moreover, with one that is able to offer us only a part of the empirical material necessary for identifying the truths of political economy, while they fancy to be working in the field of political economy itself.

The above view is comparable to that of the carter who wanted to be considered the architect because he had brought some loads of stones and sand to the building.

73. [The quotation is from Friedrich Schlegel, Athenaeum-Fragmente, Berlin, 1798, Fragment 80.]
74. [Volkswirtschaftspolitik und Finanzwissenschaft]
75. [Discussion of “parallelisms of economic history” is found in Roscher (1849, 180–182).]
76. [hauptsächliche Culturvölker]
77. [neuhistorische Volkswirte]
Ninth Letter.

[That historical-statistical material which is ordered according to certain scientific categories must not be confused with political economy] [47]

Do not believe, my friend, that the opinion that history is the exclusive empirical foundation of political economy is the last trump card that historicism has played in our science. In the same way as any one-sidedness needs to be pursued up to its most extreme inexorability, to play itself out as it were, to be generally recognized as such in the end, so too historicism in the field of political economy has not stopped at the above view. A part of our historical economists gave up the idea of theoretical and practical sciences of the economy altogether, and recognized historical depictions as the only legitimate task of research in the field of the economy. Yet even those who hold onto the idea of theoretical and practical sciences in the above field of phenomena more or less inexorably were able to take historicism in political economy one step further beyond the previously characterized position.

[48] Whoever regards the results of historical research as the exclusive empirical foundation of theoretical economics and of the practical sciences of the economy disregards the importance of all other empirical foundations, and moreover disregards the rational foundations of theoretical and practical direction of inquiry in the field of the economy. He will find in the “laws of economic phenomena” mere “developmental laws,” “parallelisms of economic history,” and in theoretical economics not a science of the “laws of economic phenomena,” but a science of these “parallelisms of economic history;” he will be guided by the one-sided view described above to regard the practical sciences of the economy not as depictions of the principles for expedient action in the field of the economy which is suitable within a given situation, but instead solely as depictions of experiences borrowed from economic history about the goals, the measures and successes of economic policy and public finance, etc.

However one-sided his position may be as a result of the views described above, he will nonetheless deny neither the existence of “laws” of the phenomena nor the existence of “principles for expedient action” in the field of the economy in general. History and statistics will also for him just be the empirical foundation on which—one-sidedly as they may be perceived—the truths of theoretical
economics and of the practical sciences of the economy still need to be researched. [49] The theoretical and practical insights will be, also according to this view, different from the historical-statistical material on whose basis they are obtained.

Even this view of the nature of our science, however, seems to not have satisfied the one-sided historicism of a number of German economists. These economists rather formulate as a postulate of research the principle that in political economy, too—in both its theoretical as well as its practical parts—“history has to speak for itself.” Historical-statistical material ordered according to certain categories is supposed to take the place both of laws of economic phenomena and of principles for the expedient advancement of the economy, or of the expedient setup of the national budget. If Schmoller demands that “economics should proceed essentially in a descriptive manner and offer the student a concrete individual picture, but ordered by concepts, types, and relationships, etc., and specialized to the point of pursuing the unique in the phenomena and causes,”[79] he is merely documenting himself as a representative of a view which is barely compatible with political economy: As a representative of a view who wants to replace theory and the practical sciences of the economy “essentially” with [50] historical-statistical material which has been ordered according to certain categories—this is no doubt the lowest position on which a social science can be based.

What, in this respect, was the path of development within the historical school of German economics? 80,81

Theory! — Theory garnished with historical-statistical notes and pierced by historical digressions! — Sheer notes and historical digressions with a claim to be a theory!

Further “progress” in this direction is, however, hardly possible.

**Tenth Letter.**

[About Schmoller’s opinion of the tasks of practical economics] [51]

My remarks on the relationship of theoretical economics to the practical sciences of the economy were also unable to find Schmoller’s agreement. I had characterized theoretical economics as the science which has to study and describe the general nature (the manifestations!) and the general connection (the regularities in

80. [*historische Schule der deutschen Nationalökonomie*]
81. [Menger’s common caption is “*historische Schule deutscher Nationalökonomen*,” so “of German economists” and not “of German economics.”]
coexistence and sequence—the laws!) of economic phenomena; in contrast, economic policy and public finance I had characterized as the sciences of the principles, the maxims, according to which—depending on the various circumstances—the economy can be advanced most expediently or the public budget be set up most expediently. For the sake of a more precise explanation, I compare the relationship between the first and the latter two sciences to the one between anatomy and physiology on the one hand and surgery and therapy on the other. Theoretical economics is in a similar way the theoretical foundation of the practical sciences of the economy, as are anatomy and physiology the theoretical foundations of those sciences which teach us the principles and procedures for expedient intervention in the human organism.

I believed that I had expressed myself to those for whom scientific works are written in a sufficiently intelligible manner. Even though it was obsolete, I added to the above-mentioned explanations the remark that the practical sciences of the economy themselves are fit for practical application, and that the above-mentioned sciences as well as the practice of politicians in charge of economic policy and of administrators of public finance are therefore not to be confused. There is the same difference between the latter as, for example, between surgery and therapy (which are also practical sciences!), and the practice of scientifically educated physicians, or as between chemical and mechanical technologies and the activity of practical chemists and mechanics.

Let us now hear what Schmoller has to object to these remarks.

He literally writes: “These disciplines (economic policy and public finance), as they are usually presented, and treated and mistreated in older textbooks, certainly want to be practical instructions at the same time; [53] the older, partially still used books were nothing but recipe collections in social policy, administrative law, and public finance. But it is a progress of more recent times that they moved beyond this; Roscher’s second and third volumes especially, as well as Stein’s and Wagner’s public finance, represent the most successful attempts to elevate these disciplines (economic policy and public finance!) to the rank of theoretical sciences.”

Schmoller hence considers it to be a deficiency of economic policy and public finance, a mistreatment of these sciences, if they, as he purports to be the case in older textbooks, “want to be practical instructions at the same time”? What else, with all due respect, should a practical science “want to be,” after all, but a

82. [volkswirtschaftliche Phänomene]
84. ibid., p. 204 [246].
85. ibid., pp. 203ff. [245 ff.].
86. Jahrbuch, p. 245.
87. In many passages, I designate the practical sciences as so-called arts and I believe also here to have
practical instruction in the above sense? There is no practical science which is per se something other than a practical instruction in the above meaning of the word, and the practical sciences of the economy are obviously no exception. They should teach us, not only *"at the same time,"* but altogether, the principles for expedient action in the field of the economy, depending on the various circumstances. [54] So, what is supposed to constitute the mistreatment of the practical economic sciences in the “older books”? Only in the eyes of a scholar in whose mind a complete confusion prevails about the nature of political economy and its parts, may the endeavor to solve the natural and proximate tasks of the practical sciences of the economy appear as a mistreatment of these sciences.

Indeed! As follows from his remarks, Schmoller can only think of the practical sciences in the predominant meaning of the word “mainly” as *recipe collections;* “the older, partially still used books are,” deems Schmoller, “nothing but recipe collections in social policy, administrative law, and public finance.”

So a science which teaches us the *principles,* the *maxims* for expedient action depending on the various circumstances, is a practical instruction in the sense of a recipe collection? Schmoller can only think of a science for expedient *principles* for the care of the economy, depending on the differences in local and temporal circumstances, or of the *principles* for the expedient setup of the national budget, as an economic recipe collection? Surgery and therapy are practical sciences, hence recipe collections? Technology is a recipe collection?

And the older, partially still used books about economic policy and public finance, except Roscher, Wagner and Stein, were nothing [55] but recipe collections in social policy, administrative law, and public finance?

How profound must the insight of an author be into the nature of the practical sciences of the economy, and his knowledge of the respective literature, for such a view to become possible!

**Eleventh Letter.**

[About Schmoller’s idea to elevate the practical sciences of the economy to theoretical ones] [56]

To ensure that economic policy and public finance can avoid being recipe collections in the future as well, Schmoller demands “that these disciplines be elevated to the rank of theoretical sciences,” which means in his sense to transform them into theoretical sciences; indeed “Roscher’s second and third volumes, Stein’s and

expressed myself intelligently enough for anyone who wants to understand me.
Wagner’s public finance already are successful attempts to elevate these disciplines to the rank of theoretical sciences. 88

Above all, I would think that all sciences, whether theoretical or practical, possess the same rank, the latter no lower than the former. Surgery and therapy, mechanical and chemical technology, economic policy and public finance pose different—though certainly not lower—demands to the scholars’ efforts and their genius; only in Schmoller’s fantasy are they inferior to the “rank” of their corresponding theoretical sciences. A ranking 89 of the sciences in the sense of Schmoller [57] does not exist at all; the practical sciences do not need any “elevation” to theories.

