



*Econ Journal Watch*  
*Scholarly Comments on Academic Economics*  
Volume 5, Issue 1, January 2008

Editor's Notes	1-3
<b>COMMENTS</b>	
Do Casinos Really Cause Crime? <i>Douglas M. Walker</i>	4-20
Correctly Critiquing Casino-Crime Causality, <i>Earl L. Grinols and David B. Mustard</i>	21-31
Highway Penetration of Central Cities: Not a Major Cause of Suburbanization, <i>Wendell Cox, Peter Gordon, and Christian L. Redfearn</i>	32-45
Reply to Cox, Gordon, and Redfearn's Comment on "Did Highways Cause Suburbanization?" <i>Nathaniel Baum-Snow</i>	46-50
Growth Accelerations and Regime Changes: A Correction, <i>Richard Jong-A-Pin and Jakob de Haan</i>	51-58
The EITC Disincentive: A Reply to Dr. Hilary Hoynes, <i>Paul Trampe</i>	59-65
Gulphs in Mankind's Career of Prosperity: A Critique of Adam Smith on Interest Rate Restrictions, <i>Jeremy Bentham</i>	66-77

## **ECONOMICS IN PRACTICE**

The Market for Lemmas: Evidence that Complex  
Models Rarely Operate in Our World,

*Philip R.P. Coelho and James E. McClure*

78-90

“Theory” and “Models”: Terminology through the Looking Glass,

*Robert S. Goldfarb and Jonathan Ratner*

91-108

## **CHARACTER ISSUES**

Left Out: A Critique of Paul Krugman Based on a Comprehensive  
Account of His *New York Times* Columns, 1997 through 2006,

*Daniel B. Klein with Harika Anna Barlett*

109-133

Appendix: Taking Stock of Paul Krugman’s 654

*New York Times* Columns, 1997 through 2006,

*Harika Anna Barlett and Daniel B. Klein*

I-XLV

## **THE SOUNDS OF SILENCE**

134

Entire January 2008 Issue (1.6MB)

1-134



*Econ Journal Watch,  
Volume 5, Number 1,  
January 2008, pp 1-3.*

## FOREWORD

---

### Editor's Notes

by Dan Klein

I am grateful to the co-editors Bruce Benson, Fred Foldvary, George Selgin, and Larry White and the managing editor Kevin Rollins for all their fine work, to Warren Gibson for many reports on math-intensive papers, to [donors](#) for support, and to readers for their interest and feedback.

Also, we thank these following individuals for helping provide intellectual accountability to *EJW*, its editors, and its authors.

#### Referees during 2006-2007

Ted Balaker	Reason Public Policy Institute
Jonathan Bean	Southern Illinois University, Carbondale
Niclas Berggren	Ratio Institute, Stockholm
Lawrence Boland	Simon Fraser University
Donald Bruce	University of Tennessee, Knoxville
Kenneth Button	George Mason University
Bryan Caplan	George Mason University
Roger Congleton	George Mason University
Tyler Cowen	George Mason University
Pierre Desroches	University of Montreal
Art Diamond	University of Nebraska, Omaha
Peter Gordon	University of Southern California
Robin Hanson	George Mason University
Jac Heckelman	Wake Forest University
Kevin D. Hoover	Wake Forest University
John Ifcher	Santa Clara University
Douglas Irwin	Dartmouth College
Dan Johansson	Ratio Institute, Stockholm
Thomas C. Kinnaman	Bucknell University

DANIEL B. KLEIN

Mark Martin Kocher	University of Amsterdam
Axel Leijonhufvud	University of California, Los Angeles
Robin Lindsey	University of Alberta
Michael Marlowe	California Polytechnic State University
Montgomery Marshall	George Mason University (Polity IV)
Thomas Mayer	University of California, Davis
James McClure	Ball State University
Bruce McCullough	Drexel University
Robert Nelson	University of Maryland
James Nolan	University of Saskatchewan
E.C. Pasour	North Carolina State University
Benjamin Powell	Suffolk University
Russell Roberts	George Mason University
Dani Rodrik	Harvard University
Raymond Sauer	Clemson University
Kurt Schuler	United States Treasury Department
Jane Shaw	John William Pope Center for Education Policy
Donald Shoup	University of California, Los Angeles
William Shughart	University of Mississippi
Per Skedinger	Research Institute of Industrial Economics, Stockholm
Kenneth Small	University of California, Irvine
Richard Swedberg	Cornell University
Alex Tabarrok	George Mason University
Richard Timberlake	University of Georgia
Richard Wagner	George Mason University
Robert Whaples	Wake Forest University

#### **Replying Authors (published 2006-07)**

Benjamin Alamar	Menlo College
David Altig	Federal Reserve Bank of Atlanta
Jakob de Haan	University of Groningen
Robert Frank	Cornell University
Stanton Glantz	University of California, San Francisco
Farley Grubb	University of Delaware
Linda Hooks	Washington and Lee University
Hilary Hoynes	University of California, Davis
Peter Lindert	University of California, Davis
Kenneth Robinson	Federal Reserve Bank of Dallas
Brad Setser	Roubini Global Economics
Jan-Egbert Sturm	Swiss Federal Institute of Technology Zurich

**Other individuals who commented in *EJW* on *EJW* material (published 2006-07)**

William J. Baumol	New York University
Per Hortlund	Ratio Institute
Meir Kohn	Dartmouth College
John Quiggin	University of Queensland
Robert Whaples	Wake Forest University

[Go to January 2008 Table of Contents with links to articles](#)

---

## Do Casinos Really Cause Crime?

DOUGLAS M. WALKER<sup>1</sup>

**A COMMENT ON:** EARL L. GRINOLS AND DAVID B. MUSTARD, “CASINOS, CRIME, AND COMMUNITY COSTS,” *THE REVIEW OF ECONOMICS AND STATISTICS* 88(1), FEBRUARY 2006: 28-45.

### ABSTRACT

*The Review of Economics and Statistics* published “Casinos, Crime, and Community Costs” by Earl Grinols and David Mustard in February 2006. The authors claim that their analysis of casinos and crime is “the most exhaustive ever undertaken in terms of the number of regions examined, the years covered, and the control variables used” (43-44). The paper is a noteworthy contribution to the gambling literature. The scope of their analysis is impressive.

Since its publication the Grinols and Mustard paper has generated much discussion in the press, activist websites, policymaking discourse, and the gambling literature.<sup>2</sup> Because the Grinols and Mustard paper is published in a refereed journal with high academic prestige, it is likely to be influential in subsequent research and political discussions of the casino-crime relationship.

The Grinols and Mustard analysis utilizes county level data on FBI Index I

---

<sup>1</sup> Department of Economics and Finance, College of Charleston. Charleston, SC 29424.

I would like to thank—without implication—several people who made helpful comments and suggestions that improved this paper: Jay Albanese, Bill Eadington, David Forrest, Mark Nichols, Don Ross, Richard Thalheimer, and especially John Jackson and Ben Scafidi. Several referees provided important comments and editorial suggestions.

<sup>2</sup> For example, several newspaper reports have highlighted the Grinols and Mustard study (Morin 2006, Vitagliano 2006, Yarbrough 2006). In recent months the study was discussed in articles in *Parade Magazine* (Flynn 2007) and *The Wall Street Journal* (Whitehouse 2007). Policy reports have utilized the study (Policy Analytics 2006), and recent research has reported the Grinols and Mustard findings (Morse and Goss 2007, 79-82). The paper (or an earlier version, Grinols and Mustard 2001a) has also been posted on activist websites such as the National Coalition Against Legalized Gambling ([link](#)) and CasinoFreePA ([link](#)).

offenses<sup>3</sup> for all U.S. counties from 1977 through 1996. Using a series of dummy variables to account for the existence of casino gambling in counties, as well as a number of control variables, the authors model crime rates and find that they have fallen in both casino and non-casino counties during the sample period. However, Grinols and Mustard report the crime rate dropped by 12 more percentage points in non-casino counties than in casino counties (Grinols and Mustard 2006, 30). Their analysis leads them to conclude that the higher crime rates in casino counties are caused by the existence of casinos. Grinols and Mustard find that for the first two or three years following casino openings there is little or no effect of casinos on crime. However, during the fourth and fifth years after casino openings, most forms of crime begin to escalate in the casino counties. The estimated crime effects are used in conjunction with cost of crime estimates to arrive at the estimated cost of crime caused by casinos of \$75 per adult in U.S. casino-hosting counties (28, 41).

Grinols and Mustard provide a detailed discussion of the theoretical connection between casinos and crime (31-32). They discuss two potential factors through which casinos may reduce crime. First, if casinos present better job opportunities for low-skilled workers, crime may fall. Second, there may be economic development effects attributable to casino gambling that could reduce crime.

On the other hand, Grinols and Mustard discuss five ways in which casinos may lead to an increase in crime. First, casinos may harm economic development by draining the local economy of resources. Second, casinos may lead to an increased crime payoff, resulting in more crime. Third, pathological gambling may increase with the spread of casinos, and this can lead to more crime. Fourth, casinos may also attract criminals to a region, leading to more crime. Finally, Grinols and Mustard explain that casinos may induce a change in the local population, toward one more apt to commit crimes. The Grinols and Mustard mechanisms between casinos and crime seem reasonable and largely uncontroversial.

Unfortunately, the Grinols and Mustard empirical analysis has problems, including: (1) a lack of needed data and its effect on measuring the crime rate, (2) potential problems with their crime data, (3) a possible sample self-selection bias, (4) a poor measure of casino gambling activity, and (5) skewed interpretations of the empirical results. Since the Grinols and Mustard paper has been so influential, its shortcomings need to be thoroughly explored.

Gambling is a controversial issue. It may be one of those issues where most conventional sources of support are disinclined to support research that might come to politically incorrect conclusions. Such a situation gives rise to the hazard that politically-incorrect research and interested industry groups tend to make connections, and research with any connection to such groups is then discounted, regardless of its scholarly merits and arguments. In the Appendix to this paper I make disclosures and discuss the general problem of researcher motivations and commitments.

---

3 These offenses include aggravated assault, rape, robbery, murder, larceny, burglary, and auto theft.

### CALCULATING THE CRIME RATE

The crime rate is typically measured as the number of crimes committed divided by the population. This is usually multiplied by 100,000:

$$\text{crime rate} = \frac{\# \text{ of crimes committed} \times 100,000}{\text{population}} = \text{crimes per 100,000 people} \quad (1)$$

If we let  $C$  be the number of crime incidents and  $P$  be the population, then the crime rate in (1) can be expressed as  $C/P \times 100,000$ . This rate gives a fair indication of the risk of being victimized by crime.

Relative to the U.S. population, the number of tourists is small. So an adjustment for visitors and the crimes they commit is not likely to affect significantly the U.S. crime rate or the residents' risk of being victimized by crime. However, if one is considering a very small area, such as a county that has a large tourist attraction, then for the crime rate to represent accurately the risk of being victimized, it must be adjusted to account for the crimes committed by visitors *and* for the increase in the population at risk of being victimized by crime.

Several authors have discussed how tourism should be considered when analyzing the crime rate. Nettler (1984, 48) explains, "to increase the accuracy of forecasts, a rate should be 'refined' so that it includes in its denominator *all those persons and only those persons who are at risk* of whatever kind of event is being tallied in the numerator." Nettler describes rates that do not correctly represent the population at risk as "crude" (48). Boggs (1965) considers central business districts, which attract large numbers of visitors. She explains that ignoring the visitors produces a spuriously high crime rate (900). Curran and Scarpitti (1991, 438) explain that the FBI, the source of the Grinols and Mustard crime data, warns against "comparing statistical data...solely on the basis of their population."

To illustrate the effect of visitors (tourists) on the crime rate, let  $C_R$  be the crimes committed by residents and  $C_V$  be crime committed by visitors. Also let  $P_R$  be the resident population and  $P_V$  be the population who are visiting. Then the total number of crimes committed will be  $C_R + C_V$ , and the population at risk is  $P_R + P_V$ . We can rewrite the crime rate from equation (1) as<sup>4</sup>

$$\text{Crime rate} = \frac{C_R + C_V}{P_R + P_V} \quad (2)$$

Clearly, if we are interested in the crime rate for a single county that is attracting relatively many visitors then it is critical to account for visitors in both the

---

<sup>4</sup> For simplicity we hereafter ignore the standard practice of multiplying the rate by 100,000.

numerator ( $C_v$ ) and the denominator ( $P_v$ ).

Grinols and Mustard use as the crime rate  $C_R + C_v/P_R$ , which is greater than  $C_R + C_v/P_R + P_v$ . Obviously, the difference between the two measures is greater the more tourists there are. Grinols and Mustard explain that county level visitor data are not available (34). As a result, they have no option but to exclude  $P_v$  from the denominator of the crime rate. But they do include  $C_v$  in the numerator. The result is that Grinols and Mustard overstate the crime rate in casino counties and therefore, overstate the risk to casino county residents of being victimized by crime. This latter observation is particularly important, since the apparent objective of the Grinols and Mustard paper is to analyze the risk of casino county residents falling victim to crime (34, 35). If these risks are overstated then so will be the estimated costs of crime due to casinos.

Grinols and Mustard attempt to justify their crime rate measure by first creating names for two types of crime rate: “undiluted” and “diluted” (34). The “undiluted” or “traditional” rate used in their analysis is what Nettler (1984) refers to as a “crude” rate. It is shown using our notation from above:

$$\text{“undiluted” crime rate} = \text{crude crime rate} = \frac{C_R + C_v}{P_R} \quad (3)$$

When the number of visitors ( $P_v$ ) is added to the population at risk measure, Grinols and Mustard call the result the “diluted” crime rate. This is what Nettler (1984) refers to as a “refined” rate, and it is the original crime rate from equation (2). The terminology “diluted” and “undiluted” appears to be original with Grinols and Mustard. They explain their “decision” to use the “undiluted” crime rate:

Some have argued for one [rate]...or the other without realizing that the choice is not methodological, but depends on what questions the researcher wants to answer. A common but invalid claim is that the diluted crime rate should be used to determine the change in probability that a resident would be the victim of a crime. However, knowing what happens to the diluted crime rate does not give the needed information and could even move the answer in the wrong direction.<sup>[5]</sup> (34)

Grinols and Mustard provide an example to show why the “diluted” crime rate may not provide “the needed information”—and that as a result,  $P_v$  should be excluded from the crime rate calculation:

---

5 Note that Grinols and Mustard do not explain *why* the claim that “the diluted rate should be used” is invalid. Nor do they cite work where the claim is invalidly made.

...let  $s_I$  be the share of resident population  $P$  victimized by residents, and let  $s_V$  be the share of the resident population victimized by  $V$  visitors. Similarly, let  $\sigma_I$  be the share of visitors victimized by residents and  $\sigma_V$  the share of visitors victimized by visitors. Then the [undiluted] crime rate is  $s_I + s_V + (\sigma_I + \sigma_V)V/P$ ; the diluted crime rate is  $(s_I + s_V)w_P + (\sigma_I + \sigma_V)w_V$  where  $w_P$  and  $w_V$  are the shares of visitors plus residents made up by residents and visitors, respectively; and the probability of a resident's being a crime victim is  $s_I + s_V$ . If residents do not victimize visitors ( $\sigma_I=0$ ), then  $P=V$ , and  $s_V + \sigma_V$  is smaller than  $s_I$ . The probability of a resident being victimized is  $s_I$  without visitors, and it rises to  $s_I + s_V$  with visitors. The diluted crime rate is  $s_I$  without visitors and falls to  $(s_I + s_V + \sigma_V)/2$  with visitors. Thus in this case the diluted crime rate falls while the probability of a resident being victimized rises. (34-35)

They explain that their interest is in “the costs to the host county associated with a change in crime from whatever source. We are therefore interested in the total effect of casinos on crime, and thus use the undiluted crime rate...” (35).<sup>6</sup>

It appears that their conclusion—the risk to residents rises even though the “diluted” rate falls—occurs only because of their assumptions: “If residents do not victimize visitors ( $\sigma_I=0$ ), then  $P=V$ , and  $s_V + \sigma_V$  is smaller than  $s_I$ ” (34).<sup>7</sup> One *can imagine* a situation which provides the conclusion that the risk to residents rises even though the “diluted” crime rate falls. But this is by no means the only possible outcome.

To illustrate, consider Albanese's (1985, 41) simple numerical example:

A city with a population of 100 citizens might experience 10 reported Index crimes in a year. Therefore, the probability that any one citizen will be the victim of one of these crimes is 1 in 10. If the population of this city suddenly doubles [after a casino opens]

6 Presumably, Grinols and Mustard are interested in the costs to the host county because these jurisdictions may be responsible for bearing the costs associated with any casino-related crime. In addition, some residents will be the victims of visiting criminals. Since the decision to adopt casinos is made locally, one could argue that a focus on the local, county-level effects is warranted. On the other hand, one could argue that the casino legalization question begins with the state, so state-level effects are more important to the politicians responsible for the initial legalization. In addition, casinos pay hefty fees and significant taxes that may partially offset any locally-incurred costs of casinos. Even if one agrees with Grinols and Mustard that the local effects are of primary concern, it does not necessarily imply the “undiluted” crime rate is the appropriate one.