The sciences do not differ—which Schmoller seems to overlook—by their “rank,” but by the tasks they have to solve. The theoretical sciences have to study and to describe the general nature (the manifestations!) and the regularities in the coexistence and sequence of phenomena (the laws!); the practical sciences, on the other hand, have to study and to describe the principles for expedient action, for expedient intervention into the manifestations. In this, in the diversity of the tasks, lies the difference between the theoretical and practical sciences, and the elevation of the latter to the former is a thought approximately of the same depth as if the “elevation” of the foundation to the façade, or of a column’s capital to its pedestal, were presented as an epoch-making revolution in the field of architecture. As pointless as it would be to “elevate” surgery and therapy to anatomy and physiology, chemical and mechanical technology to chemistry and mechanics, as little can one reasonably speak of an elevation of the practical sciences of the economy to a theory of economic phenomena.

Every science can, however, in a certain sense be elevated, i.e., perfected, but not, as Schmoller imagines, by assigning to it tasks that are incumbent upon other sciences and that contradict its nature, but by solving the tasks peculiar to every science [58] in as perfect a manner as the state of human knowledge permits. Of course, this also applies to the practical sciences. These also are capable of unlimited perfection, but certainly not in the way planned by Schmoller, through transformation into theoretical sciences. In order for the practical sciences not to be recipe collections, one must base them, as I have explained in detail, 90 on the theoretical sciences: Surgery and therapy on anatomy and physiology, and yet not only on these, but also, as I have shown, on physics, mechanics, chemistry, etc.; mechanical and chemical technology on mechanics and chemistry, and yet not

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88. [This quotation on Roscher’s, Stein’s, and Wagner’s works deviates slightly from the similar quotation in the Tenth Letter.]
89. [Tschin]
90. Investigations, p. 211 [257].
only on these, but also on physics, mathematics, etc. Finally, the practical sciences of the economy (economic policy and public finance) first and foremost on theoretical economics—however, not only on this, but on all those theoretical sciences whose knowledge is necessary to identify the principles for expedient action in the field of the economy.

Practical sciences of this kind have their “rank” in the circle of sciences by themselves—they do not need any “elevation” other than the one just described, least of all Schmoller’s proposed elevation into theoretical sciences.

Schmoller belongs to those scholars who have an insurmountable aversion to the treatment of all problems arising from the nature of individual sciences. No science is good enough for him in this respect. He wants to elevate the theory of the economy to a historical science, and practical economics to a theoretical science. If he were a professional historian, he would attempt to elevate history to a “natural science;” if he were a therapist, he would attempt to elevate his discipline into a “physiology;” if he were practicing botany, he would undoubtedly seek to “elevate” it to a “zoology of the plant world.”

He is the prototype of the “problematic nature” in the field of science.

**Twelfth Letter.**

**[How Schmoller imagines this elevation] [60]**

After what has been said, you, my friend, will certainly be no less curious to find out how Schmoller actually imagines the elevation of the practical sciences of the economy to the rank of theoretical ones. Let him teach us about this himself. He literally writes the following:

*Practical economics can completely strip off the garment of an art* if it presents the special development of the German, and possibly of the German and the French-English economy of the last centuries in the field of the agricultural, industrial and trade policy, and its causes and consequences in detail. It then confines itself to essentially proceeding descriptively, but is perhaps a just as good or better educational aid for future civil servants than if it merely wanted to be an art, i.e., than if it gives free-trade or state-socialist advice.

Note, my friend, the delicious logic that lies in the final sentence of these remarks. But this only in passing. Let us examine the modalities under which Schmoller, a modern Apollo, does not want to “completely strip off” the skin of

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91. [das Gewand der Kunstlehre vollständig abstreifen]
practical economics, his Marsyas, but “the garment of an art.”

That the depiction of the special development of the German, and not only “possibly” but certainly also the “English-French,” besides probably also the Italian, the Spanish, the Portuguese, the Dutch, the American economy, etc., and namely “depiction of them in their causes and consequences in detail,” including all “fields” and periods (not only those mentioned by Schmoller!), in short and plain German: “that an economic history of the civilized peoples is adequate for its tasks” is an expedient educational tool “for the statesman and, of course, also for the future civil servant” was probably known already before Schmoller. The usefulness of the history of the economic policies of individual states and their financial systems, as well as the usefulness of financial statistics, are so beyond all doubt, so beyond all discussion for the scholar of practical economics, that Schmoller should finally spare us with that sort of thing. History and statistics are useful for scholars in the field of political economy—useful for theorists, useful for practitioners, useful for students, useful for future civil servants, useful for everyone. How many times have we heard this?

Factum est jam tritum sermone proverbium!  
[62] How should the above truth relate to the question of the “elevation” of the practical sciences of the economy to a theory of the latter?

Or is it possible that Schmoller even thinks about this elevation in a way that—? No! It is impossible. And yet, you, my friend, refer me to his own words. Should Schmoller even be of the opinion that a practical science could be “elevated” to a theoretical one by putting a historical one in its place, assigning it the tasks of historical research in the respective field of knowledge? A mammal should be elevated to a reptile by putting a bird in its place?

No! My friend, I do not believe that even Schmoller is capable of such thought evolutions, specifically at the very moment when he “prepares himself, after a long interruption, to hold his lecture on methodology of the sciences of the state.” Again, no! Such absurdity is impossible, especially in such a solemn moment for the sciences of the state! Let us read again before we trust our eyes.

Practical economics can completely strip off the garment of an art if it presents the special development of the German, and possibly of the German and the French-English, political economy of the last centuries in the fields of

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93. [Wirtschafts-Geschichte der Culturvolker]  
94. [praktische Wirtschaftswissenschaften]  
95. [Translation: That old proverb is employed in conversation! (The quotation is from Cicero’s De officiis I,10.)]  
96. [Denkevolutionen]  
97. Jahrbuch, p. 239.
agricultural, industrial, and trade policy, and its causes and consequences in detail. It then confines itself to essentially proceeding descriptively, [63] but is perhaps a just as good or better educational aid for future civil servants than if it merely wanted to be an art, i.e., than if it gives free-trade or state-socialist advice.

If in the field of any other practical science—let us take the example of surgery or therapy—a writer would conceive the idea of not basing these disciplines on physiology and anatomy (the corresponding theoretical sciences!), but of elevating them to these latter disciplines, i.e., in Schmoller’s sense transforming them into theoretical natural sciences, then all of that person’s professional peers would begin to shake their knowledgeable heads with concern. But if the same author wanted to elevate surgery or therapy to the status of physiology or anatomy in such a way that he wanted to replace them with a historical science such as ethnography or anthropohistory, [98] a most active sympathetic concern among his medical colleagues would certainly turn to him immediately. And yet, fundamentally, he just would have failed to choose the right terrain for the publication of his discoveries. Had he pronounced the same thought in the field of political economy, he would not only be able to describe it as the result of his tireless historical and philosophical studies, but perhaps even find believing souls who would be willing to accept thoughts like that for epoch-making truths.

Thirteenth Letter. [64]

[Another opinion of Schmoller about this same topic]

You reply to me that Schmoller is not only of the opinion, mentioned in the previous letter, about the way in which practical economics should completely strip off the garment of an art and be elevated to a theoretical science, but also of another opinion and that it therefore would be unfair not to mention it at this point. You are right and, in order not to tantalize anyone’s curiosity, I would like to express Schmoller’s second view here immediately. Shortly after the passage quoted in my previous letter, he writes:

It (practical economics which has completely stripped off the garment of an art) then gives (⁉) the student a concrete individual picture, but one that is

98. [Anthropohistorie]
99. [allgemeinste werkthätige Theilnahme]
100. [Wirtschaftswissenschaften]
101. [The expressions interjected with parentheses into the quotations, e.g., (?), are additional, comment-
ordered according to the concepts, types, relationships which result from the general theory of economics and that is specialized to the point of pursuing what is unique in the phenomena and causes, which are either completely absent or recede in the pale overall picture of general economics. [65] And the same is true of public finance.\footnote{Jahrbuch, p. 241.}

Schmoller might ask himself whether he suddenly changes his mind in this sentence. In order to elevate the practical sciences to the level of theoretical ones, he still wants to “strip” the former of all the principles for purposeful intervention in the phenomena of the economy, i.e., that which makes them practical sciences. He adheres to this idea. However, he—if I have understood Schmoller correctly—no longer wants to replace the practical sciences completely stripped of their “garment of an art” with economic history per se, but rather with historical-statistical depictions of the individual fields of the economy, ordered according to the categories of “general” economics.

I will apply the juridical principle “Lex posterior derogat priori”\footnote{Translation: “A later law repeals an earlier.”} to Schmoller’s remarks and assume that, whatever else seems to emerge from his remarks, he is not of his first but of his second opinion. And now you ask me what I have to say about the above-mentioned way in which Schmoller intends to “elevate” the practical sciences of the economy to theoretical ones?

Even my opponent, who so strongly dislikes the practical sciences of the economy to the point of completely negating them as independent sciences, will probably no longer burden me with dealing seriously with the above-mentioned view. [66] Certain thoughts are refuted as soon as their meaning is made clear, as soon as they are stripped of the phraseology in which their author wraps them. If someone wants to begin the elevation of the practical sciences of the economy to theoretical ones by first “stripping off” all the principles for purposeful action in the field of political economy, i.e., everything that makes the above-mentioned disciplines what they are: There is as little to argue with such a person as with a surgeon who wants to regenerate an organism by the amputation of all its organs. If “we strip the practical sciences of the economy of all the principles for purposeful action in the field of political economy,” then as much remains of them as of an economic history from which we strip it of all “depictions of historical developments,” or of a theoretical political economy from which we strip it of all “laws of economic phenomena”—i.e., the well-known knife without blade and handle.

But let us assume that Schmoller wrote down the sentence quoted above

like interventions by Menger.]
conscious of the consequences resulting from it. Let us also assume that the nirvana in the field of practical sciences indeed represents his ideal, or the first stage in his quest to elevate the practical sciences of the economy to theoretical ones: So the question arises immediately how he wants to solve his problem by starting from this negative basis?

[67] Certain historical representations more closely specified by Schmoller ought to be classified—Schmoller believes—“according to the concepts, types, relationships which result from the general theory of economics” (!!), respectively to be added to the corresponding doctrines of “general” economics.

However, someone could be so presumptuous to ask how a theoretical science can become a practical one by adding historical depictions of any kind to it?

Quidquid non est simpliciter tale, illud non est cum addito tale.  

In the addition of historical depictions to a theoretical science—so you interject—one can, at most, even in the lowest sense, recognize a historical treatment of this theoretical science; but it is not foreseeable how by doing so practical sciences are to emerge, which have completely stripped off the garment of an art and which have been elevated to theoretical sciences.

How little you can follow the soaring thoughts of Schmoller! Just listen to what he continues to write:

It (practical economics which has been elevated to a theoretical science!) then gives the student a concrete individual picture, but one that is ordered according to the concepts, types, relationships which result from the general theory of economics and that is specialized to the point of pursuing what is unique in the phenomena and causes, which are either completely absent or recede in the pale overall picture of general economics.

[68] Are you still not understanding?

You object that a theoretical science and therefore also a science of the economy has neither a concrete nor an abstract picture to teach us, but the laws of phenomena; offering us a concrete picture of phenomena, in contrast, is the task of the historical sciences. Notwithstanding that: How would that pale image of phenomena, which Schmoller calls general economics, be able to become a “practical science of the economy” by adding to it historical depictions of any kind, and even more so one that is “elevated” to a theoretical science?

You repeat, my friend, this unpleasant question. Yes, you doubt that there is a second science in Germany in which such argumentation could be seriously put forward, and put forward by the editor of a scientific journal. You think that

104. [Translation: Whatever is not like this in a simple way also isn’t like this if you add something.]
105. [Gedankenflug]
these are monstrosities which show a deep decay of abstract thinking in the field of political economy. Where, you cry out, did even the simplest, the most fundamental concepts of philosophy of science end up in the new historical school of German economists, if such things are possible?

[69] “Rarus... ferme sensus communis in illa fortuna.”

That is what you think.

But now let us listen to what Schmoller himself thinks about this: “For someone who has this point of view,” he exclaims triumphantly, “the methodological differences in the treatment of theoretical and practical economics are only gradual, not fundamental as for Menger. Who thinks and teaches like this cannot regard it the worst scientific crime to have mixed the method of theoretical and practical economics.”

[70] Joking aside, Schmoller is right: For someone who has this point of view, someone who thinks and teaches like this, everything seems indeed already to be one. There is no longer an unbridgeable gap between history and statistics of the economy on the one hand and theoretical economics on the other; economic policy and public finance have been completely stripped of the garment of an art; by adding historical depictions to theoretical economics, the practical sciences of the economy have been elevated to a theoretical science, figuring as more special parts of the more general and therefore pale “image” that theoretical economics henceforth presents to us, etc., etc.

Anyone who does not recognize in this a conception of the essence of political economy that is as deep as it is philosophical, its parts and the relationship

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106. [Wissenschaftslehre]

107. [neuhistorische Schule]

108. It should also be noted here that individual passages in Schmoller’s critique (p. 245) show that he thinks of the practical sciences of the economy, which have been elevated to theoretical sciences, [69n.] as more special parts of a general theoretical economics. In doing so, Schmoller overlooks the fact that practical sciences can never stand in this relationship to theoretical ones, but rather that both theoretical and practical sciences have general and special parts. As little as chemical technology can be described as a special or “more detailed” part of chemistry, or therapy a special part of physiology, can the practical sciences of the economy be described as special parts of a general economics. In reality, economic policy has as much a general and a special part as theoretical economics. The same applies to public finance (Investigations, p. 204 [247]).

109. [Translation: “Generally common sense is rare in that rank.” (The quotation is from Juvenal’s Satires, VIII.73.)]

110. [The quotation is from Jahrbuch, p. 246. In Menger’s original the closing quotation mark is omitted.]

111. “The dispute whether (with regard to political economy) we are dealing with a ‘science’ or an ‘art’ has been settled. Both are present: pure theory as foundational science and art as applied science; although it was only recently that one had to recall the methodological consequences entailed by the differences between the two parts of the entire field of knowledge.” (E. Sax, Das Wesen und die Aufgabe der Nationalökonomie, 1884, pp. 21 ff.). But if Sax thinks that economists can hardly disagree on this issue, [70n.] then the above-mentioned passage in Schmoller’s remarks should prove the opposite.
of these latter to each other and to their auxiliary sciences, is an immodest and philosophically not sufficiently educated person who cannot do anything better for his scientific education than to sit at the feet of the editor of the Berlin *Jahrbuch* to see and to hear how this profound methodologist "thinks and teaches.”

### Fourteenth Letter.

[About the characteristics of Schmoller's fighting style] [71]

You say that I almost seem to feel satisfaction with my opponent’s deficient learning in the questions of methods treated by him and with the confusion of his concepts; meanwhile, all this, in connection with the external influence of this man in the field of our science, provokes the most serious considerations about the present state of German economics.

I know, my friend, that it is a great sin to laugh at the ridiculous; however, it is so difficult not to indulge in a tone of derision towards a bleak and haughty opponent. However, what other tone do the remarks of such a man deserve who, without the slightest solid learning in the questions of scientific methodology, behaves as a full-fledged judge on the value or worthlessness of the results of methodological investigations? Is there in the field of science a phenomenon less suitable for serious consideration than a bloated non-science which makes severe judgments over the results of careful scientific research?

Can one discuss seriously the most difficult questions of epistemology with a man in whose spirit every quest for reform of theoretical economics, even the cultivation of the latter, appears as Manchesterism? Can one discuss, without indulging in a mocking tone, the above questions with a scholar whose entire fairly original learning in the realm of theoretical economics consists in a primeval slime of historical-statistical material, with a scholar who constantly confuses the simplest concepts of science? Should such a quarrel render me satisfaction? If Schmoller’s objections to the results of my methodological investigations were not valuable to me for reasons which I have already discussed in my first letter, how gladly I would refrain from the discussion with him that I do not desire at all and confine myself to correcting the most conspicuous disfigurations of my views in his

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112. [scientifisch]
113. [dieser tiefseinig Methodiker]
114. [Unwissenschaftlichkeit]
115. [in einen heiteren Ton verfallen]
116. [Urschleim]
Also, do not imagine that a discussion with an opponent like Schmoller would be more effortless than with a scholar who is well-versed in the questions being addressed. When interacting with a well-versed scholar, is it not easy to instruct such a scholar, or to let him instruct you? Is it not easy—relatively—to discover individual inaccuracies within the consequent thought system\(^\text{117}\) of a knowledgeable scholar, i.e., individual inconsistencies and errors, and by exposing and correcting them to contribute to the advancement of science? Moreover, how gratifying is it to be able to express to an author the gratitude we owe him for correcting our own views and for the instruction we have drawn from his writings? [73] The most difficult and unpleasant issue in the field of science is always the critical contact with one-sided representatives of practical party aspirations, with men who transfer their one-sidedness and bad habits of party fight to scientific discussion; how much more unpleasant when such opponents even appear with the claim to be scientifically superior!