7 As the sentence reads, it does not make sense. First,  $P=V$  does not follow from the assumption that  $\sigma_I=0$ ; nor does “( $s_V + \sigma_V$ ) is smaller than  $s_I$ ” follow. Perhaps Grinols and Mustard transposed “then” and “and”. In an earlier version of the Grinols and Mustard paper (2001a, 14), this sentence is worded differently: “For example, assume that residents do not victimize visitors ( $\sigma_I=0$ ),  $P=V$ , and ( $s_V + \sigma_V$ ) is smaller than  $s_I$ .” This wording clearly indicates that all three conditions are assumptions.

to, say, 200 citizens, it is likely that the number of crimes that occur there will also rise—simply because there are more people to be offenders and victims. If the number of crimes also doubled to 20, it would appear as if crime increased 100%. However, this is not the case. If 200 people are now at risk and 20 crimes are committed, the probability of being a victim is *still* 1 in 10 (i.e., 20 in 200). Therefore, the risk of being victimized by crime can remain the same when *both* the population and crime increase together.

One can fabricate an example in which Grinols and Mustard's conclusion obtains, beginning with 100 residents and 10 crimes and the Grinols and Mustard assumption that residents do not victimize visitors. Suppose that now 100 visitors come and commit 8 crimes. Then the "diluted" crime rate will fall to 18 in 200 (9 in 100). If *only one* of the new crimes is committed against a resident, then the risk to residents rises to 11 in 100. It is unlikely that visitors will only victimize visitors, so the Grinols and Mustard assumption that residents do not victimize visitors virtually ensures that the risk to residents will increase, whether the "diluted" rate rises or falls. But the necessary assumptions to ensure that Grinols and Mustard's conclusions obtain are very contrived, so the justification for excluding visitors from the population at risk and using the "undiluted" rate is very weak.

Recall that the crime rate is typically used to measure the likelihood of being victimized by crime for the population at risk. If we exclude visitors from the population at risk, then we are implicitly assuming that only residents are at risk of being victimized. When Grinols and Mustard choose the "undiluted" crime rate,  $C_R + C_V/P_R$ , they are implicitly forcing the assumption that *all crime is committed against residents*—since visitors are excluded from the denominator. This certainly overstates the crime rates in tourist counties and will overstate the true risk of those counties' residents being victimized.<sup>8</sup>

Clearly there are a number of possibilities for how the "diluted" crime rate will move relative to the residents' risk of being victimized; Grinols and Mustard highlight one scenario. Now let's consider others. Again start with 10 crimes and 100 residents, and the Grinols and Mustard assumption that residents only victimize residents. If 100 visitors come and commit an additional 10 crimes, here are a few of the possibilities: (i) if visitors commit 5 crimes against residents and 5 crimes against visitors, then the risk to residents rises to 15 in 100, while the "diluted" crime rate remains constant (it changes from 10 in 100 to 20 in 200); (ii) if visitors commit all 10 crimes against other visitors, then the risk to residents and the "diluted" crime rate are unchanged; (iii) if visitors commit 5 crimes against visitors and 5 against residents, and the resident criminals also attack residents and visitors equally, then the risk to residents remains constant, and the

<sup>8</sup> The more tourism in a county, the larger the overstatement of the crime rate and the risk to residents.

“diluted” rate is unchanged; (iv) if all criminals attack only visitors, then the risk to residents falls to zero, while the “diluted” rate is unchanged. Obviously there are other possible scenarios.

The important point is that the relationship between risk to residents and the “diluted” and “undiluted” crime rates depends critically on who the criminals are and who the victims are.<sup>9</sup> Unfortunately, Grinols and Mustard do not have these data. But a variety of research, as well as common sense and common experience, suggests that tourists are popular targets for criminals (Chesney-Lind and Lind 1986, Harper 2001, Miller and Schwartz 1998, and Fujii and Mak 1980).

What are the odds that all resident and visiting criminals ignore tourists and attack only residents, as Grinols and Mustard implicitly assume? Without evidence to the contrary, it seems more likely that a resident and a visitor are roughly equally likely to be victimized. In this case, clearly the “diluted” crime rate is the appropriate one to use if we are trying to measure the risk to residents and/or visitors of being victimized. The Grinols and Mustard “undiluted” crime rate will overstate the crime rate in tourist (casino) counties. This is perhaps the most significant problem in the Grinols and Mustard paper.

#### ANOMALIES IN THE CRIME DATA

There are two potential problems with the Grinols and Mustard crime data, the *Uniform Crime Reports* (UCR). The UCR data at the county level are based on voluntary crime reporting by a number of agencies within each county. The crimes reported by the various agencies are aggregated to arrive at the county-level UCR data. The problem arises from the fact that *unreported crime data are imputed*. For the 1977-93 data, the UCR explains that the reason for the imputation was to “ensure cross-sectional data comparability and quality.” But it warns, “if there were major changes in the [agencies] reporting in a county across years, artifactual changes in the longitudinal data for a county could be introduced because of potential variation in the type of [agency] used to compute imputed county totals and rates each year” (ii). In order to make the data more useful for longitudinal analyses, for 1994 and later, the UCR changed its method of imputing missing data (i).

There are two problems with the UCR data as they relate to the Grinols and Mustard study. The first is that the imputation for crime by non-reporting agencies may introduce anomalies into the Grinols and Mustard crime data.<sup>10</sup> Maltz

9 Knowing where the crimes occur (on casino premises or off) would also provide some insight into the relative probabilities of being victimized. See Curran and Scarpitti (1991).

10 Grinols and Mustard do note that some of their observations (about 5,300) had missing data and were not included in the model (p. 35). However, they do not explain what the missing data are. Even if this refers to imputed UCR data, the absence of those data could still potentially affect their results. Grinols and Mustard do indicate that they used regressions weighted by county population (35). This could mitigate some of the data problems, to the extent that less populated counties are less likely to

(1999, 26) explains, “Most observers believe that the effect on the estimate of the *overall* crime rate in the United States would be minimal, but that it could be quite problematic when investigating the crime rate for a smaller unit such as a State or county, or when looking at rural crime rates.”<sup>11</sup> Maltz and Targonski (2002) believe the problems are so serious that, “until improved methods of imputing county-level crime data are developed, tested, and implemented, they should not be used, especially in policy studies” (297).

The second problem is that, although the Grinols and Mustard sample period is 1977-96, the authors’ model apparently does not account for the 1994 change in UCR data reporting. The UCR data codebook includes a section titled, “Break in Series,” in which it warns, “*data from earlier year files should not be compared to data from 1994 and subsequent years* because changes in procedures...may be expected to have an impact on aggregates for counties in which some [agencies] have not reported for all 12 months” (p. i; emphasis added).

It difficult to speculate on how exactly these data issues might affect the Grinols and Mustard analysis, but the effect could be serious. Much of the U.S. casino expansion occurred in 1991-93.<sup>12</sup> As discussed below, Grinols and Mustard find crime in casino counties starts to rise four or five years after casinos are introduced. For counties that adopted casinos in the early 1990s, this increase in crime rate corresponds to 1994 or later—after the UCR imputation change. It is possible that Grinols and Mustard’s finding of a crime effect results from the UCR data imputation, the 1994 change, or both.

### SAMPLE SELF-SELECTION

Grinols and Mustard use a dummy variable to track the first opening of a casino into the county. Variables are also used to account for time relative to the first casino opening in a county, from two lead years to five lag years. The empirical results show no significant changes in casino county crime rates until four or five years after the introduction of casinos. Grinols and Mustard claim that “[by] conducting the most exhaustive investigation and utilizing a comprehensive county-level data set that includes every U.S. county, we eliminate sample selection concerns” (33). The authors do not choose a biased sample, but there is a potential sample self-selection bias in their model.

---

report crime. This issue is discussed in the debate between Maltz and Targonski (2002, 2003) and Lott and Whitley (2003).

11 It is surprising that Grinols and Mustard used the UCR data at all. In the context of the “right-to-carry” gun law debate, Lott and Whitley (2003) mention that Lott and Mustard were well aware of problems with the UCR data, and that they “had compiled an eight page single-spaced list of problems” (186, note 6). Grinols and Mustard should have at least acknowledged that there are potential problems with the data, even if they are the best data available.

12 Only Nevada, New Jersey, and South Dakota had commercial casinos prior to 1991.

Grinols and Mustard do not account for the fact that counties self-select into the “casino county” category by the decision to permit casinos.<sup>13</sup> Since casino gambling has often been sold as a potential growth or tax revenue strategy (Walker 2007a), there is good reason to believe that counties with relatively poorly performing economies might be more likely to introduce casinos and to do so more quickly than counties that are better off economically. Indeed, Grinols and Mustard mention the common belief that casinos are more likely to be placed in high-crime areas (36), and that the number of casinos began increasing rapidly in 1991 (38). The time was toward the end of a recession, and corresponds to the 1996 Lag 5 crime estimates, which are the only basis for some of the Grinols and Mustard cost of crime estimates (41). Some states and counties may have legalized casinos in part because of economic hardships caused by the recession of 1990-91, representing factors that may be driving Grinols and Mustard’s results. The importance of state self-selection is shown by Fink, Marco, and Rork (2004) in the case of lottery adoption and the lotteries’ impact on state budgets. A similar consideration should have been incorporated into the Grinols and Mustard analysis.

Grinols and Mustard argue that because they include control variables in the model and find no significant differences between casino and non-casino lead period crime rates, “casinos were not more likely to be placed in areas that had systematically different crime environments than other regions” (40; also see 36). But the lead period crime rates are mostly positive (though statistically insignificant) in casino counties. Perhaps there are observed or unobserved factors that explain casino adoption. Grinols and Mustard do not account for the possibility of sample self-selection bias in their model.<sup>14</sup>

### CASINO DUMMY VARIABLES

Aside from the potential self-selection problems for casino counties, the variables Grinols and Mustard use to measure casino activity have other problems. They note that the ideal measure of casino activity would be revenues or profits (29), but that such data are not available for Indian casinos.<sup>15</sup> Grinols and Mustard instead use a dummy variable indicating the year in which a casino first opened in the county (35) and lead and lag dummies to account for the existence of casinos for various lengths of time.

The Grinols and Mustard casino dummy may show how sensitive crime rates are to the opening of a casino, but if there is a relationship between casino

13 This obviously occurs only after the state has legalized casinos.

14 A standard procedure for dealing with sample self-selection bias is the Heckman (1979) two-step method. See Fink et al. (2004) for an application of this procedure to lotteries, or Walker and Jackson (2008a) for an application to an analysis of the relationships among gambling industries.

15 There are available measures of casino volume. For example, Walker and Jackson (2008a) use Indian casino square footage as a proxy for gambling volume.

gambling and crime, one would expect that relationship to be dependent on the volume or size of the casino, the number of casinos, and perhaps even on the types of games offered. But the Grinols and Mustard first-year dummy cannot pick up any such variations in the casino industry in the counties. It essentially treats all the Las Vegas mega-casinos as having the same impact on crime in the county as, say, a single small casino in a Colorado county.

Furthermore, the dummy variable technique used by Grinols and Mustard to denote casino counties will pick up *any* differences in the crime rates between casino and non-casino counties, not just those differences that are due to the presence of casinos. In general, anything that distinguishes the casino counties from national norms will be picked up by the dummy. Even the effects of the included demographic and other normalizing variables, to the extent that their impact on the crime rate differs between casino and non-casino counties, will be picked-up by the dummy. Thus, inferring that a positive and significant dummy coefficient for casino counties implies a higher crime rate in those counties *because of the presence of casinos* is conjectural.<sup>16</sup>

For example, it is possible that the crime effect found by Grinols and Mustard in casino counties is due to *tourism in general* rather than to *casino-specific tourism*.<sup>17</sup> If a county had decided to build new attractions along an urban strip and was deciding to authorize *either* a casino or an adventure water park that would attract teens and young adults, it might be misled if it interpreted Grinols and Mustard's results as speaking of casino-specific tourism. Had they compared casino counties with similar non-casino tourism counties,<sup>18</sup> their results would have been more likely to show any existing crime effect attributable to casino-specific tourism.

## LAG 5 CRIME RATES

Grinols and Mustard's conclusion that "roughly 8% of crime in casino counties in 1996 was attributable to casinos, costing the average adult in casino

16 This problem is related to the previous issue, self-selection bias. The Grinols and Mustard dummy variables may be indicative of those variables that would help explain the casino adoption decisions by counties.

17 Grinols and Mustard anticipate this argument and use available visitor data from Las Vegas and the three largest tourist attractions in the U.S. (Mall of America, Disney World, and Branson, MO) along with National Parks (32, 34; also see Grinols and Mustard note 13). They show that, adjusted for the numbers of tourists, the crime rate in Las Vegas is significantly higher than at the other venues. The implication is that casino tourists are more likely than other tourists to commit crimes. While this may be true, the Grinols and Mustard comparisons do not show it. First, most Las Vegas tourists are adults, while many tourists to the comparison destinations are children. Second, Mall of America and Disney World are destinations principally enclosed in an encompassing private area, quite unlike "the strip" and environs in Las Vegas. Third, National Parks are usually located far outside of urban settings.

18 Stitt, Nichols, and Giacomassi (2003) perform an analysis of casinos and crime using control communities.

counties \$75 per year” (28; also see 41) is based on a series of questionable assumptions and interpretations, most of which have the effect of increasing the apparent casino effect on crime.

At least some of the of the Grinols and Mustard results and conclusions are based on only the Lag 5 casino crime rate estimates,<sup>19</sup> a technique that calls for two objections. First, the Lag 5 crime rate estimates are the highest of any in the model (37, Table 4).<sup>20</sup> Second, the Lag 5 estimates are based on only 49 of the 178 casino counties (or about 28% of them; p. 35).<sup>21</sup> The truncation raises questions about whether these early adopting casino counties with the highest estimated crime rates are representative of all casino counties. After all, the early-adopting counties represented by Lag 5 crime rates likely attracted more tourism than those counties represented in more recent lag periods, when casinos had become more widespread. This would suggest that the Lag 5 casino county crime rates are probably the most overstated of any period’s, because the “undiluted” crime rate used by Grinols and Mustard excludes visitors from the population at risk.

Finally, one may question whether the Grinols and Mustard results accurately portray the marginal effect of casinos on crime. Their Lag 5 crime rates, for example, show how high the mean crime rates in casino counties (which have had casinos for 5 years) are relative to the mean crime rates of non-casino counties. But this does not take into consideration the fact that the crime rate coefficients in casino counties were often positive (albeit mostly insignificant) relative to non-casino counties prior to the introduction of casinos. As Grinols and Mustard indicate (36), there is a common belief that casinos are more likely to be placed in high-crime areas.

Rather than focusing on Lag 5 casino crime rates relative to non-casino county crime rates, one could argue that a more accurate picture of the effect of casinos on crime could be drawn from, for example, subtracting the average

---

19 Grinols and Mustard use the fifth year crime rate alone in estimating the number of crimes that would be committed by problem and pathological gamblers if that was the one source of additional crime in casino counties (40-41). They also use only the fifth year period to calculate the average property loss for four of the criminal offenses they study (41). However, when calculating their “implied cost of additional crime” due to casinos (\$75 per adult in casino counties; p. 41), Grinols and Mustard are not clear about how the calculation is made. They write, “Summing the estimated number of crimes attributable to casinos for each county, taking into account how many years the casino was in operation, and dividing by the casino counties’ total population measures the contribution of casinos to observed crime” (41). A reasonable reader could infer from the surrounding discussion that the authors based their results on only the Lag 5 crime rate estimates because they explicitly state that these were the crime rates used in the other calculations, described above. For such a critical issue, one would expect the authors to provide a clear, detailed explanation.

20 Recall that the Lag 5 estimates correspond closely to counties that adopted casinos toward the end of a recession.

21 Each lag period crime coefficient is based on a partially changing sample of casino counties. For example, the Lag 4 sample includes all the Lag 5 counties plus counties that introduced casinos four years ago. Lag 3 includes the counties from Lags 4 and 5, plus counties that adopted casinos three years ago.

lead-period crime rates in casino counties—which are mostly positive—from the average lag period crime rates. This calculation takes into account crime rates both before and after casinos are introduced, and it better accounts for all casino counties. The Grinols and Mustard Lag 5 crime rates are between 1.5 and 5.5 times higher than the average change in crime rates from before to after the introduction of casinos.<sup>22</sup> This suggests that Grinols and Mustard may be seriously overstating the true average effects of casinos on crime.

## CONCLUSION

Other studies examine crime rates while accounting for visitors in particular casino markets. They find mixed results.<sup>23</sup> It is reasonable to believe that tourist areas might act as “hot spots” for crime, and attract criminals. Casino patrons often carry lots of cash, and many casinos serve free alcohol, so patrons may be less alert than usual. On the other hand, casinos are famous for their security measures. Stitt et al. (2003, 281) conclude that casinos built with the approval of the surrounding community probably do not act as “hot spots.”

Grinols and Mustard confidently present their study as being the “most exhaustive ever undertaken” (43) and their results as being “lower bounds on the true effect [of casinos on crime]” (44). But in this comment I have identified several serious problems with their data, model, analysis, and interpretation of results. Most of the problems identified here will have the effect of overstating the estimated effect of casinos on crime.

My point is not to suggest that casinos do not cause crime. They might.<sup>24</sup> Many economists will concede that there are problems in any empirical study. However, the errors in the Grinols and Mustard study deserve attention because of the influence their study seems to be having among researchers, policymakers, the media, and voters.

## APPENDIX: COMMITMENTS AND MOTIVATIONS

Gambling research is still fairly young, developing mostly since the spread of casino gambling across the U.S. in the 1990s. Casino gambling is a controversial

22 For each type of crime I took the average lead crime rates and subtracted them from the average lag crime rates. The resulting marginal impacts of casinos on crime were, for the most part, lower than the average lag crime rates, and were much lower than the Grinols and Mustard Lag 5 crime rate estimates used in some of their cost calculations. The only exception is for murder; Grinols and Mustard found a slightly negative coefficient for murder in Lag 5. The difference in means is slightly positive.

23 See Albanese (1985), Curran and Scarpitti (1991), Stitt et al. (2003), and Stokowski (1996).

24 It would be ideal to replicate the Grinols and Mustard analysis using appropriate data and analysis. Unfortunately, the required data (county visitor count) simply do not exist. In addition, county-level crime data are potentially unreliable. Still, it would be interesting to see if the Grinols and Mustard results hold using more recent data, say through 2006.

policy issue, and the controversy has stimulated debate, both public and academic, especially over how to identify and measure the costs and benefits. Readers may wonder what motivated the present comment on the Grinols and Mustard paper. I explain that, as well as some background on gambling research.

My own contributions to this literature and debate have dealt with empirical issues such as the state-level economic growth and tax effects of casino gambling in the U.S., as well as the relationships among gambling industries; and methodological issues surrounding social costs.<sup>25</sup> My empirical work has found short-term regional economic growth from the introduction of casino gambling, but there appears to be no longer-run economic growth effect. One of my studies currently under review indicates that casino gambling decreases tax revenues in casino states. My work on social costs has focused on methodological problems in identifying and measuring the social costs of gambling.<sup>26</sup> Overall, my research leads me to believe that there is some evidence that casinos may have a positive economic effect in the short-term, but the long-term effects are less certain. This is hardly a warm endorsement of casinos. But at the same time, I do reject the assessment that Grinols and Mustard would have us believe.

In addition to publishing in peer-reviewed journals, I have done a variety of consulting work, primarily on the social costs of gambling. This work has been aimed at identifying potential problems for researchers attempting to measure the costs and benefits of gambling, as well as the refutation of specific cost-benefit analyses which appeared to me to be seriously flawed. Sponsors of my consulting work have included the casino industry (e.g., American Gaming Association, Nevada Resort Association, Casino Association of Indiana) as well as government/research organizations (Alberta Gaming Research Institute and the Canadian Centre on Substance Abuse). I assume that the industry has hired me as a consultant because my social cost methodology (welfare economics) leads to significantly lower social cost estimates than the methodologies used by other researchers, including Grinols and Mustard.<sup>27</sup>

Much has been made of financial ties that researchers sometimes have to industry. For example, Grinols and Mustard have questioned the validity of casino-crime research that was conducted or funded by pro- or anti-casino groups (28). In other work, Grinols has cited a paper of mine (Walker 2003) as being an example of “shadow research,” or work that is “funded in the hope or expectation that it will contradict research unfavorable to the sponsoring industry” (Grinols 2007,

25 See Walker and Jackson (1998, 2007, 2008a, 2008b), Walker and Barnett (1999), and Walker (2007a, 2007b).

26 I have been critical of a variety of researchers who have attempted to measure social costs without first giving a clear explanation of what they are trying to measure. See Walker (2007a, chapters 6-8).

27 Grinols and Mustard (2001b) and Grinols (2004) provide social cost estimates based on previous research, most of which was not peer-reviewed (Grinols and Mustard 2001b, 152). Such social cost studies have been criticized as being somewhat arbitrary (National Research Council 1999, 185). For a detailed discussion, see Walker (2007a).

517).<sup>28</sup> At the same time, Grinols claims to believe that “research can be evaluated on its own merit, regardless of its sponsor. It is certainly not improper for an industry to sponsor research or for a researcher to accept industry money” (516).

In order to address any perceived conflict of interest, I should emphasize that my consulting work has always been an application of my un-funded, peer-reviewed published work. Furthermore, my current comment on Grinols and Mustard’s crime paper was not funded by, nor even discussed with, any industry representative or organization. My motivation for writing this comment was simply to question the Grinols and Mustard analysis and results because they were published in such a prestigious journal and have been influential, despite with what I see as flagrant errors. But even my being paid to write the comment would not, in itself, invalidate the arguments.

Every researcher has sensibilities related to the subjects he studies. To claim otherwise would be disingenuous. The Nobel laureate economist Gunnar Myrdal propounded the view that whenever personal commitments, financial, intellectual, or otherwise, might color one’s formulation or analysis, science and ethics demand that such commitments be made known to readers (Myrdal 1969). I generally take a libertarian perspective on consumer issues such as gambling.<sup>29</sup> However, I try to keep these sensibilities from distorting my research, and I attempt to be as transparent as possible in explaining my methods and reasoning.

I do not believe either Grinols or Mustard does paid consulting work on gambling. However, Grinols recently co-authored an op-ed piece with the co-chair of Citizens Against Casino Gambling in Erie County (Grinols and Rose 2007). In fact, he has consistently argued that the costs of casinos are greater than the benefits, at least as early as 1992, prior to there being much of any data on the effects of casinos outside of Las Vegas and Atlantic City.<sup>30</sup> And Grinols and Mustard’s work is posted or cited on a variety of anti-casino activist websites. Do these things indicate that Grinols and Mustard are biased, or view casino gambling as a negative “merit good”? No more than being an industry consultant is indicative of a pro-casino bias. Regardless of how controversy, personal or religious beliefs, funding sources, and other factors may affect a researcher’s work, the best way to assess a dispute among researchers is on the basis of the research itself.

---

28 But as Grinols and Mustard’s paper demonstrates, some gambling research is flawed. I see no good reason that researchers should shy away from debating flawed research simply because there are interested parties.

29 I note that Grinols and Mustard have been, respectively, President and Vice President of the Association of Christian Economists ([link](#)), so their personal views of gambling may well be different from mine. I am not suggesting, however, that these views distorted their research findings.

30 An anti-gambling op-ed by Grinols was entered into the Congressional Record by Senator Simon on January 22, 1992 (p. S187). In the article, Grinols refers to gambling as a “delusion.”

## REFERENCES

- Albanese, J.** 1985. The effect of casino gambling on crime. *Federal Probation* 48: 39-44.
- Boggs, S.L.** 1965. Urban crime patterns. *American Sociological Review* 30: 899-908.
- Chesney-Lind M., and I. Lind.** 1986. Visitors against victims: Crimes against tourists in Hawaii. *Annals of Tourism Research* 13: 167-191.
- Congressional Record.** 22 January 1992. Washington, D.C.
- Curran, D., and F. Scarpitti.** 1991. Crime in Atlantic City: Do casinos make a difference? *Deviant Behavior* 12: 431-449.
- Fink S.C., A.C. Marco, and J.C. Rork.** 2004. Lotto nothing? The budgetary impact of state lotteries. *Applied Economics* 36: 2357-2367.
- Flynn, S.** 2007. Is Gambling Good for America? *Parade Magazine*. 20 May. [Link](#).
- Fujii, E., and J. Mak.** 1980. Tourism and crime: Implications for regional development policy. *Regional Studies* 14(1): 27-36.
- Grinols, E.L.** 2004. *Gambling in America: Costs and Benefits*. New York, NY: Cambridge University Press.
- Grinols, E.L.** 2007. Social and economic impacts of gambling. In *Research and Measurement Issues in Gambling Studies*, ed. G. Smith, D. Hodgins, and R. Williams, 515-539. Boston, MA: Academic Press.
- Grinols E.L., and D.B. Mustard.** 2001a. Measuring industry externalities: The curious case of casinos and crime. Paper posted on the website of the National Coalition Against Legalized Gambling. [Link](#).
- Grinols, E.L., and D.B. Mustard.** 2001b. Business profitability versus social profitability: Evaluating industries with externalities, the case of casinos. *Managerial and Decision Economics* 22: 143-162.
- Grinols, E.L., and D.B. Mustard.** 2006. Casinos, crime, and community costs. *The Review of Economics and Statistics* 88(1): 28-45.
- Grinols, E.L., and J.S. Rose.** 2007. Another voice: Laudatory report misstates conclusions on gambling. *Buffalo News*. 13 March.
- Harper, D.W.** 2001. Comparing tourists crime victimization. *Annals of Tourism Research* 28(4): 1053-1056.
- Heckman, J.J.** 1979. Sample selection bias as a specification error. *Econometrica* 47(1): 364-369.
- Lott, J.R., and J. Whitley.** 2003. Measurement error in county-level UCR data. *Journal of Quantitative Criminology* 19(2): 185-198.
- Maltz, M.D.** 1999. Bridging gaps in police crime data. Bureau of Justice Statistics. [Link](#).
- Maltz, M.D., and J. Targonski.** 2002. A note on the use of county-level UCR data.

- Journal of Quantitative Criminology* 18(3): 297-318.
- Maltz, M.D., and J. Targonski.** 2003. Measurement and other errors in county-level UCR data: A reply to Lott and Whitley. *Journal of Quantitative Criminology* 19(2): 199-206.
- Miller, W.J., and M.D. Schwartz.** 1998. Casino gambling and street crime. *Annals of the American Academy of Political & Social Science* 556: 124-137.
- Morin, R.** 2006. Casinos and crime: The luck runs out. *Washington Post*, 11 May.
- Morse, E.A., and E.P. Goss.** 2007. *Governing Fortune: Casino Gambling in America*. Ann Arbor: University of Michigan Press.
- Myrdal, G.** 1969. *Objectivity in Social Research*. New York: Pantheon Books.
- National Research Council.** 1999. *Pathological Gambling: A Critical Review*. Washington, DC: National Academy Press.
- Nettler, G.** 1984. *Explaining Crime*. 3<sup>rd</sup> ed. New York: McGraw-Hill.
- Policy Analytics.** 2006. A benefit-cost analysis of Indiana's riverboat casinos for FY 2005: A report to the Indiana Legislative Council and the Indiana Gaming Commission. 17 January. [Link](#).
- Stitt B.G., M. Nichols, and D. Giacomassi.** 2003. Does the presence of casinos increase crime? An examination of casino and control communities. *Crime & Delinquency* 49(2): 253-284.
- Stokowski, P.** 1996. Crime patterns and gaming development in rural Colorado. *Journal of Travel Research* 34: 63-69.
- Uniform Crime Reports.** 1994. Codebook for UCR 1994 (ICPSR 6669). University of Michigan, Inter-University Consortium for Political and Social Research. [Link](#).
- Vitagliano, E.** 2006. Casinos and crime: A sour bet. *American Family Association Journal* (August). [Link](#).
- Walker, D.M.** 2003. Review of Schwer, Thompson, and Nakamuro, "Beyond the limits of recreation: Social costs of gambling in Las Vegas." Paper prepared for the Nevada Resort Association.
- Walker, D.M.** 2007a. *The Economics of Casino Gambling*. New York, NY: Springer.
- Walker, D.M.** 2007b. Problems with quantifying the social costs and benefits of gambling. *American Journal of Economics and Sociology* 66(3): 609-645.
- Walker, D.M., and A.H. Barnett.** 1999. The social costs of gambling: An economic perspective. *Journal of Gambling Studies* 15(3): 181-212.
- Walker, D.M., and J.D. Jackson.** 1998. New goods and economic growth: Evidence from legalized gambling. *Review of Regional Studies* 28(2): 47-69.
- Walker, D.M., and J.D. Jackson.** 2007. Do casinos cause economic growth? *American Journal of Economics and Sociology* 66(3): 593-607.

DOUGLAS WALKER

**Walker, D.M., and J.D. Jackson.** 2008a. Do U.S. gambling industries cannibalize each other? *Public Finance Review* 36 (forthcoming).

**Walker, D.M., and J.D. Jackson.** 2008b. Katrina and the Gulf States Casino Industry. *Journal of Business Valuation and Economic Loss Analysis* (forthcoming).

**Whitehouse, M.** 2007. Bad Odds. *Wall Street Journal*. 11 June.

**Yarbrough, B.** 2006. Casinos increase crime. *Hesperia Star*. 6 June. [Link](#).

#### ABOUT THE AUTHOR



Douglas M. Walker is an associate professor of economics at the College of Charleston, in Charleston, SC. He received his Ph.D. in economics from Auburn University in 1998. Prior to coming to the College of Charleston, he taught at Auburn, Louisiana State University, and Georgia College. His research focus is on the economic and social effects of casinos and other types of legalized gambling. His book, *The Economics of Casino Gambling*, was published in 2007 by Springer. His gambling research has also been published in journals including *American Journal of Economics and Sociology*, *Journal of Gambling Studies*, *Public Finance Review*, and *Review of Regional Studies*. His email is WalkerD@cofc.edu.

[Go to Reply by Earl Grinols and David Mustard](#)

[Go to January 2008 Table of Contents with links to articles](#)

---

## Correctly Critiquing Casino-Crime Causality

EARL L. GRINOLS<sup>1</sup> AND DAVID B. MUSTARD<sup>2</sup>

### ABSTRACT

**A REPLY TO:** DOUGLAS M. WALKER, “DO CASINOS REALLY CAUSE CRIME?” *ECON JOURNAL WATCH* 5(1), JANUARY 2008: 5-20. [LINK](#).

We thank Professor Walker for his attention to our paper on casinos and crime, published in the *Review of Economics and Statistics* (Grinols and Mustard 2006). Professor Walker raises five concerns that are standard in empirical research. We addressed these concerns in the working and published versions of the paper and discussed them with the referees and editor during the review process. Some are well-known statistical issues, some are data limitations, and some are methodology issues. All of his concerns speak of *potential* problems. He includes no new research or statistical results to provide evidence that the potential problems are actual problems or that they are important. Nevertheless, we respond by taking in turn each of the issues he mentions, explaining how we treated them in our previous work, providing the references to our previous work, and, where appropriate, elaborating on the concerns. Because he presents no new data, no new research, and his criticisms are largely addressed in the working and published versions of our paper, we have no reasons to alter the conclusions of our existing research.

### CALCULATING THE CRIME RATE

Professor Walker’s first concern is that our use of the standard crime rate as defined by the F.B.I. (the number of crimes divided by 100,000 of the popula-

---

<sup>1</sup> Department of Economics, School of Business, Baylor University. Waco, Texas 76798.

<sup>2</sup> Department of Economics Terry College, University of Georgia. Athens, GA 30602.

tion) is incorrect. Walker believes the correct crime rate measure is the number of crime incidents divided by the sum of population *plus* some estimate of the number of visitors or visitor-days associated with the area in question. In our paper we call this the “diluted” crime rate to distinguish it from the standard (undiluted) crime rate because it divides the number of crimes by a larger denominator. This is an important distinction that we addressed in our original paper (Grinols and Mustard 2006, 33-35) where we provided a framework for addressing the appropriate crime rate. What are the central conclusions about the use of various crime rates?

First, there is no theoretical reason why one crime statistic should be the only object of study. We repeat the conclusions of our original paper, “Some have argued for one [statistic] or another without realizing that the choice is not methodological, but depends on what questions the researcher wants to answer.” Those who prefer using the diluted crime rate support their view by arguing that the diluted crime rate is a better indicator of the probability that a resident will be the victim of crime. However, this reasoning is incorrect. Grinols and Mustard (2006) states: “A common but invalid claim is that the diluted crime rate should be used to determine the change in probability that a resident would be the victim of a crime” (34). We then proceed to provide a hypothetical example and conclude that: “Thus in this case the diluted crime rate *falls* while the probability of a resident being victimized *rises*” (emphasis in original). Professor Walker devotes over 2,000 words in his paper to our single paragraph on this issue and reaches the following points of agreement with us, “One *can imagine* a situation which provides the conclusion that the risk to residents rises even though the ‘diluted’ crime rate falls (emphasis in original),” and “The important point is that the relationship between risk to residents and the ‘undiluted’ and ‘diluted’ crime rates depends critically on who the criminals are and who the victims are” (10).

If there are many crime-related statistics of interest, and we and Professor Walker agree that diluted crime rates and the probability of a resident being victimized can move in opposite directions, then why does Professor Walker believe there is a problem with our paper? The answer, we believe, is twofold. First, Professor Walker misreads our paper. Walker writes “the apparent objective of the Grinols and Mustard paper is to analyze the risk of casino county residents falling victim to crime.” Walker’s statement is false. Grinols and Mustard (2006, 35) clearly states, “In this study we are interested in the costs to the host county associated with a change in crime from whatever source. We are therefore interested in the total effect of casinos on crime, and thus use the undiluted crime rate based on equation (3).” In other words, because crime perpetrated in a given geographical area can impose costs that fall on local taxpayers, it is appropriate to consider the total number of crime incidents relative to the local population and tax base. Second, Professor Walker misreads the literature. Walker says “Clearly, if we are interested in the crime rate for a single county that is attracting relatively

many visitors then it is critical to account for visitors in both the numerator and the denominator.” Why is this true? We demonstrated that there is no theoretical reason that only one object of study is correct. A careful reading shows that scholars, including those cited by Walker, prefer one statistic or another *conditional on what the researcher wants to do*.