In a specialized library set up by a knowledgeable hand, however rich it may be, the eye of the expert is easily able to discern individual gaps. In an arbitrarily assembled library, the eye searches in vain for a resting point and finally turns away because it does not actually provoke serious judgment. The same is true for the judgment of an author’s knowledge. The strength of Schmoller’s methodological standpoint lies in the fact that it is incomprehensible and beyond any serious criticism. Here you want to fault me for not being misled by the historical-philosophical studies which he constantly tells us about, nor by his lectures on the methodology of the sciences of the state for which he currently “equips” himself, and for not taking the methodologist Schmoller more seriously than he deserves to be?

What would you say, for example, if I were to shed more light on Schmoller’s thoughts about the actual methodological problems of our science? [74] His thoughts about the inductive and deductive methods in the field of our science?

The results of his profound investigations into the nature and certainty of these paths of inquiry in general and in political economy in particular?

From your dismay I notice how little you take the methodologist Schmoller seriously. But do not worry, you do not have to fear the worst. Whoever gropes in the dark about the goals of research in the field of economics as completely as does the editor of the Berlin Jahrbuch, his thoughts about the paths of inquiry in the field of our science are protected against any attack.

\(^{117}\) consequentes Gedankengefüge
I would like to evoke only a few remarks by Schmoller that refer to the epistemological problems I have already dealt with because they are extremely characteristic of the way in which he expresses criticism and of his fighting style.

I described that the task of the historical sciences is to examine and present the individual nature and connection of human phenomena (their individual relationships in space and time!).

Here arose for me naturally the interesting question, which had already been raised many times by the scholars of methodology of the historical sciences: in what way were the historical sciences able to solve their task in the face of the innumerable individual phenomena of human life?

Regarding the above-mentioned question, the majority of authors believe that the historian [75] has to present the more important human phenomena and set aside the less important ones, and in doing so let himself be guided by his own fine sense, since there is lack of an actual principle for the choice of the “historical” phenomena in contrast to those phenomena whose depiction is not a matter for the historian.

I had come to believe that I had found a solution to the above interesting question in such a way that the historian should not only investigate a part of the human phenomena, since doing so would contradict the principle of the universality of the sciences. Rather, the historian should present the totality of the human phenomena, but all this from a collective point of view. I said:

The historical sciences can respond to their task in universal fashion only under the presupposition of collective consideration of human phenomena and the historical economic sciences only under the presupposition of collective consideration of economic phenomena. This evidently must be the case if we consider the vast number of individual phenomena of human life or economy and the exigencies of the technique of scientific presentation. The historical economic sciences are just because of their universal-scientific task [76] necessarily presentations of human economy from the point of view of a collective outlook.

And in a note about the above remarks I say:

118. Already Pliny the Younger (Letters, 5.8.9–10) writes, not without any relationship to our question: Habit quidem oratio et historia multa communia, sed plura diversa in his ipsis, quae communia videntur. Narrat sane ipsa, narrat haec, sed aliter. Huic plerique humilia et sordida et ex medio petita, illi omnia recondita, splendida, excelsa convenient. Hanc saepius ossa, musculi, nervi, illam tori quidem et quasi inae decent! [Translation: Oratory and history have many things in common, but they also differ greatly in the points that seem common to both. There is narrative in both, but of a different type; the humblest, meanest and most common-place subjects suit the one; the other requires research, splendor, and dignity. In the one you may describe the bones, muscles, and nerves of the body, in the other brawny parts and flowing manes.]

119. Investigations, pp. 209ff. [253ff.].
Here also the basis for solving a problem frequently concerning historical research is to be sought: which phenomena of human life is it the task of the historical sciences to lift out of the vastness and to present? These disciplines really have the task of presenting the individual phenomena of human life from the point of view of collective considerations, and of presenting the individual phenomenon only insofar as it is per se significant for the collective image of human life. Only in this way can they satisfy their particular task universally.

What is called the artistic problem of writing history is also sufficiently explained in the above conception of the nature of history and its relationship to the individual phenomena of human life: “The real art of the historian (also of the statistician!) consists chiefly in the ability to make us aware of the immense number of individual phenomena of human life from the point of view of a collective outlook. It consists in the ability to offer us a collective picture of the development and of the condition of human phenomena in their totality.”

[77] This theory that I have put forward seems to have pleased my critic to some extent; he is so far away from fighting it that he rather accepts it without reservation. However, in what way characteristic of this man’s fighting style?

“Menger”—writes the same—“does not see that all of the more important economic phenomena are so extensive in space and time that they are only accessible by a collectivist view, as history and statistics are able to. That is closed to him.” For this I was lacking the organ!

There you have Schmoller! The full Schmoller!

That a critic opposes, in a tone of angry superiority, an author with the thoughts he himself clearly expressed—Lessing somewhere says: “sprinkles the author with his own fat”—is a shabbiness which is not quite unusual for a certain category of reviewers; but that a critic denies someone the knowledge of his own theory, even denies him the organ for understanding it, this is a phenomenon which is unparalleled even in the present desolate state of a part of scientific criticism in the field of political economy in Germany.

Fifteenth Letter.

[Continuation] [78]

You interject that a procedure like the one I outlined in my previous letter might almost be unbelievable, since one cannot assume that a scholar who to some
extent values his academic reputation would use such extravagant means in order to strike such a tone of superiority towards a scholarly opponent, i.e., for a small and—considering the possibility of correction, only passing—thrill of vanity. How little you know Schmoller! As if he had not been fighting every academic opponent in exactly the same manner for more than a decade now! Read, my friend, the citations from my work, as well as those from his review, follow his other critical activities and you will stop being surprised.

And still, one could ignore these and other things if Schmoller’s attacks did not present a much more disquieting aspect.

[79] I leave aside that Schmoller, whenever he makes my academic positions the subject of his review, makes me often say the opposite of what I actually said, that he confronts me in a lecturing tone me with things which I claim myself, that he reprimands me for things which he praises others for, as well as all other artistic resources of Schmollerian critique. Obvious as the misunderstandings and misinterpretations of my views may be which I encounter in Schmoller’s critique, and plausible as the question how justified the misleading and thoughtless scribblings in an academic discussion may at best be, I do not want to draw any conclusion about the love of truth of the editor of the Berlin Jahrbuch. Partiality in preconceived opinions, superficiality of reading, inadequate learning in the discussed material, the bad manners which regularly result from doing critique as a craft, and a character apparently predestined and educated for the lower forms of party fight rather than for academic discussion: With men like Schmoller, all these circumstances combined allow even the most obvious misinterpretations of someone else’s opinions as mere errors to excuse.

The case is different when we deal with untruthful claims where every misinterpretation is excluded from the outset by the nature of the matter, untruthful claims whose only purpose is to prevent the rightful appreciation of an author and the results of his research by his professional peers. [80] Untruthful claims of this kind are intrigues and no rebuke can be too harsh, no ridicule too bitter when these claims are to be denounced.

Schmoller accuses me by claiming that I “complain about W. Roscher and B. Hildebrand’s historical work,” he tries to invoke in his readers the impression that I “dismiss” Knies with just a few words, he calls me a follower of Manchesterism, he imputes to me sympathies for the mysticism of Savigny’s

122. [In belehrendem Tone]
123. [handwerksmässig betriebene Kritik]
124. [Cabalen]
126. ibid., p. 250.
127. ibid., p. 241, p. 251.
Volksgeist, and so forth.

All these accusations are utterly unfounded, insinuations for which there is not the slightest foundation in my work.

I am supposed to have complained about Hildebrand’s and Roscher’s historical work? The truth is that nowhere did I speak a word about Hildebrand’s historical work, while I explicitly acknowledged Roscher’s “outstanding merits and his advancement of the historical understanding of a number of important economic phenomena” (Investigations, p. 189 [225]).

I am supposed to have “dismissed” Knies, according to some words cited by Schmoller? The truth is that I call Knies the most outstanding methodologist of the historical school of German economists (Investigations, p. 191 [228]). I do not criticize his work in just a few words, as cited by Schmoller. Instead, I dedicate to it multiple pages of my work and conclude that Knies completes the sphere of ideas of the historical school regarding the methodology of political economy. I conclude that what since his time has been brought to light by the investigations into the methodological problems of historical economics, this author [Knies] had at least already hinted at.

Now you see, my friend, what is the matter with Schmoller’s claim that I have “dismissed” Knies through the words cited by him. This allegation is just as untruthful as all the other allegations made by Schmoller.

Can it be true that these too are only simple errors? Be well aware, my friend, of the tendency resulting especially from these highly personal allegations against me!