On the matter of calculating the crime rate, our second central point is empirical—we tried to obtain systematic data sets that record annual visitors at the county level, but found nothing of sufficient quality. We believe that a careful examination of the effect of casinos on diluted crime rates would be a contribution to the literature. The best data that we were able to find on visitors was county-level data on the number of visitors to national parks and monuments. We (2006, 34) gave some preliminary evidence that increased national park visitors are not associated with higher crime rates and suggested that future research investigate the extent to which the type of visitor matters to crime levels. We followed up this line of inquiry by more thoroughly examining how the number of visitors to national parks affects crime rates. We (2007) conclude that the number of visitors to these national landmarks generally has no effect on crime. Therefore, if visitors are a determinant of crime, then the type of visitor that is attracted to casinos is very different from the type of visitor that is attracted to national landmarks.

The last main point on this issue concerns our empirical results on neighbor counties (Grinols and Mustard 2006, sect. VI). One reason that we examined how casinos in one county affect the crime rates in border counties is that if people from surrounding areas substituted committing a crime in a casino area for committing a crime in their home area, then we expected crime to decrease in the neighbor counties, because a substantial share of visitors to casinos that we used to identify the effect of casinos on crime come from nearby (Grinols and Mustard 2006, 42). However, the data clearly do not show such a pattern. The effect of a casino on the crime rate of neighbor counties is very similar to the pattern in the home counties, but the magnitude is not as large. The data clearly reject the hypothesis that casinos reduce crime in border counties. If anything, casinos lead crime to either stay the same or increase in the surrounding areas (depending on the type of crime and how many years after it opened). This pattern is inconsistent with the notion that criminals in neighboring areas are substituting the location of their crime from border counties to casino counties.

To summarize, the choice of the dependent variable in part depends on the question you want to ask. Also, there are empirical limitations to doing exactly what Professor Walker proposes because there are no systematic data that record the annual number of visitors by county. Lastly, to the extent that we can obtain quality data on a subset of visitors, the data show that visitors to national landmarks do not raise crime rates.

### MEASUREMENT ERRORS IN THE CRIME DATA

Next, Professor Walker is concerned that the crime data may contain measurement error. He focuses on one way that may have occurred—that during the 1990s there was a change in the way that the crime data were put together. However, the measurement errors in crime data go beyond this specific concern and are well known to individuals who work in this area. Other examples are that cities may report crime differently, that the degree of underreporting differs across geographic region, etc. The F.B.I. Uniform Crime Report, which provides the crime data, regularly cautions against comparing crime rates across cities, counties, metropolitan areas, and states because different jurisdictions report crime rates in different ways.<sup>3</sup> Clearly there is the *potential* for bias, but Walker's contentions do not go much beyond that. He does not reference the way the literature addresses potential bias, does not provide any frame of reference for understanding how or when the estimates could be biased, and offers no evidence that the estimates actually are biased. Nevertheless, we examine the possibility of measurement error in more detail and consider the implications they have for our estimated casino effects.

First, if the measurement error is uncorrelated with the error term, then the coefficient estimates are unbiased and the standard errors are larger than they should really be. That means that our reported estimated effects are less precise than the real effects. To the extent that such measurement error exists, our published results are biased *against* our finding an effect.

Second, while it is important to acknowledge the possibility of measurement error in the data it is also important to realize that there is an extensive literature using crime data that has developed ways to deal with the measurement error. For example, many studies, including ours, use county-level, metropolitan-area, or state-level fixed effect regressions. Fixed effect regressions are very powerful because they control for differences in reporting and administration across jurisdictions that are unobservable to the analyst.

Third, if the measurement error in crime rates varies over time and across counties in a way that is correlated with casino openings and that fixed effects do not completely control for, then there is a possibility for the coefficient estimates to be biased. If true, then our published estimated effects could be biased down. So the concern that Professor Walker articulates on this point could make our results even stronger. His concern works against our results only in a relatively narrow set of possible situations, and he provides no statistical evidence that such is case. When we first started to write the paper we ran some simple correlations between counties where casinos opened and counties that changed the way that crime rates were estimated in the mid 1990s, and found that there was little cor-

<sup>3</sup> For examples of cautions about the data, see <http://www.fbi.gov/ucr/cius2006/rankingmessage.htm> and [http://www.fbi.gov/ucr/cius2006/about/variables\\_affecting\\_crime.html](http://www.fbi.gov/ucr/cius2006/about/variables_affecting_crime.html).

relation between the two.

In sum, the question is not whether there is measurement error in the data—virtually all data sets have some measurement error. Instead, we want to know whether the measurement error that exists can be addressed using various research methods, and if not, whether the measurement error is sufficiently large and correlated with the variables of interest in such a way as to lead to an overestimate of the true effect. Neither our inquiry nor Professor Walker's criticism provided evidence of such systematic correlation. Therefore, we have no reason to believe that the measurement error is large enough or systematically correlated enough with the variables of interest so as to lead to the estimated effects being overstated.

#### **SIMULTANEITY—COUNTIES MAY SELF-SELECT INTO THE “CASINO COUNTY” CATEGORY**

Professor Walker's third concern is that our results may suffer from simultaneity bias caused by counties self-selecting into the casino-county category. He says, “counties with relatively poorly performing economies might be more likely to introduce casinos and to do so more quickly,” and “counties may have legalized casinos in part because of economic hardships...representing factors that may be driving Grinols and Mustard's results” (12). Once again, no theoretical details, supporting statistical evidence, or further explanation is given as to precisely how these facts would bias the results of our paper. The only empirical evidence cited is that of Fink, Marco, and Rork (2004), but that paper deals with states self-selecting into lotteries, not counties into casinos.

Simultaneity is a consideration that could apply to any multiple regression. If  $y$  is the dependent variable, variables  $x$  and  $y$  could be correlated either because  $x$  is an exogenous variable that influences  $y$ , or  $x$  and  $y$  are endogenous. In the present context, if “crime causes casinos,” then the estimates of the effect of a casino on crime may be biased. We were keenly aware of the possibility of endogeneity<sup>4</sup>, and we spent considerable time on this issue.

We addressed the question, first, in a theoretical way. Our paper was the first paper to provide a clear theoretical treatment of how casinos could affect crime differently over time (Grinols and Mustard 2006, sect. III C, 31). We showed that casinos may be more likely to be placed in high crime areas, a concern Professor Walker reiterates. However, we also stated that crime may be lower in counties with casinos before they open because of the crime-reducing effects of better labor market opportunities through the construction and building phase of the

---

4 Professor Walker acknowledges, “Indeed, Grinols and Mustard mention the common belief that casinos are more likely to be placed in high-crime areas and that the number of casinos began increasing rapidly in 1991” (12).

casino (a potential source of bias that could lead us to understate the effects of casinos on crime that Professor Walker does not mention). Therefore, there are theoretical reasons why crime in casino counties would be higher or lower before opening, and we must examine the data to learn more.

Second, our paper was the first one in this literature to empirically test the degree to which casinos have different effects over time (Grinols and Mustard 2006, sect. V and VI). We did this by including a series of leads and lag variables. Section IV C (35) shows the empirical specification that we used to estimate intertemporal effects. The baseline regression results include two lead and five lag indicator variables, but we estimated many alternative specifications that included up to five lead and seven lags. The lead variables allow us to explicitly test concerns about endogeneity.

We first regressed the crime rate on the casino leads and lags and county and year fixed effects with no other control variables. These results generally showed that the lead variables are positive and frequently statistically significant—providing some evidence that crime is higher in casino counties prior to casino openings (Grinols and Mustard 2006, Table 3, 35-36). Next, we re-ran the same regressions and included a large set of control variables (Table 4).<sup>5</sup> This time the results generally showed that the coefficient estimates on the lead variables, which were statistically significant prior to inclusion of our large set of control variables, were not statistically different from zero.<sup>6</sup> Once control variables were used, casino counties were not statistically different from non-casino counties before casinos were introduced. We emphasized these results, because this specification provides a better fit and accounts for endogeneity.

The third piece of evidence on simultaneity comes from Grinols and Mustard (1999), a working paper version of the published paper, in which we devoted considerably more attention to this problem. There we identified a set of instrumental variables and ran sets of two-stage regressions to control for potential endogeneity. In this earlier version of the paper we wrote:

The earlier empirical results and Figures 7 and 8 [shown in that paper] show that casino and non-casino counties have very similar crime patterns prior to casino opening. We do not believe that simultaneity is a significant problem. Nevertheless, to investigate

5 Grinols and Mustard (2006, sect. II) lists and defines the basic control variables in detail. They include population density, population distributions by race, age, and sex, and an assortment of income and unemployment variables. This list of control variables is the most exhaustive of studies in this literature. In robustness checks later in the paper we also included control variables on law enforcement variables like arrest rates and four capital punishment variables.

6 We studied seven crimes—murder, rape, robbery, aggravated assault, robbery, burglary, larceny, and auto theft. The leads were not statistically different from zero for the first six offense types. Only for auto theft was there some evidence that, after controlling for an array of variables, crime rates were higher in casino counties before the casino opened. We talk about this case in more detail in the original paper.

whether our conclusions might be biased by simultaneity, we conducted Hausman tests on each of the crime regressions.<sup>7</sup> The instruments we used were indicator variables for counties with major rivers,<sup>8</sup> counties that bordered states, counties that bordered states and bordered metropolitan statistical areas with population of 50,000 or greater, counties with Indian reservations, and counties with Indian gambling compacts for the years after 1990. Casinos are often located on major rivers (there are riverboat casinos in Indiana, Illinois, Iowa and Louisiana), are deliberately placed on the borders of states to attract clients from neighboring areas (Tunica, Mississippi borders Memphis, Tennessee and East St. Louis, Illinois borders St. Louis, Missouri) and are often located on Indian reservation trust land.

All of the regressions passed the Hausman test except robbery.<sup>9</sup> However, even in this case the casino coefficients for the OLS and 2SLS regressions were very similar. Both methods showed virtually the same pattern over time and very similar coefficient estimates.<sup>10</sup> Because the Hausman test rejects simultaneity and the results for OLS and 2SLS are nearly the same, we use the OLS estimates for the rest of the paper. (Grinols and Mustard 1999)

We presented working paper versions of the published paper at annual meetings of the American Law and Economics and of the American Economic Association, and at scholarly workshops at the Universities of Buffalo, Georgia, Illinois, and Rochester. Workshop and conference participants consistently indicated that we spent too much time examining this problem of simultaneity because there was little evidence that it existed or was problematic. Referee and editorial comments reiterated this theme. Therefore, we dropped the instrumental variable approach with two-stage least squares from the published version of the paper.

To conclude, we believe that it is very important to control for endogeneity in doing quality empirical work. The published and working paper versions of this paper spent considerable time investigating whether endogeneity was a significant concern, and the data clearly show that it is not. Until there is evidence to the contrary, we see no reason to deviate from the original conclusion (Grinols

7 The Hausman (1978) specification test compares an efficient and consistent estimator under the null hypothesis with another consistent estimator. The test evaluates whether there is sufficient difference between the coefficient sets to reject the null hypothesis that the original estimator is consistent.

8 To be included a county had to be in the top 10 in length, volume of flow, and watershed area. This screen resulted in the Mississippi, Ohio, and Missouri Rivers, all of which have riverboat casinos.

9 The null hypothesis failed to be rejected at the 100, 100, 99, 84, 48 and 26 percent levels for murder, larceny, auto theft, rape, burglary, and aggravated assault, respectively. The P value for the robbery  $\chi^2$  statistic was marginally significant at .05.

10 The simple correlation between the lag coefficients of the robbery OLS and 2SLS regressions was  $p = .99$ .

and Mustard 2006): “the casino-opening lead variables suggest that after controlling for other variables casinos were not more likely to be placed in areas that had systematically different crime environments than other regions” (40).

**CAN WE IDENTIFY THE EFFECT OF A CASINO? IDENTIFICATION—THE DUMMY VARIABLES USED TO ACCOUNT FOR CASINOS DO NOT ALLOW THE AUTHORS TO ISOLATE THE CRIME EFFECT CAUSED BY CASINOS**

Professor Walker’s fourth point is that indicator variables do not lead us to isolate the effect of crime caused by casinos. In this section he again proposes a variety of *potential* problems, but provides no data to show that they are, in fact, problems. He reiterates a comment that we brought up in the original paper—that we would very much like to use a more dynamic measure of casino growth like casino revenues. However, such data were not available in any systematic and reliable manner. So we took an alternative approach. As we said in our original paper, if good data on casino revenues are obtained in the future we believe that would make a significant contribution to the literature.

Beyond this concern, Walker follows up on his earlier claim that “the crime effect found by Grinols and Mustard in casino counties is due to *tourism in general* rather than to *casino-specific tourism*” (13, emphasis in original). We certainly acknowledge that this is a possibility, but to the extent that data exist on this issue, again, we found no evidence for the contention, and Walker does not provide any evidence. Similarly, he claims, “In general, anything that distinguishes the casino counties from national norms will be picked up by the dummy” (13). It appears as though he is referring to the potential for omitted variables to bias our coefficient estimates, but once again offers no evidence of what the omitted variables might be or how they are correlated with casino openings. Grinols and Mustard (2006, sect. I) provides an overview of the literature on this topic, shows that omitted variable bias is a problem because most studies include very few control variables. In contrast, our study included by far the largest set of control variables and we included county and year fixed effects that control even for unobserved differences across counties and time.

Interestingly, in his discussion of omitted variables, Professor Walker does not refer to variables that were omitted from our base regressions, that do affect crime, and that are correlated with casino openings. We (2006, sect. V C) examine how the casino estimates are affected when law enforcement control variables like arrest rates and measures of capital punishment are included. When these variables are included, the estimated casino effects were larger than the ones we emphasized in the paper. In this case, as in others in the paper, we deliberately chose to emphasize results that provided smaller estimates of the casino effects, something that Professor Walker neglects to mention.

As a matter of logic, it is impossible for us to prove that some unknown variable that affects crime and is correlated with casinos in such a way as to lead our results to be biased upward does not exist. No matter how exhaustive our examination, there is always the logical possibility that some, as yet unknown, mechanism might be found. However, in such cases, the burden is on scholars like Professor Walker to produce an actual example of what they claim, and provide the evidence that proves that it operates in the manner they say it does.

### LAG 5 CRIME RATES

Professor Walker is concerned that other calculations might give different numbers than the statistics we report in our paper for the effect after five years from opening of casinos on crime rates. For example, he says,

the early-adopting counties represented by Lag 5 crime rates likely attracted more tourism than those counties represented in more recent lag periods, when casinos had become more widespread. This would suggest that the Lag 5 casino county crime rates are probably the most overstated of any period's. (14)

However, Professor Walker provides no evidence for this conjecture. In fact, later-adopting casinos are likely to have had higher tourism in the period we study because novelty initially works in favor of tourism but fades with time.

Other conjectures are simply false. For example, Walker says, "the Lag 5 crime rate estimates are the highest of any in the model" (14). We wrote:

We checked whether the rising patterns of coefficient estimates in the last three years with the lag 5 estimated coefficients positive and significant persisted or disappeared after the fifth year. Estimates of the sixth- and seventh-year lags were 745 and 1,069 for larceny and 201 and 229 for burglary, respectively. Moreover, lags 5 through 7 pass a 5% F-test for significance for both offenses. (Grinols and Mustard 2006, 38)

The lag 5 estimate for larceny was 614.695, which is *lower* than the reported estimates for lags 6 and 7. However, even if the fifth year lag is larger, why is this a concern? Research should not pre-judge outcomes. Estimates of the lag 5 crime rates are the best econometric estimate of the impact of a casino on crime rates in the fifth year after casino opening. They remain, therefore, the best statistic we have to calculate the effect of casinos on crime rates after five years.

## CONCLUSION

It is important to keep a balanced perspective. We undertook our comprehensive study of casinos, crime, and community costs because we believed that existing research could be improved on. We (2006, sect. I) provide a detailed account of weaknesses of the prior casino-crime literature. First, no previous study examined the intertemporal effects of casinos on crime. Second, many studies used very small and sometimes selected samples. Third, some studies made conclusions about crime rates without studying actual crime rates. Fourth, most studies used very few control variables and suffered substantial omitted variable bias. Fifth, few studies examined the theoretical links between casinos and crime. Lastly, many studies were agenda driven and funded by organizations with a vested interest in the outcome of the research.

What are the facts? We know that casinos cause crime, because we have many cases, even documented in the press, where individuals engaged in crime for reasons that trace to their casino gambling. A few examples were cited in our original paper on pages 32 and 33 to document this fact. The research question, therefore, is not whether casinos cause crime, but whether one is able to show the magnitude of this connection statistically and separate it from the many other causal sources. Those who engage in statistical work know that it can be hard, painstaking, and tedious, especially in situations where many factors contribute to the object of study. A failure to show a link does not prove there is none, only that the researchers may have used a sample too small or methodology too weak to make the connection.

Our paper makes clear and substantial improvements to the existing body of literature in at least these six dimensions. Is our paper perfect or without blemish? Absolutely not. For research to be moved forward we can use new methodologies, formulate better theories, and examine different data to better understand how mechanisms work and in what context they work. We welcome criticism and new research along this dimension. Unfortunately, the Walker criticisms provide nothing in this vein. He raises *potential* criticisms that are well-known in the literature and are largely dealt with in our working paper and published versions of our paper. Although he reiterates our discussion that these concerns may bias our results, he provides no new evidence that they in fact occur or are important if they do occur. In light of this absence of new information we have no reasons to alter the conclusions of our initial research on this topic.

## REFERENCES

- Fink, S.C., A.C. Marco, and JC Rork.** 2004. Lotto nothing? The budgetary impact of state lotteries. *Applied Economics* 36: 2357-2367.

- Grinols, Earl L., David B. Mustard, and Cynthia Hunt Dilley.** 1999. Casinos and Crime. University of Georgia. Working draft.
- Grinols, Earl L. and David B. Mustard.** 2006. Casinos, Crime, and Community Costs. *The Review of Economics and Statistics* 88(1): 28-45.
- Grinols, Earl L. and David B. Mustard.** 2007. Visitors and Crime. University of Georgia. Working draft.
- Hausman, J.** 1978. Specification Tests in Econometrics. *Econometrica* 46(6): 1251-1271.
- Walker, Douglas.** 2008. Do Casinos Really Cause Crime? *Econ Journal Watch* 5(1): 4-20. [Link](#).

#### ABOUT THE AUTHORS



**Earl L. Grinols** is Distinguished Professor of Economics at the Hankamer School of Business at Baylor University and former Senior Economist for the Council of Economic Advisers. A University of Michigan Angell Scholar and a mathematics *summa cum laude* graduate of the University of Minnesota, he earned his PhD at MIT. In addition to Baylor, he has taught at MIT, Cornell University, the University of Chicago, and the University of Illinois. His current research, health care, is treated in his book nearing completion, co-authored with James Henderson of Baylor, titled *Health Care for Us All: Getting More for Our Investment*. His email is [Earl\\_Grinols@baylor.edu](mailto:Earl_Grinols@baylor.edu).



**David B. Mustard** is an Associate Professor of Economics in the Terry College of Business at the University of Georgia and a research fellow at the Institute for the Study of Labor ([link](#)) in Bonn, Germany. Mustard earned a Ph.D. in Economics from the University of Chicago. His research focuses on microeconomic policy-related questions, especially law and economics, crime, casino gambling, lotteries, gun control, sentencing, labor economics, education and merit-based aid.

Mustard has won many university- and college-wide teaching awards and regularly teaches in UGA's honors program. His email is [mustard@terry.uga.edu](mailto:mustard@terry.uga.edu).

[Go to January 2008 Table of Contents with links to articles](#)

---

## Highway Penetration of Central Cities: Not a Major Cause of Suburbanization

WENDELL COX<sup>1</sup>, PETER GORDON<sup>2</sup>, AND CHRISTIAN L. REDFEARN<sup>2</sup>

**A COMMENT ON:** NATHANIEL BAUM-SNOW, “DID HIGHWAYS CAUSE SUBURBANIZATION,” *QUARTERLY JOURNAL OF ECONOMICS* 122(2), MAY 2007: 775-805.

### ABSTRACT

NATHANIEL BAUM-SNOW HAS DONE INTERESTING WORK ON THE IMPACT OF highways on the process of suburbanization. His basic idea is to estimate the statistical links between a central city’s new-highway penetration and population change. The article’s title is “Did Highways Cause Suburbanization?” published in the *Quarterly Journal of Economics* 2007. He has published related work in the *Journal of Urban Economics* (2007b). There are three judgments we would make about the article: two are positive but the third questions what can be reasonably learned from the author’s findings.

(1) *Remarkably good:* Baum-Snow’s *QJE* paper “assesses the extent to which the construction of new limited access highways has contributed to central city population decline” (775). To do so, he uses “planned portions of the interstate highway system as a source of exogenous variation” in transportation conditions. Baum-Snow’s investigation appears to give a remarkably detailed treatment of the variables included in the model. He reports on numerous robustness tests. We do not know and cannot comment on the numerous underlying judgment calls he had to make in the details of his investigation, but the ambition, care, and thoroughness with which he has developed and utilized the data used in his model are remarkable and impressive. Indeed, the “highways caused suburbanization” thesis is one that has been suggested many times, but Baum-Snow may be the first to seriously attempt to test it.

---

<sup>1</sup> Conservatoire National des Arts et Metiers. Paris, France 75141.

<sup>2</sup> School of Policy, Planning and Development, University of Southern California. Los Angeles, CA 90089.

(2) *Statistically remarkable*: Based on the new highway variable developed in the paper, Baum-Snow's econometric results lead him to conclude that "one new highway passing through a central city reduces its population by about 18 percent." That is remarkably large. The size of the effect is shown by doing the math on the counterfactual of not having built the highways in question: Baum-Snow notes that, in fact, between 1950 and 1990 the aggregate population of central cities in the United States declined by 17 percent despite national population growth of 64 percent, but, by contrast, the estimates from his model imply that, had the interstate highway system not been built, the "aggregate central city population would have *grown* by about 8 percent" (775). This remarkable result should prompt flashing lights for anyone working on related issues of urban economics and demographics. Given the substantial decline in household sizes that has occurred since 1950, this would have required 40 percent more housing units to be crowded into the cities—a densification that has occurred in no constant-geography, fully developed central city. Again, we make no criticism of the way Baum-Snow carried out his econometric investigation. On the customary presumption that it was done in a workmanlike, honest way, the fact that it produced the reported results is important news that should alert us.

(3) *Remarkably quiet on alternatives*: Having obtained the remarkable results he got, Baum-Snow wants us to believe that they are reasonable estimates of the true impact of highways on central city population. The second sentence of the paper is as follows: "This paper demonstrates that the construction of new limited access highways has contributed markedly to this central city population decline" (775). In his conclusion he writes:

I evaluate the importance of highways for explaining central city population decline by examining the counterfactual evolution of aggregate city population where no highways were constructed. A coefficient of -0.09 implies that had the interstate highway not been built, aggregate central city population in MSAs in the primary sample would have increased by 8 percent between 1950 and 1990. (800)

Nowhere does Baum-Snow suggest that there are reasons to question these results and only briefly does he mention other possible explanations for suburbanization (775, 801). He cites "changes in the amenity value of suburbs relative to central cities" (775), but only fleetingly and calls it a "complementary explanation." He does not look beyond his investigation at anything that would cast doubt on his results. In answer to the question posed in the title of his paper, "Did Highways Cause Suburbanization?" Baum-Snow is effectively saying: Yes, to an economically significant extent. He writes: "Estimates presented below indicate that highways can explain about one-third of the change in aggregate central

city population relative to metropolitan area population as a whole” (775). The suggestion that we should take the model estimates as reasonable estimates is remarkable. Baum-Snow’s investigation appears to be well-done, but there is also considerable evidence to suggest that the large correlation he estimates is spurious. Again, Baum-Snow suggests that central city populations would have grown had the interstate highways not been built, but there is much evidence to suggest that, under that counterfactual, central city populations still would have declined, though perhaps not by as much as they actually did.

Suburbanization has, for a long time, been a trend based on consumer preferences and larger trends, notably rising wealth and transportation and communications improvements (including the highways Baum-Snow investigates). Jackson (1985) finds U.S. suburbanization began at the end of the 19<sup>th</sup> century. Indeed, he refers to “streetcar suburbs.” The 20<sup>th</sup> century U.S. experience is shown in Figure 1, which shows the percentage of US population living in metropolitan areas, and breaks that percentage down into central cities population and suburbs. The growth of the suburbs relative to the central city is seen well before 1950. Moreover, in the figure the relative decline of the central cities is understated because central cities have been annexing suburbs for many years.<sup>3</sup>

The simple broad narrative is that, by and large, suburban living expanded throughout the twentieth century *because it could*. Around the world, as incomes rise, people choose the mobility of the automobile; they overwhelmingly prefer the range and choice of personal transportation. As they choose automobility, origins and destinations disperse; and as these disperse, the attraction of the auto grows. It is a self-reinforcing cycle that is facilitated by better highways. But as with most public sector infrastructure developments, these usually follow rather than lead.

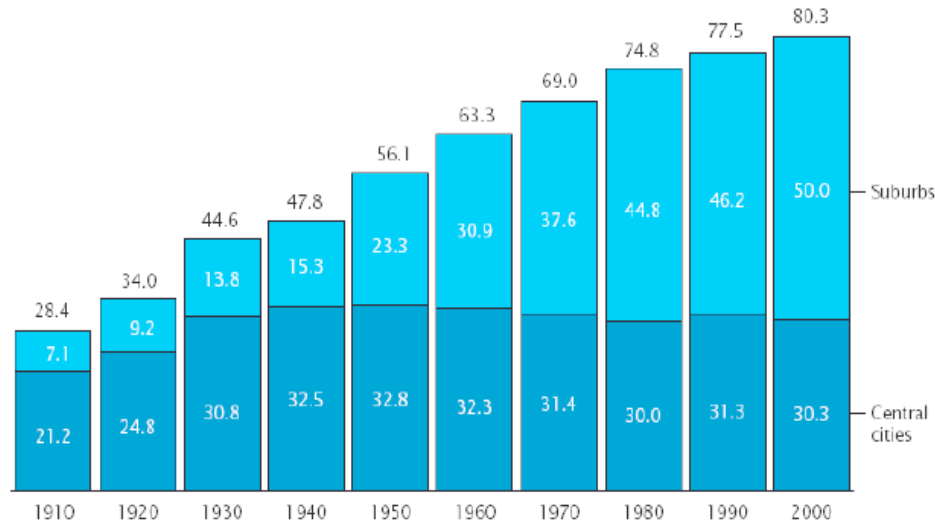
As suggested earlier, we think that the history of urban dispersal is a natural trend reflecting consumer preferences and the historical developments of transportation modes and living standards that enabled dispersal. Dependency on a “central city” or “central business district” that might be in the area is generally much less important than has been supposed. Thus, we doubt that penetration as such is a significant cause of suburbanization.

In this view, *highway penetration of central cities remains a minor factor* in the story. The central cities’ importance would have continued to decline even if the interstate highway system had not been built. The reasons for doubting Baum-Snow’s result are not esoteric or arcane. Rather, they are obvious and well-known.<sup>4</sup>

3 Nicole Stelle Garnett (2007): “By 1920, ‘exit’ [from central cities] had become a mass phenomenon” (6). She also notes that exit is now over. “The exit story ... no longer captures the American suburban experience. For a majority of Americans, suburbs have become points of entrance to, not exit from, urban life. Most suburbanites are ‘enterers’—people who were born in, or migrated directly to, suburbs and who have not spent time living in any central city” (1).

4 Also, there was undoubtedly some displacement of population, inside and outside central cities, as housing units were demolished and people were forced to move as a consequence of highway construc-

**Figure 1: Percent of Total Population Living in Metropolitan Areas and Their Central Cities and Suburbs, 1910-2000 (%)**



Source: Hobbs, Frank and Nicole Stoops. 2002. Demographic Trends in the 20th Century. U.S. Census Bureau, Census 2000 Special Reports, Series CENSR-4.

Empirical work must meet two standards. It must meet the rules of evidence that statisticians have developed and it must be plausible in light of what we know about the topic. The Baum-Snow paper, in our view, only meets the former standard. Deirdre McCloskey has famously described economists as looking for their lost keys under the lamppost because the light is better there; she criticizes them for failing to get beyond the light of the lamppost (McCloskey 1990, 73). Similarly, Thomas Mayer (1993) has described the problem as economists fashioning and polishing one very strong link, rather than tending to the entire chain of a learned line of argument. We suggest that, unfortunately, Baum-Snow's narrow focus on the results of an econometric test serves as a good example of the problem described by McCloskey and Mayer.

#### **THE MECHANISM POSITED BY BAUM-SNOW: THE ASSUMPTION OF GREAT DEPENDENCE ON THE CENTER IS MISGUIDED**

In explaining the central-city-penetration effect, Baum-Snow sketches the mechanisms at work with the standard monocentric model of cities: "In its simplest form, this theory assumes all employment occurs at a central location and the rental rate of land adjusts as a function of distance from the center to com-

---

tion. But we have no data on the extent to which this occurred and expect that it was relatively minor.

pensate for different commuting times of identical agents. A standard extension allows for heterogeneity in various factors that affect the demand for space and the value of commuting time” (785). The primary explanation Baum-Snow offers for his highway effect, then, is that primary employment activity is monocentrically distributed, and better highways enable people to move their residence away from the center and yet still get to their jobs on a daily basis without spending too much time commuting.

Relying on the standard model in this way to sustain the idea that highways are crucial drivers of suburbanization depends crucially on the idea that good and speedy access to activities in the central city allows the suburbs to grow. This has been a common presupposition in urban and transportation studies, but in fact it is much less sound than is often supposed. Rather, decent road transportation makes suburbs highly viable so long as they can connect to needed resources, *wherever they come from*. One study has shown that in 2000, in the largest U.S. metropolitan areas, less than ten percent of the jobs were in the downtown, 15 percent were in subcenters (many outside the central city) and 78 percent were dispersed everywhere else (also much of it outside the central city; Lee, 2006). This suggests two problems with Baum-Snow’s theoretical model. First, the monocentric model in no way describes the real world. Second, “the center” of the monocentric model is not well represented by the Census Bureau’s central city; highway penetration or access to the conventionally defined central city is not what the monocentric model is about. For example, among the 30 largest central cities in 1950, there was a variation in land area from approximately 40 square miles to 450 square miles. Our Figure 1 reveals that in 1950, the central-city areas “penetrated” accounted for 58.5 percent of metropolitan area population (which in turn was 56.1 percent of the U.S. population). But this is not what the monocentric model describes. The real “centers”, the downtown areas, were typically small, generally accounting for under three square miles (most under two), even in 2000 (with the exceptions of New York and Chicago).<sup>5</sup>

The spatial pattern that we are describing has been in effect for some years. Although there are no historic data on intra-metropolitan employment location changes, we do know that commuting times have been remarkably stable for many years (Hafeez 2000). This means that we can infer that people and jobs have for many years co-located to the suburbs. That suggests a long history of significant suburbanization of employment. And that tells us that it is unlikely that highway access to the central city was crucial to suburban development.<sup>6</sup>

Later in his paper (801), Baum-Snow notes how employment, in addition to residence, has become more decentralized, and suggests that this too helps to explain his results about central-city highway penetration. He does not acknowl-

<sup>5</sup> <http://www.demographia.com/db-cbd2000.pdf>.

<sup>6</sup> We thank an anonymous referee for pointing out that the link between improved highways and better accessibility has been found to be weak by Winston and Langer (2006).

edge, however, that, if employment also becomes decentralized, the assumption of needing speedy access to the central city is weakened. Were he to look at just how extensive this phenomenon is, he might re-examine his reliance on the monocentric model. In fact, there is good reason to believe that highway development aids suburbanization *regardless of whether it connects to the central city*—most commuting and a great deal of travel and trade is suburb-to-suburb. The interstate highway system did facilitate suburb-to-suburb commuting (without being the main causal factor) but that is another study.

The author bases his analysis on the monocentric model because its comparative statics suggest that rising income and cheaper travel cause suburbanization. If monocentric thinking has misled Baum-Snow, it wouldn't be the first time that it has misled researchers. The model has analytical appeal, for its tractability. Also, in the days of primitive transportation it had some validity. But the convenient assumption of monocentricity is implausible, to say the least, when less than ten percent of large-metropolitan area jobs are in the downtowns.

### BAUM-SNOW'S INVESTIGATION

Before advancing our criticisms of Baum-Snow's paper, we should explain his investigation in a bit more detail. Again, we will not criticize the investigation as such, but rather the failure to consider evidence beyond the model and the investigation.

Baum-Snow identifies planned highway "rays" from suburbs to center cities, as shown in an early draft (from 1947) of the interstate highway plan (776). These are plausible instrumental variables because they cannot be said to be endogenous or responsive to suburbanization that may have occurred in later years. "[A] 'ray' is defined as a segment of road that connects the central business district (CBD) of the central city with the region outside the central city. If a highway passes through the central city, it counts as two rays whereas if a highway terminates in or near the central city it counts as only one. Rays must pass within one mile of the CBD and serve a significant area of the MSA out of the 1950-definition central city to be counted" (780-781). The author reports tests that show the subsequent change in rays was responsive to population change whereas planned 1947 rays could not be explained in this way. These rays are plausibly exogenous.

For controls, the author adds a constructed income variable, a Gini coefficient, central city radius, metropolitan area growth as well as fixed effects. The latter are actually poor proxies for other influences because a long time span is being considered; the cities have changed considerably and whatever the fixed effects variables describe in 1950 has very little meaning in 1990.

Mieszkowski and Mills (1993)<sup>7</sup> note that, "the trend toward suburbanization

---

<sup>7</sup> We want to thank an anonymous referee of reminding us of this important paper.

has been prewar as well as postwar, and has been international in scope.” (135). But more importantly, these authors tried to assess and compare two dominant theories of suburbanization, the one “favored by urban theorists and transportation experts,” (and similar to the Baum-Snow model); the other “stresses fiscal and social problems of the central cities: high taxes, low quality public schools and other government services, crime congestion and low environmental quality. These problems lead affluent central city residents to migrate to the suburbs, which leads to further deterioration of the quality of life and the fiscal situation of central city areas, which induces further out-migration.” (137). They conclude “[o]ur judgment is that both the natural evolution and the fiscal-social approaches are important” (144).

### EVIDENCE AGAINST THE PENETRATION THESIS: WESTERN EUROPE

If, as posited by Baum-Snow, the number of rays is associated with significant population loss in the constant-geography central cities, then the same effect should be found outside the United States. The only reference that Baum-Snow makes to any evidence beyond the post-WW II experience in the United States is a glancing remark in a footnote (777).