But I almost forgot to mention that I do not just call Knies the most outstanding methodologist of the historical school of German economists, and the new ones only as his epigones; among those latter, and truthfully in second place, I also named Schmoller: Him, in second place, the editor of “his” Jahrbuch! Reckless me, I did not only refuse him the accustomed glorification, but I nothing short of violated the considerations which I owed to Schmoller’s privileged position—an action which obviously triggered certain sensitivities. “If he is itching, he shall scratch,” so I thought in an adequate appreciation of this economic Rhadamanthus. But Schmoller does not want to appear sensitive, which is why he scratches Roscher, scratches Knies, and even scratches the dead Hildebrandt—because it is actually his own wounded vanity which is itching.

\[128. \text{ibid.}, \text{p. 250.} \]
\[129. \text{[Ideenkreis]} \]
\[130. \text{Investigations, p. 192 [230].} \]
Spare me, my friend, the necessity to defend myself against Schmoller’s accusation that I am a follower of the Manchester party\textsuperscript{131} or [83] of the “mysticism of Savigny’s spirit of the people.”\textsuperscript{132} Both accusations are completely pulled out of thin air. If anything reconciles with Schmoller’s influence on our science, spiteful in so many respects, it is the fact that he fights with unmistakable devotion side-by-side with venerable men against social evils and for the fate of the poor and weak, a fight in which, as different as the direction of my research may be, my sympathies are completely on this side. I want to dedicate my limited capabilities to inquiring into the laws that govern the economic life of men; however, nothing would be further away from the direction of my research than serving capitalism’s interest. None of Schmoller’s charges is more untruthful, no accusation more frivolous than the one that I am a follower of the Manchester party; unless the simple pursuit of establishing the economic laws, or the reference to the necessity to seriously bear in mind the previous achievements of civilization when all economic reforms are considered, is sufficient to justify the above accusation—an idea, however, which would only originate from a truly dissolute mind.\textsuperscript{133}

Further, considering Schmoller’s accusation of me being a follower of the “mysticism of Savigny’s spirit of the people,” [84] I have not only not argued for it, I have explicitly argued against it. I literally write (p. 177 [208] of my Investigations):

Against these efforts of the Smithian school there was revealed to our science a vast realm of fruitful activity in the sense of the orientation of Burke-Savigny—\textit{not in the sense of simply maintaining what had organically developed as unassailable, as if it were the higher wisdom in human affairs as opposed to the intended...}

\textsuperscript{131} Being a follower of the so-called Manchester School is definitely not dishonorable; it merely means holding on to a set of scientific convictions the most important one of which is that the free play of individual interests as the most beneficial to further the economic common good. Social philosophers who are far superior to Schmoller intellectually and led by the most noble love of truth have maintained the above principle and the resulting maxims for economic policy. As I said, being called a follower of the Manchester School does not imply per se the slightest accusation.

It is different when it comes out of the mouth of such a one-sided partisan of the so-called socio-political variety like Schmoller. Manchesterism in \textit{his} mouth is the stigma with which he denounces all dissenting minds, an abusive word he throws towards his opponents—whenever he lacks arguments.

Rightfully, H. Dietzel (Hildebrand’s \textit{Jahrbücher}, 1884. N.F. VIII. p. 110) therefore protests the fact that the stigma of Manchesterism is thrown at those who deal with the exact analysis of economic phenomena.

In my opinion, Manchesterism has roughly as much to do with question of the justification of an exact theory of the economy as a gunpowder conspiracy has to do with the legitimacy of theoretical chemistry.

\textsuperscript{132} ["Mysticismus des Savigny’schen Volksgeistes"]

\textsuperscript{133} At several points in my \textit{Investigations} I argue against the so-called “ethical” direction of political economy, while I strictly separate it from the “socio-political” variety of economic research (p. 189, fn. 123 [226, fn. 123]).
ordering of social conditions. The aim of the efforts under discussion here had to be, on the contrary, the full understanding of existing social institutions in general and of organically created institutions in particular, the retention of what had proved its worth against the one-sidedly rationalistic mania for innovation in the field of economy. The object was to prevent the dissolution of the organically developed economy by means of a partially superficial pragmatism, a pragmatism that contrary to the intention of its representatives inexorably leads to socialism.

I believe that I have not defended the aspirations of “Manchesterism” and “mysticism” in the field of the economy but fought against them in adequate manner on the basis of my scientific point of view; and yet, Schmoller throws the accusation of mysticism and Manchesterism at me—this popular socio-political Hep! Hep! which the editor of the Berlin Jahrbuch cries out every time and at any place no matter how inappropriate, [85] whenever the arguments desert him.

I believe, my friend, we now have clarity about Schmoller’s impartiality in scientific critique. His propensity to misunderstand is clearly not the most regrettable side of his critical influence in the field of our science.

Sixteenth Letter.

[Closing remarks] [86]

“We are done with the book!”—these are the triumphant words, expressing the most noble gratification, with which Schmoller closes his critique of my work, a critique which might have no equal at least among the academic literature when expertise and objectivity of judgment are considered.

The future, and I hope not a too distant one, will judge whether Schmoller was “done with” my methodological inquiries or whether I “was done with” the methodologist Schmoller. It almost appears as if the development of the methodological dispute so far which I reignited with my investigations shows that the editor of the Berlin Jahrbuch put on the toga picta and the toga palmata134 prematurely, yes, that he who performed as the roaring lion of the historical school did a disservice to it.

However this may be, one thing is certain to me already. May the methodologist Schmoller in the future pace the sands of the Spree as lionishly as he wishes, [87] shake his mane, lift his paw, and yawn epistemologically, only children and fools will still take his methodological gestures seriously. But through the wide

134. [In ancient Rome, this formal attire, the “toga picta” (painted toga) and the “toga palmata” (toga ornamented with palm twig motifs), was worn by generals on the occasion of their triumphs.]
cracks in his scholarly mask some inquisitive minds\textsuperscript{135} will peck, unfortunately also some gapers,\textsuperscript{136} and contemplate the true appearance\textsuperscript{137} of this epistemologist with amusement and gratification.

But my small effort will be rewarded through the awareness of having done a service, in more than just one regard, in the field of German economics.

References


\textsuperscript{135} [Wissbegierig]
\textsuperscript{136} [Neugierig]
\textsuperscript{137} [Gestalt]
Carl Menger (1840–1921) was an influential thinker and professor at the University of Vienna. His *Principles of Economics* of 1871 was a breakthrough in economic theory, notably for its marginalist approach and its subjectivist approach to value. His individual-centered conception of the economy was soon perceived as revolutionary and has ever since attracted generations of scholars to expand on this conception, with his immediate and closest associates being Eugen von Böhm-Bawerk (1851–1914) and Friedrich von Wieser (1851–1926). Following the publication of his *Investigations into the Method of the Social Sciences with Special Reference to Economics* in 1883, Menger engaged in what later would become known as the Methodenstreit, a controversy which, among others, addressed the relative roles of theory and history in economics. His principal opponent was the head of the Younger Historical School, Gustav Schmoller (1838–1917). In the decades after the Methodenstreit, Menger remained influential, both as teacher and as a public figure, but did not publish any new treatises. After the birth of his son Karl in 1902, Menger increasingly withdrew from public life, and Wieser succeeded him at the University of Vienna as the professor of economic theory. After his death, his partner Hermine Andermann sold his library to Hitotsubashi University in Japan. His archives are preserved at Duke University. Between 1933 and 1936, Friedrich A. Hayek edited the four-volume Collected Works of Carl Menger. Menger is widely credited as one of the founders of the Austrian School of economics.
What 21st-Century Works Will Merit a Close Reading in 2050?: First Tranche of Responses

Econ Journal Watch

prologue by Daniel B. Klein

“To a generous Mind nothing is so agreeable as to commend the Works of others, and to be the Means of ushering into the world such happy Productions, as thro’ their prevailing Merits must in Process of Time be esteemed by every Body.”


Edmund Burke died 223 years prior to this year 2020. Today, Burke is certainly not esteemed by “every Body,” but many still read him. Some in 2020 even find his work timely.

Two hundred twenty-three years from now will be 2243. Will any 21st-century authors to date still be read in 2243? If so, which authors?

Let’s start small and ask about 30 years from now, the year 2050: What 21st-century works will merit a close reading in 2050? That is the question asked of *Econ Journal Watch* authors (specifically: those who authored material in sections other than the Comments section of the journal). So far we have invited such authors with last names beginning A through K. The invitation to authors L through Z will go out soon.
Our invitation clarified the question as follows:

If you were to provide a reading list for someone who in 2050 was aged 40 and who had already come to an outlook like your own, what works published 2001–2020 would you include? What 2001–2020 works would you urge such a person to read if he or she hasn’t already?

Clarifications:

• Assume that the person already basically shares your moral and political sensibilities.
• You may select up to ten works.
• Regarding any of the works you select:
  ◦ The work may be a book, an article, a chapter, or any other written form.
  ◦ It may not be authored or coauthored by yourself.
  ◦ It need not be confined to your own outlook. A listed work may be of whatever flavor.
  ◦ It may be from any discipline, represent any point of view, and may even be fiction or poetry.
  ◦ It may be of any language.
• We also encourage brief remarks or annotations about:
  ◦ your reasons for selecting the works,
  ◦ commentary on the selected works, and/or
  ◦ reflections on making such a list.