Indeed, the best place to test the thesis is in countries with cities with greater variation in ray penetration. In the United States, rays have penetrated the cores of all metropolitan areas over 1,000,000 population in 1990, and of most, if not all, over 500,000. Moreover, no central city that has ever achieved 400,000 population in the United States is not served by at least one ray. In Western Europe, however, rays have not penetrated into the core of many central cities. For example, the cores of London, Paris, Copenhagen, Milan, Antwerp, and Manchester are not penetrated.

Table 1 shows a list of fully developed Western European central cities that have (ever) achieved 400,000 population and have not materially increased their land area since 1950. The Table shows peak population, the 1990 cycle population, and the percentage changes in population at 1990.<sup>8</sup> It also shows whether the central city had ray penetration as of the year 1990.

The numbers in Table 1 make for some obvious calculations. We may compare population decline of cities with rays to cities without rays. Also, we may measure the declines, as of 1990, from the city's peak population.

Table 2 summarizes the results. It shows that the annualized average population decline for the Western European central cities, from the city's peak popu-

<sup>8</sup> As the table shows, not all countries conduct their population census in the same year. We have chosen the generic title “cycles,” indicating the general period of each country's census year. The growth rates are average annual rates between the dates noted in each row of the table. Hereafter, when our discussion refers to 1990 data, we mean data from that cycle.

**Table 1: Fully Developed (as of 1980) Central Cities in Western Europe<sup>+</sup>  
Achieving Peak Population of 400k or More in the 20<sup>th</sup> Century with  
No Material Land Area Expansion since 1950**

Central City	Freeway Ray in 1990?	Peak Population	Peak Population Year	1990 Census Cycle Population	1990 Census Cycle Year	1990 Change from Peak (Annualized%)
Amsterdam	N	866	1960	695	1990	-0.73%
Athens	Y	886	1981	772	1991	-1.37%
Barcelona	Y	1,753	1981	1,644	1991	-0.64%
Belfast	Y	444	1951	295	1991	-1.02%
Bilbao	Y	433	1981	370	1991	-1.56%
Birmingham	Y	1,113	1951	966	1991	-0.35%
Bologna	N	487	1971	404	1991	-0.93%
Bremen	N	606	1970	533	1987	-0.75%
Bristol	Y	468	1971	408	1991	-0.68%
Cologne	Y	1,014	1970	928	1987	-0.52%
Copenhagen	N	768	1950	467	1991	-1.21%
Dublin	N	566	1971	478	1991	-0.84%
Dusseldorf	N	705	1961	564	1987	-0.85%
Edinburgh	N	471	1951	401	1991	-0.40%
Essen	Y	730	1961	623	1987	-0.61%
Florence	N	456	1971	403	1991	-0.62%
Frankfurt	Y	672	1961	618	1987	-0.32%
Genoa	N	844	1971	679	1991	-1.08%
Glasgow	Y	1,088	1931	658	1991	-0.83%
Gothenburg	N	451	1970	422	1990	-0.33%
Hague	Y	605	1960	442	1990	-1.04%
Hanover	N	576	1961	494	1987	-0.59%
Leeds	Y	511	1961	424	1991	-0.62%
Lisbon	N	818	1981	663	1991	-2.08%
Liverpool	N	857	1941	482	1991	-1.14%
London	N	4,536	1901	2,504	1991	-0.66%
Lyon	Y	570	1936	415	1990	-0.59%
Madrid	Y	3,159	1971	3,010	1991	-0.24%
Manchester	N	766	1931	403	1991	-1.06%
Milan	N	1,713	1971	1,369	1991	-1.11%
Naples	N	1,278	1971	1,067	1991	-0.90%
Nuremburg	Y	499	1981	470	1991	-0.60%
Oslo	N	488	1970	458	1990	-0.32%
Paris	N	2,906	1920	2,157	1990	-0.42%
Rotterdam	Y	730	1960	579	1990	-0.77%
Sheffield	N	513	1951	432	1991	-0.43%
Stockholm	N	808	1960	674	1990	-0.60%
Stuttgart	N	649	1961	552	1991	-0.54%
Thessaloniki	N	406	1981	384	1991	-0.56%
Turin	N	1,191	1971	963	1991	-1.06%
Vienna	Y	2,031	1911	1,540	1991	-0.35%
Zurich	N	440	1960	365	1990	-0.62%

Note: Populations in thousands. Sources: Multiple sources, including national census data and reference volumes (almanacs, atlases & Statesman's Yearbooks).

<sup>+</sup> EU-15 (excluding East Germany), plus non-members Switzerland, Norway and smaller nations in this geography. London central city is pre-1966 boundaries (former London County Council).

lation to its 1990 population, was actually slightly larger for cities without rays. The typical time period between the year of peak population and 1990 is 31 years, meaning that over this period the average annual population loss was 0.79 percent for cities without rays—again larger than the 0.73 percent average annual loss in cities with rays. (Note that this result is not a function of outliers in either group: median gross losses show the same pattern.) These data make no reference to 1950 or any base year because we are not here looking for instrumental variables but simply showing that there has been substantial suburbanization with and without rays and certainly without anything like the U.S. Interstate Highway System.

**Table 2: Summary of Results: Western Europe Central Cities Population Change from Peak and Freeway Rays**

Central Cities	Number	Number Gaining Population	Number Losing Population	Mean Change in Population (Annualized%)	Median Change in Population (Annualized%)
With Rays in 1990	17	0	17	-0.71%	-0.62%
Without Rays in 1990	25	0	25	-0.79%	-0.73%

The following examples describe population losses that have occurred in essentially stable geography central cities between their peak population years and the early 1990s and which were also fully developed by 1980.<sup>9</sup>

- The central city of Copenhagen reached its population peak in 1950 and lost nearly 40 percent of its population by 1990. Copenhagen's core is not penetrated by a single ray, yet its population loss was nearly as great as some of the highest loss rates in the U.S. Rust Belt. (Over a similar period, Detroit, Cleveland and Pittsburgh lost about 45 percent of their population.) Copenhagen's loss<sup>10</sup> was near double that of Chicago and 1.5 times that of Philadelphia.
- The central city of Paris reached its peak population 1920 and has lost one-quarter of its population by 1990,<sup>11</sup> yet has no ray penetration. The Paris population loss is similar to that of Philadelphia and greater than that of Chicago.
- The central city of Liverpool reached its population peak in 1940 and lost

<sup>9</sup> Years indicate decadal census rounds. Actual census years may vary slightly (see <http://www.census.gov/ipc/www/cendates/cennaeni.html>).

<sup>10</sup> <http://www.demographia.com/db-kbn.htm>.

<sup>11</sup> <http://www.demographia.com/dm-par90.htm>.

more than 40 percent of its population by 1990, yet has no freeway ray penetration.

- The central city of London reached its peak population in 1900<sup>12</sup> and lost 45 percent of its population by 1990, yet has no freeway ray penetration.

The virtually universal dynamic of constant geography fully developed central city population decline is also evident in smaller cities. This includes examples such as the constant-geography central city of Antwerp<sup>13</sup> and of Brussels.

Again, we have not attempted to replicate Baum-Snow's analysis here but that is not our point. Besides, such an effort would be complicated, would need to include the constant geography of central cities that have materially expanded their land areas, and would need to take into consideration the much later freeway building and population growth patterns of Western Europe, much of which was barely recovering from the devastation of World War II in 1950. It seems clear, however, that any such replication would yield results materially different than those of Baum-Snow for the United States. In particular, the assertion that without freeway rays there would have been population growth in central cities (775) is indisputably at odds with the European experience. In fact, in every European central city that had achieved a population of 400,000 by or before 1950, the population had declined, with and without freeway rays. In some cases, the decline goes back to the early 20<sup>th</sup> century. It might be suggested that the population loss has been driven by other factors. That is precisely our point.

The dominance of suburban growth is not limited to Western Europe and the United States. Nearly all metropolitan area population growth has been in the suburbs in Japan, Canada, Australia and New Zealand in recent decades.<sup>14</sup>

#### **ANOTHER FACTOR THAT CLOUDS THE EMPIRICS: CENTRAL-CITY GREEN-FIELDS IN 1950**

Baum-Snow goes to admirable lengths to maintain constant geography to estimate population changes for central cities. This was a necessary filtering, otherwise the population trends in the constant-geography cities would have been camouflaged by the additional population added through the years within the expanded borders.

However, a second and important filtering of the data does not appear to

12 London County Council area, which was the central city before creation of the Greater London Council in 1965, generally called "Inner London." <http://www.demographia.com/dm-lonarea.htm>.

13 While Antwerp combined with some of its suburbs in the early 1980s, the 1990 district of Antwerp represents the constant-geography central city of 1950.

14 See: <http://www.demographia.com/db-highmetro.htm>. This analysis uses central city boundaries that are not adjusted for constant geography. It is likely that the use of constant geography would show virtually all metropolitan growth in these nations to have been outside the earlier constant-geography central city boundaries.

have been performed. Some central cities were not completely built out in 1950—that is, they had substantial greenfield areas. Suburban development has occurred on greenfield land in every U.S. metropolitan area since and has accounted for virtually all population growth in the corresponding urban areas.<sup>15</sup> As a result, urban footprints as they existed in 1950 could well have lost population, however, the population loss could have been camouflaged by growth in the undeveloped areas; any penetration effect, as suggested by Baum-Snow, could be obscured.<sup>16</sup>

Based upon the urban development trends that have occurred since 1950, it is likely that all cases of higher constant-geography 1990 population increases are the result of development on greenfield land. Ideally, the complication would be overcome if one could do the analysis upon constant-geography urban footprints within the constant-geography central cities. In many cases the urban footprint covered the entire 1950 geography of the central city (and more), but in some cases it did not.

Consider some rather substantial examples of the problem:

- Much of Staten Island, one of the five boroughs of the central city of New York, was undeveloped in 1950. A substantial portion of Staten Island's near doubling of population over the period occurred on greenfield land. This growth reduced New York's overall population loss.
- Within Los Angeles, much of the San Fernando Valley was not developed in 1950. The San Fernando Valley represented nearly one-half of the city of Los Angeles land area. Between 1950 and 1990, the population of the San Fernando Valley nearly quadrupled, rising by 900,000, representing 60 percent of the city's growth.

We cannot speculate on the impact of including New York and Los Angeles in Baum-Snow's data set, much less any other central cities that were not fully developed in 1950. Suffice it to say that given the general density decline<sup>17</sup> that has occurred in the cores of U.S. central cities over the period, it is likely that any constant-geography central city population gain observed was largely the result of new development on greenfield land. Our guess is that an analysis limited to an appropriate data set—central cities without substantial greenfield land in 1950 or the urban footprints within central cities—would find population declines in virtually all. Like the European experience, that would suggest that important factors in addition to ray penetration are driving depopulation.

---

<sup>15</sup> This fact can be verified by reviewing the population, land area and density changes in urbanized areas as defined by the United States Bureau of the Census. An urban area may also be called an urban agglomeration, which means the urban footprint.

<sup>16</sup> If the existence of substantial greenfield land is not a basis for excluding a central city from the sample, it would seem that Baum-Snow spent considerable effort determining the 1990 population of 1950 constant geography central cities. It would have been much simpler to have used central counties, which have had no boundary changes, and for which population data are readily available.

<sup>17</sup> A principal factor has been a reduction during the period of average household size by 22 percent.

## CONCLUSION

At first glance, Professor Baum-Snow's work seems sound and convincing because of his care and rigor in applying "best practice" of statistical norms to the canonical model of urban areas. Yet, a second glance compels us to take a closer look at the data for the U.S. cities and beyond. Baum-Snow invokes the well worn monocentric model to support the link between better highways and central city decline. The model does suggest an explanation for suburbanization from rising incomes and lower travel costs. But the monocentric model tells the story in a specific way that is narrow, implausible and unnecessary. Incomes and accessibility also enter into our simpler discussion of the dynamics of urban growth. Where we part company, then, is on the importance of improved access *to and from the "central city."* The monocentric story is actually inapplicable, first, because these "central cities" tend to be large and poorly represent the core of the mythical monocentric model, and, second, they began declining well before there was an interstate highway system.

Our look at Western Europe confirms that suburbanization is the norm, in line with our simple dynamics of growth discussion. Suburbanization abroad occurred without the highway penetration story that Baum-Snow elaborates. There are, then, strong reasons to doubt the conclusion that highway penetration of central cities was a major cause of suburbanization in the United States.

Our bigger point is that it is possible to do the research correctly and still reach the wrong conclusions.

## REFERENCES

- Baum-Snow, Nathaniel.** 2007a. Did Highways Cause Suburbanization? *Quarterly Journal of Economics* 122(2): 775-805.
- Baum-Snow, Nathaniel.** 2007b. Suburbanization and Transportation in the Monocentric Model. *Journal of Urban Economics* 62(3): 405-423.
- Garnett, Nicole Stelle.** 2007. Suburbs as Exit, Suburbs as Entrance. *Michigan Law Review* 106 (forthcoming).
- Hafeez, Bader A.** 2000. Journey-to-work travel trends in the U.S., 1977-1995. Ph.D. Thesis, Civil Engineering, University of Illinois at Chicago.
- Hobbs, Frank, and Nicole Stoops.** 2002. Demographic Trends in the 20th Century. *Census 2000 Special Reports, Series CENSR-4*. U.S. Census Bureau
- Jackson, Kenneth T.** 1985. *Crabgrass Frontier: The Suburbanization of the United States*. New York: Oxford University Press.

- Lee, Bumsoo.** 2006. *Urban Spatial Structure, Commuting and Growth in U.S. Metropolitan Areas*. Ph.D. Dissertation, Urban Planning, University of Southern California.
- Mayer, Thomas.** 1993. *Truth versus Precision in Economics*. Aldershot: Edward Elgar.
- McCloskey, Deirdre N.** 1990. *If You're So Smart: The Narrative of Economic Expertise*. Chicago: University of Chicago Press.
- Mieszkowski, Peter, and Edwin S. Mills.** 1993. The Causes of Metropolitan Suburbanization. *Journal of Economic Perspectives* 7 (3): 135-147.
- Winston, Clifford, and Ashley Langer.** 2006. The Effect of Government Highways Spending on Road Users' Congestion Costs. *Journal of Urban Economics* 60: 463-483.

#### ABOUT THE AUTHORS



**Wendell Cox** is an international public policy consultant specializing in urban policy, transport, and demographics. He is visiting professor at Conservatoire National des Arts et Metiers, Paris (a national university). He has completed projects in North America, Europe, Asia, Australia, South America and Africa. Clients have included government agencies, public policy institutes, and industry organizations. His books include *War on the Dream: How Anti-Sprawl Policy Threatens the Quality of Life* and *The Wal-Mart Revolution* (co-authored with Richard Vedder). He is co-author of the *Demographia International Housing Affordability Survey* and author of *Demographia World Urban Areas* (the only worldwide source on population densities for all urban areas over 500,000 population in the world). His email address is: [demographia@gmail.com](mailto:demographia@gmail.com).



**Peter Gordon** is a Professor in the University of Southern California's School of Policy, Planning and Development. Gordon's research interests are in applied urban economics. He is the co-editor (with David Beito and Alexander Tabarrok) of *The Voluntary City* (The University of Michigan Press, 2002). Gordon and his colleagues have developed the Southern California Planning Model, which they apply to the study of economic impacts. He has consulted for local, state and federal agencies, the World Bank, the United Nations and many private groups. Gordon received the Ph.D. from the University of Pennsylvania. His email is [pgordon@usc.edu](mailto:pgordon@usc.edu).



**Christian L. Redfearn** is an assistant professor in the School of Policy, Planning, and Development at the University of Southern California. He joined the faculty at USC after completing his Ph.D. in economics at the University of California, Berkeley. An urban economist, he is engaged in research projects that focus on the evolution of metropolitan land and real estate markets. Specific examples of his current research involve neighborhood evolution, housing price measurement

issues in a complex urban setting, trading property rights and externalities in historic preservation districts, the hierarchy of urban land markets, as well as the spatial organization of metropolitan employment and its persistence over long periods of time. He has published recently in the *Journal of Urban Economics*, the *Journal of Regional Science*, *Real Estate Economics*, and *Environment & Planning A*. His email is [redfearn@sppd.usc.edu](mailto:redfearn@sppd.usc.edu).

[Go to Reply by Nathaniel Baum-Snow](#)

[Go to January 2008 Table of Contents with links to articles](#)

---

## Reply to Cox, Gordon, and Redfearn's Comment on "Did Highways Cause Suburbanization?"

NATHANIEL BAUM-SNOW<sup>1</sup>

**A REPLY TO:** WENDELL COX, PETER GORDON, AND CHRISTIAN L. REDFEARN "HIGHWAY PENETRATION OF CENTRAL CITIES: NOT A MAJOR CAUSE OF SUBURBANIZATION," *ECON JOURNAL WATCH* 5(1), JANUARY 2008: 32-45. [LINK](#).

### ABSTRACT

I take it as a compliment that my paper has generated sufficient interest to merit the comment by Cox, Gordon, and Redfearn. It is my intention that my paper's data construction and methods are sufficiently transparent such that others can evaluate and replicate my results at a relatively low cost. Indeed, I encourage Cox, Gordon, and Redfearn to look for themselves at the programs used for data construction and model estimation. A CD-ROM with all of the data construction and estimation results is available to any interested party from me upon request.