We intend to publish the responses and to reveal the identity of the provider of each and every response.

In this tranche we have nine replies, from Niclas Berggren, Arthur Diamond, Lanny Ebenstein, David George, Hannes Gissurarson, Charles Goodhart, Jimena Hurtado, Daniel Klein, and Arnold Kling.

response from Niclas Berggren

This reading list contains ten works that have shaped my way of looking at the world and that I consider essential reading for people in 2050 who largely share my moral and political sensibilities. There are certainly important questions, treated by some of my selections, to which neither classical liberalism nor political economy provide unequivocal answers. Can classical liberalism and political
economy be enriched by and gain insight from these kinds of issues? Can those questions be aided by classical liberal or economic thought? Those are some basic thoughts that have motivated me in making my selections. Each selection concerns a topic of great importance to me and is accompanied by a brief motivation. The list is provided in alphabetical order.

Acemoglu, Daron, Simon Johnson, and James A. Robinson (2005). “Institutions as the Fundamental Cause of Long-Run Growth.” In Philippe Aghion and Steven N. Durlauf (Eds.), Handbook of Economic Growth, Volume 1B. Elsevier: 385–472 (link). For its simple yet convincing exposition of the importance of both political and economic institutions for the workings of the economy, and of how they develop dynamically.


Benatar, David (2006). Better Never to Have Been: The Harm of Coming into Existence. Oxford University Press (link). For providing a thorough and provocative challenge to notions of the positive value of one’s coming into existence. Might it be better never to have been born?


Carvalho, Jean-Paul (2013). “Veiling.” Quarterly Journal of Economics 128(1): 337–370 (link). For providing a crisp (mathematical) analysis of how regulations of veiling can result in unintended consequences: While compulsory veiling can reduce religiosity, bans on veiling can increase it. Might freedom be the better policy?


Hollinghurst, Alan (2004). The Line of Beauty. Picador (link). For providing an exquisite formulation of how two yearnings in life—for beauty and power—worked, individually and in relation to each other, during the Thatcher years. Conflicts, explosions, purges, pleasures and social liberation emerge. Due to or in spite of politics as such?
Kramer, Matthew K. (2003). *In Defense of Legal Positivism: Law Without Trimmings*. Oxford University Press (link). For providing a modern clarification of what legal positivism is and is not, and of what it does and does not require of us. Is morality overrated as a guide to human behavior? Or does it take forms other than conceived by many?

Strawson, Galen (2010). *Freedom and Belief*. Oxford University Press (link). For providing a strong defense of the propositions that there is no free will and that as a consequence there is no ultimate moral responsibility. How does liberalism fit into a world in which this holds?


**response from Arthur M. Diamond, Jr.**

The most durably useful books from the past 20 years, books that will still be worth a careful read 30 years from now, are mainly those that are rich in empirical detail on important topics. So it is no surprise that eight of my 10 books are detailed histories, and the other two are full of meaningful examples and case studies. The histories are on the topics I consider most important for human betterment: invention, innovative entrepreneurship, and medical breakthroughs. The other two books, the ones full of meaningful examples, were written by two friendly antagonists in psychology on crucial topics of how we know and discover.

Many other books of the last 20 years have made substantial contributions, successfully overturning widely accepted views on important issues. Examples of such books would be Tyler Cowen’s *Creative Destruction*, Deirdre McCloskey’s *Bourgeois trilogy*, Amar Bhidé’s *The Venturesome Economy*, and Susan Cain’s *Quiet*. They are not included below because I expect, or at least hope, that their main discoveries will be recognized and appreciated enough in the next 30 years that the discoveries will be fully incorporated in the literature of 2050, and so the original books will not be as rewarding to carefully read in that year.

DeVita, Vincent T., and Elizabeth DeVita-Raeburn. *The Death of Cancer: After Fifty Years on the Front Lines of Medicine, a Pioneering Oncologist Reveals Why the War on Cancer Is Winnable—and How We Can Get There*. New York: Sarah Crichton Books, 2015. Vince DeVita provides a passionate insider’s account of how his mentor Emil Freireich used nimble trial-and-error adjustments to develop a chemo cocktail that cured some patients of childhood leukemia, and how DeVita himself
then used the same process to develop a chemo cocktail that cured some patients of Hodgkin’s lymphoma. DeVita went on to high positions at the National Cancer Institute, the Memorial Sloan Kettering Cancer Center, and the Yale Cancer Center. So he writes with credibility when he gives insider accounts of inefficiencies and biases in the funding and management of medical science. In particular, in the chapters toward the end, he documents how Food and Drug Administration regulations block the trial-and-error process that worked in the past for him and his mentor Freireich.

Hager, Thomas. The Demon under the Microscope: From Battlefield Hospitals to Nazi Labs, One Doctor’s Heroic Search for the World’s First Miracle Drug. pb ed. New York: Three Rivers Press, 2007. The hero of Thomas Hager’s narrative is Gerhard Domagk who sought a “magic bullet” antibiotic that would be broadly effective against a variety of diseases. Through dogged trial and error, he finally found it in what they called “Prontisil.” Domagk was the medical inventor; the entrepreneur who got Prontosil manufactured and commercially available was Bayer’s Carl Duisberg. The research was funded on the expectation that a patent would bring profits. Before Prontisil was fully tested, Domagk used it to save his young daughter’s life from a deadly infection. He was awarded a Nobel Prize, but Hitler’s Gestapo arrested him so that he could not leave Germany to accept it. I predict that by 2050, someone will be alert to the opportunity to make a movie of Domagk’s story. But Hager’s detailed telling will still reward a careful read.

Kahneman, Daniel. Thinking, Fast and Slow. New York: Farrar, Straus and Giroux, 2011. Daniel Kahneman won the Nobel Prize for his research identifying many ways in which our default modes of thinking (what he calls “System 1”) are biased or otherwise flawed. My favorite example is “theory-induced blindness,” in which a clever or elegant theory seduces us to the point where we ignore or undervalue evidence inconsistent with the theory. Our conscious advanced thinking (“System 2”) can recognize and compensate for the flaws, but it takes time and effort. As we learn more about how an innovative entrepreneur thinks, I suspect we will find that, compared to the rest of us, his System 2 does a better job of detecting the biases of his System 1.

Klein, Gary. Seeing What Others Don’t: The Remarkable Ways We Gain Insights. Philadelphia, PA: PublicAffairs, 2013. When Gary Klein looks at our automatic thought processes, he sees insights, not flaws and biases. Rather than start with theories of how insights happen, he starts with a collection of important insights he has read about. He sorts his collection to see what patterns emerge. Most current accounts of insights repeat the well-worn idea that they always arise from the interaction of separate domains of thought or experience. This idea underlies the knee-jerk mantra that collaboration is the key source of innovation. Klein agrees that interaction is indeed one source but concludes that there are at least a couple
of other fundamentally different sources. One of these is when we face a desperate situation that makes innovation necessary. Another is when we observe curiously contradictory phenomena. Klein’s taxonomy will not be the final word, but his approach to taxonomizing is fresh and promising. The most important implication of his early taxonomy is that some important kinds of insights can be, and often have been, achieved by individuals, not crowds.

**Levinson, Marc. The Great A&P and the Struggle for Small Business in America.** New York: Hill and Wang, 2011. When we think about examples of Joseph Schumpeter’s “creative destruction,” what comes to mind are the innovative new goods like televisions, iPhones, and computers. But Schumpeter listed other forms of creative destruction, such as process innovations, which are not as sexy or as well-documented. An exception is Marc Levinson’s account of how the entrepreneurial Hartford brothers self-disrupted A&P’s business model several times to advance the process of grocery retailing. In doing so they increased the quality and variety of groceries, and greatly reduced prices. When FDR’s antitrust lawyers went after A&P, the Hartford brothers at first refused to hire lobbyists because they thought their beneficence to consumers was so obvious that they were invulnerable to attack. They learned they were wrong.

**McCullough, David. The Wright Brothers.** New York: Simon & Schuster, 2015. David McCullough eloquently displays much that is meaningful about the quest for flight. The Wright brothers worked hard, were self-funded, and tested their hypotheses by trial and error at Kitty Hawk. Wilbur mattered most as he derived hunches from the flight of birds, and ideas for controlling airplanes from his experience in controlling bicycles. He was modest, intense, and did not give much weight to what other people thought of him. McCullough shows that this was largely an individual achievement, against the current consensus that invention always involves simultaneous multiple discoveries. Several knew how to take off; Wilbur figured out how to safely land.

**Meyers, Morton A. Happy Accidents: Serendipity in Modern Medical Breakthroughs.** New York: Arcade Publishing, 2007. Morton Meyers shows many important examples about how breakthrough medical advances are often the result of individuals who observed a serendipitous phenomenon and had a hunch how it could be made useful. These individuals were often less-credentialled outsiders who had to persevere for long periods of time against an indifferent or hostile medical establishment. In the future, we will need to learn more about what serendipity is and how it can be encouraged and used. This book will give us some of the grist for the mill. Innovation in medicine, and elsewhere, depends on individuals having hunches about the potential uses of serendipitous events, and having the courage to persevere in following up on their hunches.

**Rosen, William. The Most Powerful Idea in the World: A Story of Steam, Industry,
and Invention. New York: Random House, 2010. William Rosen makes a strong, detailed, and well-written case that the British jurist Edward Coke in the early 1600s changed patents from depending on crony connections to the monarch, to depending on the transparent merit of inventions, a change that made it possible for tinkerers of modest means and education to invent the engines of the industrial revolution. Rosen’s book provides evidence that patents can provide funds to enable inventors to self-fund their future inventions. The evidence in the book should be important in future debates on whether to reform or abandon the patent system. The book shows that patents did, and can again, work.

Susanin, Timothy S. Walt before Mickey: Disney’s Early Years, 1919–1928. Jackson, MS: University Press of Mississippi, 2011. Timothy Susanin gives a detailed account of how young Walt Disney and his family, friends, and acquaintances, during his early years in Kansas City, pivoted back and forth, seemingly seamlessly, between employment and entrepreneurship. I see the book as providing a proof of concept that what I call a “robustly redundant labor market” can exist. A “robustly redundant labor market” is one in which unemployed workers can fairly quickly and fairly easily find another job, or an entrepreneurial opportunity, that leaves them at least roughly as well off as before. The possibility of a robustly redundant labor market is a precondition for the widespread acceptance, and hence the sustainability, of innovative dynamism.

Zuckerman, Gregory. The Frackers: The Outrageous inside Story of the New Billionaire Wildcatters. New York: Portfolio/Penguin, 2013. Detailed narratives of invention and entrepreneurship help us understand what encourages and what blocks innovation. Most such narratives from the last 20 years cover some aspect of information technology. An important exception is Gregory Zuckerman’s account of fracking. Here rough-hewn outsiders, ignored by the venture capitalists, had to self-fund their projects as they took big risks to innovate through trial-and-error experiments. Silicon Valley venture capitalists would not invest a dime to help George Mitchell develop fracking, because Mitchell did not look, talk, or act like Gates, Bezos, or Jobs. Working for Mitchell, Nick Steinsberger serendipitously used too much water in a frack. Fracking theory predicted a failed frack, but it produced way more natural gas than predicted. Instead of shrugging and moving on, Steinberger had a hunch that the theory was wrong, and that his eyes were right.

response from Lanny Ebenstein

It’s a great idea to memorialize works published since 2001 that someone should read in 2050. Here is my list:

Nicholas Phillipson, Adam Smith: An Enlightened Life (2010). Best
biography of Smith.


Ross B. Emmett (ed.), *The Elgar Companion to the Chicago School of Economics* (2010). Best work on economists and economics at the University of Chicago.


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**response from David George**

Gray, John. 2000. *Two Faces of Neoliberalism*. New York: The New Press. “Markets are not free-standing. They are highly complex legal and cultural institutions. They do most to promote pluralism and autonomy when they are complemented by other, non-market institutions. Without the ‘positive freedoms’ conferred by enabling welfare institutions, the ‘negative’ liberties of the market are of limited value” (p. 18). “Since the last decades of the twentieth century many governments…have formed their policies on the unexamined assumption that only one economic system is compatible with the requirements of modernity. In this, they are influenced by neoliberal ideologues who believe that in promoting the free market they are easing the birth of a universal economic system that history would anyway have made inevitable” (p. 23).

London: Yale University Press. “In recent years, reformers of both private and public institutions have preached that flexible, global corporations provide a model of freedom for individuals unlike the experience of fixed and static bureaucracies Max Weber once called an ‘iron cage.’ Sennett argues that, in banishing old ills, the new-economy model has created new social and emotional traumas” (front book jacket).

**Cox, Harvey. 2016.** *The Market as God.* Cambridge: Harvard University Press. “[Adam] Smith recognizes what classical theology calls *akrasia*, or ‘weakness of will.’ Because of this common human failing…it is not necessarily true that every choice, even with full information in hand, is necessarily good. As Saint Paul discerned within himself, even though he desired to do what was right, the evil he did not want is what he kept on doing” (p. 151).


**Giridharadas, Anand. 2018.** *Winners Take All: The Elite Charade of Changing the World.* New York: Alfred A. Knopf. “The wealthy targets of growing populist scorn talk often of fixing society. They are quieter about their involvement in what’s broken. In *Winners Take All*…Anand Giridharadas takes us into the inner sanctums of a new gilded age, where the rich and powerful fight for equality and justice any way they can—except ways that threaten the social order and their position in it” (front book jacket).

**Raventos, Daniel, and Julie Wark. 2018.** *Against Charity.* Chico, CA: AK Press. “[C]harity is a false, divisive gift, not least because the receiver, unable to reciprocate, cannot willingly enter into any socially binding relationship with the giver” (p. 17).


**Lind, Michael. 2020.** *The New Class War: Saving Democracy from the Managerial Elite.* Penguin Random House. “Neoliberalism is a synthesis of the free market economic liberalism of the libertarian right and the cultural liberalism of the bohemian/academic left. Its economic model…weakens both democratic nation-states and national working-class majorities. Its preferred model of government is apolitical, anti-majoritarian, elitist, and technocratic” (p. 48). “The…working-class white ethnics now found themselves defined as bigots by the same white Anglo-Saxon Protestant (WASP) elites who until recently had imposed quotas on Jews and Catholics in their Ivy League universities, but who were now posing as the
virtuous, enlightened champions of civil rights” (p. 82).

Frank, Thomas. 2020. The People, No: A Brief History of Anti-Populism. New York: Henry Holt and Company. “From 1896 to the present, anti-populists have polished an elegant archetype: the ‘undereducated class’...are different from you and me. They are obsessive and suggestible and given to fanaticism. They fall for demagogues: they join the mob: they rise up against the experts who direct the system. Economics is beyond them, as are most forms of higher reasoning. And the weakness of democracy is that it is at the mercy of such people. This is the imagined threat that Populism presented and the threat that what is called ‘populism’ will always present to the enlightened few who know how to run things” (p. 82).

response from Hannes Gissurarson

Anne Applebaum, Gulag: A History (New York: Doubleday, 2003). Applebaum treats an engrossing, though grim, topic, and she tells the history well: the labour camps of the Soviet Union under communism. She has access to new historical evidence, and uses her sources with care and compassion, but without any sentimentality. She is a worthy successor to Alexander Solzhenitsyn and Robert Conquest, the masterful chroniclers of communism.

Frank Dikötter, Mao’s Great Famine (London: Bloomsbury, 2010). Dikötter, a Dutch historian residing in Hong Kong, draws on archive material, hitherto locked away in remote Chinese provinces, to describe perhaps the worst humanitarian disaster of the twentieth century, apart from the Holocaust: the man-made famine in China under Mao, between 1958 and 1962, which claimed no less than 44 million lives. The Great Leap Forwards turned out to be a leap into catastrophe.

Thomas Sowell, Intellectuals and Society (New York: Basic Books, 2010). Marx was wrong, and socialism was an abject failure. Nevertheless, socialists, sometimes under other names, dominate the production and distribution of ideas in the West through their control over universities and the media. Intellectuals, most of whom are socialists of one kind or another, have never been more powerful. But while they make a lot of mischief, they contribute almost nothing positive to society, as Sowell, one of today’s most distinguished economists, demonstrates.

Matt Ridley, The Rational Optimist (New York: Harper Perennial, 2011). Ridley points out that we have never had it as good as we do “now.” We live in a Golden Age in comparison with the past. Food availability, income, and lifespan are up; disease, child mortality, and violence are down. This shows that progress is possible. But it is not inevitable. We have to defend entrepreneurial capitalism which with its creative powers has turned out to be the most effective means of
improving human lives.

**Niall Ferguson**, *Civilization: The West and the Rest* (London: Penguin, 2012). Ferguson provides plausible explanations for the rise of the West: competition, science, the rule of law, modern medicine, consumerism, and work ethic. The rest of the world fortunately has acquired some of these habits, but the decline of the West is not a foregone conclusion. History is not a zero-sum game. The West can flourish, if it does not lose faith in itself.

**Daniel Hannan**, *How We Invented Freedom and Why It Matters* (London: Head of Zeus, 2013). Hannan, the former leader of British Conservatives in the European Parliament, eloquently describes how the English-speaking nations unconsciously developed a tradition which could and did foster individual freedom. He traces English individualism back to the Middle Ages and identifies the most important traits in the British political tradition such as private property, representative government, and the rule of law. Liberty is a skill which has to be learned, and in the Anglosphere it was learned through preserving and extending traditional liberties.