The comment takes the general view that my empirical finding that each additional highway passing through a central city causes about an 18 percent decline in its population cannot possibly be right because it is "too big". Further, the authors decry my lack of attention to other potential sources of population decentralization. On the second point, I do not dispute that forces other than transportation must be at play in driving population decentralization. These include increases in nonlabor income, changes in production and information technologies such that productivity spillovers can operate over larger distances and relative amenity values of cities and suburbs. Evaluating the importance of these mechanisms goes beyond the scope of my highways paper and requires at least one paper each. A working paper version of my *Quarterly Journal of Economics* article discussed these other mechanisms more extensively.

In the process of the research project, I devoted considerable attention to the size of the highway-penetration effect. To be confident about my results, I undertook an extensive set of empirical robustness checks, the most important

---

<sup>1</sup> Department of Economics, Brown University. Providence, RI 02912.

and time consuming of which was to estimate a version of the regressions allowing identification to come only from time series variation in the highway infrastructure within metropolitan areas. In addition, I developed the companion theoretical paper (Baum-Snow 2007b), which was originally a part of the empirical paper, to evaluate how large a tractable theoretical model predicts the effects of highways to be. The simulation results from this paper demonstrate that the monocentric model mechanism alone generates conservative estimates that are remarkably similar to the empirical estimates arrived at completely independently. While I agree that the monocentric model could not possibly capture all aspects of the true data generating process, one may expect other mechanisms only to make the true treatment effect larger than that predicted by a monocentric model. The value of using such a model is that it is simple enough to generate qualitative and quantitative implications that are robust to specification.

I consider the main criticisms of my paper in turn:

1. *Using his estimates, Baum-Snow calculates that, had the interstate highway system not been built, the aggregate population of 1950 geography central cities would have grown by 8 percent between 1950 and 1990 rather than declined, as observed, by 17 percent. The magnitude of this increase cannot possibly be right because not enough housing could ever be built to increase population by 8 percent in a given built up geographic area.*

This claim cannot be true given that the 1950 geography of San Diego grew by 72 percent in population between 1950 and 1990. This city represents the maximum for change in log constant geography central city population reported in the Appendix table (Baum-Snow 2007a, 804). There are 11 other cities with more than 8 percent growth 1950 to 1990.

2. *It is not highways, it is rising incomes plus the automobile that caused suburbanization.*

I certainly agree that cars have been essential for population decentralization. But the fact remains that the car has come to dominate commuting in almost every U.S. metropolitan area, yet we saw much greater changes in the spatial distribution of the population in cities receiving more highways. If highways are not important, you have to tell a story as to what unobservables correlated with actual highways (and those in the 1947 plan) could generate the empirical pattern.

3. *The paper relies too heavily on the monocentric model.*

The paper in no way relies on the monocentric model for identification. It estimates a treatment effect which may be driven by any number of data-generating mechanisms. The paper is not an exercise in structural estimation, though if one wanted to limit oneself to the monocentric model, one could interpret the estimates structurally.

4. *MSA fixed effects are poor proxies for unobserved influences because of the long time span.*

I intended the fixed-effects analysis primarily as a robustness check on the long-difference analysis. Identification of the treatment effect of interest using the long difference estimator relies solely on exogeneity of the 1947 highway plan, conditional on appropriate control variables. The fact that the fixed-effects analysis delivers the same results as the long-difference estimates simply says that the number of rays in the 1947 plan is not correlated with any fixed (unobserved) MSA attributes that might have generated suburbanization. That is, the fixed-effects results provide support for the claim that the 1947 plan is exogenous. As seen in Table IV in my paper (791), the 1947 plan also appears exogenous to the MSA income distribution and population, as I argue it should be.

5. *Data from European cities does not indicate a correlation between central city highway penetration and city population loss.*

Cox, Gordon, and Redfearn provide a table showing population growth rates and highway infrastructure in 42 European cities. The table contains some interesting nuggets of information that merit further investigation. Indeed it is striking how similar the population declines of European cities have been to American cities. However, I am not convinced that their cursory investigation suffices to conclude that there is no relationship between transport infrastructure and the spatial distribution of the population in Europe. I break my critique of the authors' analysis into five parts.

- a. The data reported in Table 1, which indicates whether central cities had ray penetration as of 1990, does not match current maps that I've found. Amsterdam, Bremen, Genoa and a few other cities on the list currently have limited access highways serving their downtowns when measured using the same definition as in my paper. This recent construction may have been anticipated in 1990 with some population movement to the suburbs.
- b. The authors' analysis of the European data makes no attempt to control for the spatial size of the central areas for which population numbers are reported. Spatial size is the one variable for which it is essential to control in regressions. As an example, if these European cities have very large areas, then we would expect to find small or zero effects of highways on the spatial distribution of the population.
- c. The analysis of the European data does not take into account when the highways were built. Cox, Gordon, and Redfearn calculate the population change between its peak year and 1990, but in 19 of 42 cities in their sample the peak occurred in 1970 or 1980, plausibly after some of the

freeway rays were built. A more convincing analysis would examine the relationship between the change in population over a fixed time period and the change in freeway availability over this same period.

- d. I agree that it is remarkable that every single European city in Table 1 lost population in the period prior to 1990, whether they received highways or not. However, the authors misinterpret the counterfactual calculations I make for U.S. cities. That calculation shows that given my results, holding all other influences of 1950-1990 population changes the same, point estimates indicate that some U.S. cities would have grown in population absent highway construction, leading to aggregate population growth. Taking the calculation out of sample by applying it to cities in Europe would be appropriate only if all the other influences were the same as those in the United States. Clearly, that is not the case. For example, aggregate European population grew only about half as fast as aggregate US population did between 1950 and 1990. If this were the only difference between the two continents (which of course it is not), and noting that European cities received fewer roads on average than American cities over the period, it is absolutely consistent with my results that European cities absent new highways lost population.
- e. The list of cities in Table 1 excludes those that expanded geographically. As such, one may worry that the sample is not random. I would be more confident in the authors' results if they used the full sample of large European cities.

6. *Large tracts of undeveloped land existed in US central cities in 1950.*

The existence of greenfields is one of many reasons that it would be a bad idea to take structural estimation of a land use model viewed in isolation too seriously. Regardless of the prevalence of greenfields, the fact remains that I estimate a causal relationship between highways and suburbanization for US cities. To the extent that the true treatment effect of highways is a function of open space in central areas, I agree that one should be cautious about the external validity of the results.

In conclusion, it is my hope that my *Quarterly Journal of Economics* article spurs further investigation of the mechanisms driving the observed relationship between transport infrastructure and the spatial distribution of the population. I applaud Cox, Gordon, and Redfearn for investigating the topic with European data.

## REFERENCES

- Baum-Snow, Nathaniel.** 2007a. Did Highways Cause Suburbanization? *Quarterly Journal of Economics* 122(2): 775-805.
- Baum-Snow, Nathaniel.** 2007b. Suburbanization and Transportation in the Monocentric Model. *Journal of Urban Economics* 62(3): 405-423.
- Cox, Wendell, Peter Gordon, and Christian Redfearn.** 2008. Highway Penetration of Central Cities: Not a Major Cause of Suburbanization. *Econ Journal Watch* 5(1): 32-45. [Link](#).

## ABOUT THE AUTHOR



Nathaniel Baum-Snow is the Stephen Robert Assistant Professor of Economics at Brown University. He received his Ph.D. in 2005 from the University of Chicago. Baum-Snow's research interests include suburbanization, urban transportation, segregation and the city size wage premium. His email is [Nathaniel\\_Baum-Snow@brown.edu](mailto:Nathaniel_Baum-Snow@brown.edu).

[Go to January 2008 Table of Contents with links to articles](#)

---

## Growth Accelerations and Regime Changes: A Correction

RICHARD JONG-A-PIN<sup>1</sup> AND JAKOB DE HAAN<sup>1,2</sup>

### ABSTRACT

**A COMMENT ON:** RICARDO HAUSMANN, LANT PRITCHETT, AND DANI RODRIK, “GROWTH ACCELERATIONS,” *JOURNAL OF ECONOMIC GROWTH* 10(4), 2005: 303-329.

**THERE IS MUCH RESEARCH ON THE IMPACT OF POLITICAL, LEGAL, AND ECONOMIC** institutions on long term economic growth. The usefulness of the growth regression framework is questionable, however, as it assumes that a single linear model is appropriate for all countries at all times (De Haan 2007). Very few countries have experienced consistently constant growth rates over time. Pritchett (2000) documents, for instance, that the variation in growth rates within countries is large relative to both the average growth rates and the variance across countries. Likewise, Jones and Olken (2005) report that no less than 48 countries have experienced one or more structural breaks in their economic development. These breaks lead to very distinct growth patterns. Whereas some countries have sustained long periods of growth, others have experienced rapid growth followed by stagnation or even crisis. Still others have faced continuous stagnation or steady decline. Empirical growth research has underestimated the importance of instability and volatility in growth rates, especially in developing countries.

One promising research strategy is to examine the economic, political, institutional, and policy conditions that accompany changes in growth patterns. A pioneering contribution in this field is by Hausmann, Pritchett and Rodrik (2005)—abbreviated here as HPR. They examine whether political regime changes and economic reforms precede growth accelerations. They identify more than 80

---

<sup>1</sup> Faculty of Economics and Business, University of Groningen, The Netherlands 9700 AV.

<sup>2</sup> CESifo, Munich, Germany.

We would like to thank Dani Rodrik for providing the data used in the analysis.

growth accelerations since the 1950s, which tend to be highly unpredictable. They find that a political regime change increases the probability of a growth acceleration by 5.3 percentage points while economic reforms are not related to growth accelerations. Their approach has attracted much attention and some recent papers have followed their approach (see, e.g., Doeven and Nunnenkamp 2007).

We argue that these conclusions of HPR are wrong as these authors were led astray by a data-description error in the Polity IV manual. When we correct for the error and stick to the Polity IV definition of regime change, we find that political regime changes are not related to the probability that a growth acceleration occurs. We also find some evidence that economic liberalization increases the probability of a growth acceleration (sustained or otherwise).

Our work can be seen as an illustration of the importance of replication as stressed by Hamermesh (2007). This paper contains a particular form of replication, namely redoing an analysis as published in a major journal using the data as used in that analysis to check whether the conclusions drawn are correct.

### OUR REPLICATION

For the period 1957-1992, HPR identify 83 periods of accelerated growth, using the following filter. For each country (with more than 1 million inhabitants and more than 20 available observations) the logarithm of real GDP per capita (taken from the Penn World Tables 6.1.) is regressed on time for every eight-year period ( $n=7$ ). That is,

$$\ln(y_{t+i}) = a + g_{t,t+n} * t + \varepsilon, \quad i = 0, \dots, n$$

Where  $y$  denotes real GDP capita and  $t$  is time. The estimated parameter,  $g_{t,t+n}$ , is taken as a proxy for the average growth rate over the period  $t$  to  $t+n$  and labeled the “least squares growth rate”. To qualify as a growth acceleration, the least squares growth rate must be at least 3.5% per annum. Furthermore, it must be at least 2 percentage points higher than in the previous eight years. Finally, to rule out episodes of full economic recovery, the level of real GDP must be higher at the end of the acceleration than in all years before the acceleration. In cases in which consecutive years qualify to be the start of a growth acceleration, the year is chosen with the highest F-statistic of a piecewise linear (or spline) regression with the break at the relevant year. HPR allow for the possibility that an acceleration is followed by another acceleration as long as the second acceleration starts at least five years after the first one.

We base our analysis on the definition and the identification of growth accelerations of HPR—though we feel the definition could be improved upon—and

focus on the explanatory variables used by these authors. These are categorized under three headings.

- (i) *External shocks.* Growth accelerations may be triggered by favorable external conditions, and HPR therefore include a terms-of-trade dummy, which takes the value 1 whenever the change in the terms of trade from year  $t-4$  to  $t$  is in the upper 10 percent of the entire sample.
- (ii) *Economic reform.* To quantify a change in economic policy, the authors rely primarily on an index provided by Wacziarg and Welch (2003), which incorporates a number of structural features (e.g., presence of marketing boards and socialist economic regimes) and the macroeconomic environment (e.g., presence of a large black-market premium for foreign currency), in addition to tariff and non-tariff barriers to trade. The variable used is a dummy that takes the value of 1 during the first five years of a transition towards “openness”.
- (iii) *Political regime changes* are proxied by a dummy that takes a value of 1 in the 5-year period beginning with a regime change as recorded in the Polity IV dataset (Marshall and Jaggers 2002), where a regime change is defined as a three-unit change in the Polity score (or as a regime interruption).

Professor Rodrik kindly provided the data used by HPR. We were able to reproduce their findings (results available on request). However, we discovered that HPR were led astray by the description in the Polity IV manual of the variable they used to construct their political regime change dummy.<sup>3</sup> As a consequence, in the dataset of HPR the political regime change dummy takes a value of 1 whenever there is a *one-unit change* (or more) in the Polity score.<sup>4</sup> This is not in line with the definition of a political regime change as outlined above. We have corrected this error and examine whether the results change.

---

3 HPR assume that a regime change occurs whenever the Polity IV data provides a non-missing value (including 0) for the variable REGTRANS. The error in the Polity manual is that on p. 26 it says: “Variables in Section 4 are coded only when there has been a change in regime authority characteristics that account for a 3-point change in the POLITY score or the assignment of a Standard Authority Code (“-66”, “-77”, or “-88”).” This is not correct as the variables in section 4 are coded for *any* change in the POLITY score or the assignment of a Standard Authority code. The quoted sentence should read so as to include the following words in bold type: “Variables in Section 4 are coded **and, in the case of REGTRANS, non-zero** only when...” In a telephone conversation with Daniel Klein, Montgomery Marshall, author of the Polity IV manual confirmed this understanding. The REGTRANS variable may be used to identify regime changes by excluding REGTRANS=0 observations. Marshall confirmed that it is erroneous to count REGTRANS=0 observations as regime changes.

4 A good example is Egypt 1976 that is coded 0 in the REGTRANS column *merely* because that year experienced a one-point change in the POLITY score. As a consequence, HPR erroneously treat this observation as a regime change. If HPR had checked their regime change variable with the Polity IV data series, they would have discovered that observations with REGTRANS=0 are not instances of regime change as they (HPR) defined them.

Table 1 shows the relationship between growth accelerations and regime changes (cf. Table 7 of HPR 2005, 320):

- Of 83 accelerations, we find that *21.7 percent*, are preceded or accompanied by a regime change, whereas HPR had *50 percent*.
- Of 130 regime changes, 13.8 percent, are followed by a growth acceleration, whereas HPR had 13.9 percent. And 4.6 percent, are followed by sustained accelerations, whereas HPR had 8.5 percent.

**Table 1: Regime Changes and Growth Accelerations**

	#	Acceleration and regime change in the same years (overlap)
Regime changes in the sample	130	
Growth accelerations	83	18
Sustained growth accelerations	32	6
% of accelerations accompanied or preceded by regime change	21.7%	
% of sustained accelerations accompanied or preceded by regime change	18.8%	
% of regime changes that result in acceleration	13.8%	
% of regime changes that result in sustained acceleration	4.6%	

Using the dataset as provided by Rodrik, we were able to fully reproduce Table 8 of HPR (322), in which they report on the relationship between the probability of a growth acceleration and a political regime change. As the first step in our subsequent analysis we corrected the coding mistake of political regime changes. Next, we checked the econometric specification of HPR. If we test for the restriction that all time dummies equal zero, it is not rejected for the model specifications as reported in columns (1)-(9). Therefore, we omit the time dummies for those specifications, but include them in columns 10 and 11. Table 2 reports our results if we redo the regressions in Table 8 of HPR using the corrected regime change variable and taking time dummies into account.<sup>5</sup> Our results diverge substantially from those of HPR. Whereas the latter report that regime changes have a highly significant impact on the probability of the occurrence of a growth acceleration, our evidence suggests that, in

<sup>5</sup> We use the Polity IV dataset in constructing our regime change dummy. The number of observations in our Table 2 differ from those in Table 8 of HPR as the dataset of HPR provide data for some countries for which the Polity IV does not provide data.