**William Easterly**, *The Tyranny of Experts* (New York: Basic Books, 2014). Poverty will not be reduced by subsidising it, or the forces which gain from maintaining it. The choice is between development without aid (Hong Kong, Singapore, Taiwan, South Korea, Mauritius) and aid without development (most of Africa). Easterly reveals the fundamental errors in the traditional approach to development in poor countries.

**Mario Vargas Llosa**, *La llamada de la tribu* (Madrid: Alfaguara, 2018). Llosa, a Nobel Laureate in Literature, in elegant, yet passionate prose describes the thinkers who have influenced him the most: Adam Smith, José Ortega y Gasset, Friedrich Hayek, Karl Popper, Isaiah Berlin, Raymond Aron and Jean-François Revel. I can say the same for Smith, Hayek, and Popper, and I certainly do not deny that the others all wrote well and judiciously, although they did not always understand that entrepreneurial capitalism is the driving force of our Western civilisation.

**John Kay**, *Other People's Money* (London: Profile Books, 2015). A lot has been written about lessons to be learned from the 2007–2009 financial crisis, but Kay’s book is one of the best. He cogently describes modern banks as a mixture of casinos and utilities and argues that they should not be allowed to hold governments to ransom. Financial firms have to operate under the same rules as other businesses: to enjoy the profits and suffer the losses, instead of enjoying the profits and passing the losses on to taxpayers.

**Charles Moore**, *Margaret Thatcher*, I–III (London: Allen Lane, 2013–2019). Moore, a conservative journalist, has written a masterful authorised biography of one of the most important politicians of the twentieth century. Sympathetic but balanced, full of fascinating details but very readable, it illustrates that in history
individuals matter and that they sometimes play a crucial role. In 1979, Britain was the ‘Sick Man of Europe,’ but Thatcher, inspired by Friedrich Hayek and Michael Oakeshott, brought the country back to health. She accomplished this by returning to the political tradition of liberty under the law which had for centuries been the pride of Great Britain.

response from Charles Goodhart

It is not entirely clear to me why someone in 2050 might want to be reading any publications from the last two decades. On the assumption that that person might be reading it to get some idea of what we thought then, I would tentatively suggest the following list:

G. Mankiw, latest version of his textbook on *Economics*, so the reader can see what undergraduates were being taught at the time.

M. Woodford, *Interest and Prices*, the most influential book on money and macro of this period.

Kahneman and Tversky, ed., *Choices, Values, and Frames*, covering their main work on behavioural economics.

T. Piketty, *Capital in the 21st Century*, good on data, but poor on analysis. But it struck a chord and was influential in shifting the focus of thought.

B. Milanović, *Global Inequality*: much better but less influential.

Either of the biographical accounts by T. Geithner (*Stress Test*) or B. Bernanke (*The Courage to Act*) of the Great Financial Crisis. One or other of these, but not both.

A. Meltzer’s *A History of the Federal Reserve*.

L. Summers, one of his several articles on secular stagnation.

D. Rodrik, almost anything that he has written.

R. Rajan, presentation at Jackson Hole, where he warned Greenspan and the assembled conference on the potential fragility of the financial system.

response from Jimena Hurtado

In making this list I have selected the publications that have marked me as a person, a citizen and a scholar. These books have allowed me to have a better understanding, sense and feeling of the diversity of the world around me. This diversity refers to places, aspirations, identities, communities, stories, dreams, joys and sufferings that I associate with the times I have been living in. And, at the same time, they all connect our present experiences with the past, giving this sense of
continuity, of overarching quests that we share.

*El olvido que seremos*, a book by Héctor Abad Faciolince published in 2006, tells the story of the author’s family and the assassination of his father, a renowned doctor and human rights activist, who worked for the creation of an inclusive health system in Colombia and for public sanitation.

*The Swerve*, a book by Stephen Greenblatt published in 2011, gives an insightful view of the connection between ages through lost manuscripts. It shows how we share common interests and visions through time without even knowing. It is an invitation to review our views of past times. In this same line, Philipp Blom’s *A Wicked Company: The Forgotten Radicalism of the European Enlightenment* published in 2010 revisits this much studied period with questions and approaches from the first years of the XXIst century.


Another call for modesty for scholars and theorists came for me from Michael Ignatieff’s 2017 book *The Ordinary Virtues*, an extraordinary account of how people in their ordinary lives deal with ethical and social dilemmas.

Finally, I have to mention J. K. Rowling’s *Harry Potter saga* that has marked, I believe, a generation. Magic, other worlds or dimensions, intertwined with ordinary life and the possibility of forging our own destinies reflects what I identify as a desire to own our lives—just as all the epic sagas in the movies, with rebels, super heroes, and super powers, and a bit connected to Andy Warhol’s 15 minutes of fame.

**response from Daniel B. Klein**

Dear 2050 reader who is like me in political sensibilities (assuming there is one),

Greetings from 2020, from someone 58 years old who has fiddled for decades in classical liberalism. As a reader, I am slow and intensive, so it is not many books that have really come into my survey. The following are ordered vaguely by the topic’s place in history.

Paul H. Rubin, *Darwinian Politics: The Evolutionary Origin of Freedom* (New Brunswick, NJ: Rutgers University Press, 2002). A more apt subtitle might be “The Evolutionary Origin of Statism.” There has been remarkably little in the way of
forthright book-length efforts to develop Hayek’s atavism thesis, but Rubin’s book is in that vein and it opened my mind up to very important angles, and led me to scrutinize Hayek’s writings on the matter. The book is weak on epistemics, sentiment, and jural theory, but still I think it is a very important work to improve upon. Larry Arnhart’s work is noteworthy in the same vein, probably others still, but I happened to have immersed myself more in Rubin’s formulation of the matter.

Larry Siedentop, Inventing the Individual: The Origins of Western Liberalism (London: Allen Lane, 2014). A rich telling of the moral intuitions of Christianity, their departure from the ancient outlook, their development in Paul and Augustine, and onward, over centuries, in philosophizing and institutionalization, to the so-called Renaissance, and giving rise to liberalism.


Deirdre McCloskey, The Bourgeois Virtues: Ethics for an Age of Commerce (Chicago: University of Chicago Press, 2006). Part of her trilogy, teaching that interpretations, ideas, beliefs, words, discourse, and moral authorization are stuff for empirical social science, and that the liberal ascendancy in such were vital to the Great Enrichment.


Daniel Hannan, Inventing Freedom: How the English-Speaking Peoples Made the Modern World (New York: Broadside, 2013). (The title given here was for the U.S. version of the book.) Instructive even in its simplifications. A worthy necklace comes from a string of often corny beads. I’m very glad the book was written.

Arthur M. Melzer, Philosophy between the Lines: The Lost History of Esoteric Writing (Chicago: University of Chicago Press, 2014). Perhaps my #1 recommendation. Great on explaining what esotericism is, four motives to it, its techniques, and its remarkable place, and turns, in history. Also great for enabling the disentangling of esotericism from Straussianism. Parts of the book are too Straussian, too demarcationist and foundationalist, for my tastes.


Iain McGilchrist, The Master and His Emissary: The Divided Brain and the Making of the Western World (New Haven: Yale University Press, 2009). You likely know pretty well how sound is what McGilchrist says about the left and right
hemispheres. At any rate it, his analysis of the functional divisions, provinces, and sentiments makes a lot of sense to me, and his historical interpretation, picking up around 1700, has much truth in it, I think.


response from Arnold Kling

Ray Kurzweil, 2005, The Singularity Is Near. If Kurzweil’s extrapolations are correct, then the reader in 2050 will be a transhuman cyborg, who probably will find the rest of this list trivial. If instead the human race remains much as it is in 2020, then the reader should be curious to figure out “what went wrong.”

Steven Pinker, 2002, The Blank Slate. This will provide a window into the philosophical debates about psychology and anthropology that were taking place at the time.

E. Roy Weintraub (ed.), 2014, MIT and the Transformation of American Economics. This collection of essays provides valuable intellectual and social history of a department that influenced economics after World War II.

Joseph Henrich, 2015, The Secret of Our Success. This captured the state of the art in anthropology as of when it was written.

Tom Wolfe, 2004, I Am Charlotte Simmons. Wolfe was a keen observer of the sociology of the period.

Deirdre McCloskey, 2006, The Bourgeois Virtues, which is part of a larger project. Her writing is seductive, and her ideas are original and fascinating.

Russ Roberts, 2014, How Adam Smith Can Change Your Life. Including this book allows me to smuggle Adam Smith into a 21st-century list.

Brian Doherty, Radicals for Capitalism. He provides a history of important thinkers who contributed to the libertarian movement. I consider it a masterpiece of scholarship and entertaining story-telling.

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