**Table 2: Predicting Growth Accelerations**  
**(Dependent Variable is a Dummy for the Timing of Growth Accelerations)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
TOT_thresh90	0.039 (2.15)**	0.040 (2.21)**	0.040 (2.18)**	0.037 (2.10)**	0.035 (2.20)**	0.032 (2.10)**	0.032 (2.11)**	0.033 (2.15)**	0.038 (2.15)**		
Econ Lib	0.039 (1.78)*	0.043 (1.93)*	0.040 (1.84)*	0.040 (1.83)*	-0.007 (0.39)	-0.014 (0.80)	-0.014 (0.79)	-0.014 (0.78)	0.040 (1.86)*	0.031 (1.55)	0.032 (1.59)
Xregchange	0.024 (1.66)*									0.011 (0.87)	
Xposchange		-0.001 (0.04)	-0.000 (0.02)	0.001 (0.03)	0.002 (0.10)	0.000 (0.01)	0.000 (0.02)	0.000 (0.03)	0.002 (0.11)		0.007 (0.32)
Xnegchange		0.028 (1.22)	0.027 (1.17)	0.028 (1.20)	0.023 (1.21)	0.021 (1.18)	0.021 (1.16)	0.023 (1.25)	0.027 (1.19)		0.009 (0.42)
Leader Death			-0.030 (1.20)	-0.062 (1.94)*	0.000 (0.00)	0.000 (0.01)	0.001 (0.01)	0.001 (0.02)	-0.062 (1.92)*		
Tenure				0.006 (1.98)**	-0.045 (3.06)**	-0.048 (3.00)**	-0.048 (3.00)**	-0.048 (3.00)**	0.006 (1.95)*		
War End							0.002 (0.16)	0.010 (0.59)	0.027 (1.28)		
Civil War								-0.019 (0.82)	-0.017 (0.64)		
Finance					0.035 (1.64)	0.114 (2.85)**	0.113 (2.85)**	0.116 (2.90)**			
Finance Dev						-0.044 (2.28)**	-0.044 (2.28)**	-0.044 (2.34)**			
Observations	2094	2094	2094	2094	1891	1891	1891	1891	2094	2739	2739
Pseudo R-squared	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.02	0.02	0.04	0.04
Time dummies equal 0, prob> Chi^2	0.7667	0.7670	0.7725	0.6986	0.5015	0.5648	0.5519	0.561	0.6861	0.0048	0.005
Time dummies included	No	No	No	No	No	No	No	No	No	Yes	Yes
Robust z statistics in parentheses * significant at 10%; ** significant at 5%; *** significant at 1%											

general, regime changes are hardly related to growth accelerations. For instance, HPR find a coefficient of regime instability of 0.044 and a t-statistic of 4.16. If we correct the error in the regime change data of HPR, we find instead, as reported in column 10 of Table 2, a coefficient of 0.011 and a t-statistic of 0.87. Likewise, whereas HPR find that the coefficients of positive and negative regime changes (*Xposchange* and *Xnegchange*, respectively) are generally highly significant, these variables are never significantly related to the probability of a growth acceleration according to our results. Furthermore, in our regressions the economic liberalization variable (*Econ Lib*) becomes significant at the 10 percent level, whereas HPR find that this variable is always insignificant.<sup>6</sup> The results for the other variables are similar to those of HPR.

We also examined the determinants of sustained and unsustained growth accelerations, while liberalization is significant. Table 3 reports the results when we estimate the models given in Table 12 of HPR. Again, we find that political regime changes are unrelated to growth accelerations. All other results are similar to the findings of HPR.

**Table 3. Predicting sustained and unsustained growth accelerations**  
(Dependent variable is a dummy for the timing of growth accelerations)

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Sustained	Sustained	Sustained	Unsustained
TOT_thresh90	0.067 (2.80)***	0.074 (3.25)***	0.019 (1.35)	0.016 (1.22)		0.023 (3.67)***
Econ Lib	0.090 (1.98)**	0.093 (2.08)**		0.140 (3.94)***	0.091 (3.50)***	
Xposchange	-0.012 (0.33)	-0.008 (0.23)	0.010 (0.44)	0.015 (0.66)	0.013 (0.65)	-0.002 (0.40)
Xnegchange	0.046 (1.76)*	0.050 (1.92)*	0.017 (1.04)	0.019 (1.18)	0.011 (0.63)	0.005 (1.20)
Finance	-0.006 (0.17)					0.994 (8.15)***
Observations	1211	1300	1300	1300	1723	1140
Pseudo R-squared	0.02	0.03	0.01	0.05	0.12	0.11
Time dummies equal 0, prob > Chi <sup>2</sup>	0.2254	0.3175	0.9793	0.9794	0.0001	0.0000
Time dummies included	No	No	No	No	Yes	Yes
Robust z statistics in parentheses * significant at 10%; ** significant at 5%; *** significant at 1%						

6 If we estimate the models with time dummies, we find that the political regime change dummy (properly defined) remains insignificant, and that economic liberalization does not significantly explain the occurrence of an acceleration.

## CONCLUSIONS

Economists treat replication the way teenagers treat chastity—as an ideal to be professed but not to be practiced (Hamermesh 2007, 1).

HPR's finding that a political regime change increases the probability of an economic growth acceleration is wrong and the result of a data error. When we correct for this error and stick to the definition of political regime change as a three-unit change in Polity, we find that regime changes do not affect the probability that a growth acceleration occurs. We also find some evidence that economic liberalization increases the probability of a growth acceleration (sustained or otherwise).

The paper by HPR appeared as a National Bureau of Economic Research working paper in 2004 (#10566) and as an article in the *Journal of Economic Growth* in 2005. As of October 2007, the paper had received 22 citations, as recorded in Thomson-ISI's Social Science Citation Index. We have not examined those 22 articles, but it is quite possible that some or many of them have cited HPR as support for the idea that a political regime change is correlated with an economic growth acceleration.<sup>7</sup>

The work represented here was submitted, of course, to the *Journal of Economic Growth*, although in that version of the paper we had not yet pinpointed the data-description error in the Polity IV manual. The paper was rejected on the basis of the argument that our note is a “welcome correction, however, of limited significance for the main contribution of the original paper.” However, in their abstract, HPR state that one of their main conclusions is that “Political regime changes are statistically significant predictors of growth accelerations.”

Coelho, de Worken-Eley III, and McClure (2005) document the decline in critical commentary at top economics journals. Our experience indicates that editors are not even willing to publish corrections of serious errors.

## REFERENCES

- Coelho, Philip R.P., Fred de Worken-Eley III, and James E. McClure.** 2005. Decline in Critical Commentary, 1963-2004. *Econ Journal Watch* 2(2): 355-61. [Link](#).
- Dovern, Jonas, and Peter Nunnenkamp.** 2007. Aid and Growth Accelerations: An Alternative Approach to Assessing the Effectiveness of Aid. *Kyklos* 60 (3): 359-83.
- De Haan, Jakob.** 2007. Political Institutions and Economic Growth Reconsidered. *Public Choice* 131 (3/4): 281-292.
- Jones, Benjamin F., and Benjamin A. Olken.** 2005. The Anatomy of Start-Stop

<sup>7</sup> Dovern and Nunnenkamp (2007) also use Polity IV data and also find a significant impact of regime changes on the likelihood that a growth acceleration occurs. As they do not very precisely describe their regime change variables, it is not clear whether their work suffers from the same error that HPR's does.

Growth. *NBER Working Paper* No. 11528. National Bureau of Economic Research, Cambridge, MA.

**Hamermesh, Daniel S.** 2007. Replication in Economics. *NBER Working Paper* No. 13026. National Bureau of Economic Research, Cambridge, MA.

**Hausmann, Ricardo, Lant Pritchett, and Dani Rodrik.** 2005. Growth Accelerations. *Journal of Economic Growth* 10(4): 303-329.

**Marshall, Monty G., and Keith Jagers** 2002. Polity IV Dataset. [Link](#).

**Pritchett, Lant.** 2000. Understanding Patterns of Economic Growth: Searching for Hills among Plateaus, Mountains, and Plains. *World Bank Economic Review* 14(2): 221-150.

**Wacziarg, Romain, and Karen Horn Welch.** 2003. Trade Liberalization and Growth: New Evidence. Mimeo. Stanford University.

#### ABOUT THE AUTHORS



**Richard Jong-A-Pin** obtained his MSc. in Economics in 2003 from the University of Groningen, The Netherlands, where he is currently a PhD student. His research focus lies in political economy—especially the causes and consequences of political instability. His work has been or will be published in the *European Economic Review*, *Economics Letters*, *Public Choice*, and other journals. His email address is [r.m.jong.a.pin@rug.nl](mailto:r.m.jong.a.pin@rug.nl).



**Jakob de Haan** is Professor of Political Economy, University of Groningen, The Netherlands. He is also director of SOM, the research institute and graduate school of the faculty of Economics and Business of the University of Groningen. He is also editor of the *European Journal of Political Economy*. His email address is [Jakob.de.Haan@rug.nl](mailto:Jakob.de.Haan@rug.nl).

[Go to January 2008 Table of Contents with links to articles](#)

---

## The EITC Disincentive: A Reply to Dr. Hilary Hoynes

PAUL TRAMPE<sup>1</sup>

### ABSTRACT

**A REJOINDER TO:** HILARY HOYNES, “THE EITC DISINCENTIVE: A REPLY TO PAUL TRAMPE,” *ECON JOURNAL WATCH* 4(3), SEPTEMBER 2007: 321-325. [LINK](#).

IN THE SEPTEMBER ISSUE DR. HILARY HOYNES REPLIES TO MY PAPER “The EITC Disincentive: The Effect on Hours Worked of the Phase-out of the Earned Income Tax Credit.” She criticizes my remarks about a figure in Eissa and Hoynes (2005), my downplaying the income effect, and my not including a control variable in my own investigation.

### THE INCOME EFFECT

Basic labor supply theory shows that an increase in income will lead to a reduction in labor force participation and hours work. This is known as the income effect. Theory also shows that a compensated increase in wages leads to an increase in labor force participation and hours worked. This is known as the wage or substitution effect. ... Trampe, by ignoring the income effect, incorrectly concludes that the EITC is work-promoting in the phase-in region. In the flat region, the EITC produces a negative income effect leading to an unambiguous reduction in hours worked. (Hoynes 2007, 321-322).

About a third of the way into my paper I note, “Of course it is also possible to observe what appears to be a disincentive throughout the income scale due to

---

<sup>1</sup> Ph.D. candidate, School of Public Policy, George Mason University. Arlington, VA 22201.

the simple fact that the subjective marginal benefit of each additional dollar of income declines as income rises” (311), Professor Hoynes is correct that I otherwise focus on the substitution effects. I figured it was reasonable to suppose that for families living in poverty the income effect of \$4500 is not significant. If I’m wrong about the magnitude of the income effect in the plateau region, only a study specific to that income range could demonstrate that, not a study of the entire EITC population or of the entire population of single women with children regardless of income. What Dr. Hoynes’ point argues for is separate studies of each income region of the EITC but she seems to be using the income effect point to defend papers which do just the opposite, lump every income level together.

[S]ome taxpayers with incomes beyond the phase-out region may choose to reduce their hours of work and take advantage of the credit. (Hoynes 2007, 321-322)

I find it extremely unlikely that anyone with income just beyond the phase-out range would give up wages in order to receive the insignificant EITC benefit. In fact a study by John Scholz (Scholz 1996) suggests that many people near the end of the phase-out range do not even bother applying for the credit because the amount is not worth the paperwork. Furthermore, if someone did take some leisure time to get into the phase-out range, he or she would be in the population I am studying. Hoynes seems to be using the point to justify studies which include families with children above the EITC income range. Even if there were individuals who acted as she suggests I do not see how that justifies basing conclusions of the effects of EITC on those who did not act that way and remain above the EITC income range.

If the income effect for those whose incomes place them in the phase-in or plateau regions or for those whose benefit is mostly phased out is as minor as I think it is, using cases from income ranges other than the phase-out range grows the sample greatly but adds only a small number of the additional cases, if any at all, involving individuals who reduced their hours on account of the EITC, thus watering down the more substantial effects in the phase-out region to the point that statistically significant results are impossible. It may be argued that the goal of the papers I criticize was to find the effect of the EITC as a whole, whereas I am only looking for the effect of the phase-out rates. However, it would seem to me that the best way to measure the effects of the program as a whole is to study each income range separately. The income effect point would seem to support the expectation that EITC reduces hours worked, yet if researchers expand the population studied based on those theories, the result is that the effect is drowned in a sea of statistical noise.

## COMMENTS ON EARLIER STUDIES

Further, Meyer and Rosenbaum's NBER working paper version of their 2001 *QJE* paper (Meyer and Rosenbaum 1999) extends their method to examine impacts of the EITC on hours worked. This should be recognized. (Hoynes 2007, 322)

I wrote (312): "Bruce Meyer and Dan Rosenbaum (2001) found mixed results on hours worked for women with children—also using the entire income spectrum (Meyer and Rosenbaum 2001)." Further, I find it odd that Dr. Hoynes criticizes me for not discussing an earlier unpublished version of a paper I did discuss.

Obviously, descriptive trends are not conclusive as to the impact of individual policies because there is much else changing over time. The second paragraph in the section "Previous studies: Labor Force participation" makes this mistake. (Hoynes 2007, 323)

I am commenting on papers which used graphs of descriptive trends as evidence of the impact of a particular policy. I'm suggesting the evidence in said graphs are not what the authors claim (or not limited to what they claim) but the basis of the analytical tool was their choice, not mine. I am quite aware that such trends are not conclusive as to the impact of individual policies and mentioned that such analysis is quite subjective.

First, Trampe states that "... they do not comment on the dramatic increase in hours worked by single women without children starting in 1984 which was not accompanied by a similar increase for those with children." The EITC did not expand until the Tax Reform Act of 1986 so any change by single women without children prior to 1986 is not relevant. (Hoynes 2007, 323)

I merely noted that the divergence starting in 1984 should have been mentioned. The chart is presented as evidence that there is no significant difference in trends in hours worked between the EITC population and the non-EITC population. On the other hand, the divergence in the lines proceeds from the very beginning point on the chart, 1984. We do not know if the trends measure back before that, to the beginning of EITC in 1976. Or did the divergence in trends begin in 1983? Dr. Hoynes chastises me for not controlling for macroeconomic trends (see below) yet ignores that it is at least possible that the disincentive only has an effect during periods of economic growth such as the one which began in

1983 when there is more opportunity to increase one's hours.

There was no policy change after 1993 so any fluctuation between 1997 and 2000 should not have anything to do with the program! (Hoynes 2007, 323)

The 1993 policy changes to which she refers were legislated in 1993 but implemented in stages from 1994-1996. The fluctuations began shortly after the policy change was fully implemented. It is reasonable to assume that it may take years for people to learn and adapt to policy changes, particularly complex policy changes.

#### **RESPONSE TO CRITICISMS OF THE METHOD OF MY OWN STATISTICAL INVESTIGATION**

The fundamental problem with this approach is that it ignores the selected nature of the sample. As EITC expands, labor force participation increases which can lead to changes in the composition of the sample of those in the phase-out range. For example, what if women who enter the labor force work fewer hours than women already in the labor force? The hours will decrease with the expansion of the EITC yet (in this simple example) there was no reduction in hours worked! This is a very old problem in empirical labor supply and there are many approaches that are used to solve this basic endogeneity problem. (Hoynes 2007, 323)

I dealt with this problem by choosing a post-policy year ten years removed from the policy change (2006). The labor force expansion had long since taken place and there was no reason to believe that the percentage of those in the phase-out range who were new to the workforce in 2006 was higher than in my pre-policy year of 1993 (which itself was seven years removed from a smaller EITC expansion). The effect on hours, on the other hand was ongoing, as the phase-out rates have not changed since 1996. I used demographic control variables to account for other differences in the composition of the phase-out sample.

The problem is that there is no control for year fixed effects in the model. Therefore, if there are any other factors that vary by year (labor market effects, other trends, other policies) the estimates will be biased unless there are perfect controls for these features (and in point of fact, there are NO controls of this sort in the model). This is a fundamental problem with the empirical model and in

fact is the main reason that people use control groups; ideally they are selected such that they face the same environment except for not facing the policy change. (Hoynes 2007, 324)

Controlling for macroeconomic differences between the years was not possible if my model was to answer the question I was trying to answer. As Dr. Hoynes points out, controlling for such events when the value of the policy variable is determined primarily by year without a control group is pointless, as the value of the control variable will be the same for each case from a particular year. However introducing the control group would have upset the model in other ways.

When Eissa and Liebman (1996) conducted a similar study of the EITC expansion following the 1986 tax reform, they controlled for the national unemployment rate by using a control group of single women without children (and therefore not eligible for EITC) in the same income range as the rest of the sample. The policy change they were studying was one in which the maximum benefit of EITC was increased but the phase-out rate was actually reduced, creating a sizable income range which was outside the EITC before the policy change but in the phase-out range afterwards. In other words the entire sample from the pre-policy year was outside the program, without an EITC induced marginal tax rate. They were testing the effect of an EITC-induced marginal tax rate changing from 0% to 10-11% (depending on family size). Therefore adding a test group also outside of the program with a 0% marginal tax rate from EITC did nothing to change the basic composition of the population.

In my case, however, I was seeking to test the effects of moving from a 10-11% phase-out rate to 16-21% today. There was no one who remained at the 10-11% rate after 1993 so the only way to include a control group was to include cases of those outside the program. Unfortunately, adding a significant number of cases with an EITC phase-out rate of zero would have muddled the picture and left it impossible to isolate the effects of moving from 10-11% to 16-21%. As I mentioned in my conclusions, one possibility raised by Eissa-Liebman (1996) is that the effects of marginal tax rates are not linear. There may be little to no effect moving from a 0% rate to 10-11% but as the rates go higher there may come a point where the rates are high enough to trigger a response in enough cases to make a measurable difference in hours worked. Therefore it was impossible to isolate the effects of any macroeconomic control and isolate the effects of the 1993 expansion of the program at the same time, and I chose one over the other.

It is unfortunate that I could not isolate the macroeconomic effects because the effects, if any, would likely have served to advance the case that there is some discouragement of work. There are, of course, many such variables but the unemployment rate, which Eissa and Liebman used, has the most obvious

tie to the dependent variable of hours worked. In my pre-policy change year of 1993, when the phase-out rates were the lowest, the unemployment rate was 6.9%. In the year I used from the gradual implementation of the policy, 1994, the unemployment rate was 6.1%. In my post-policy change year, 2006, when the phase-out rates were at their highest, the unemployment rate was 4.6%. This means of course that the general labor market was working against the effects of the phase-out rate. If lower unemployment nationally is at all related to increasing hours for those in the EITC phase-out range (and Eissa and Liebman found that it is), then if I had been able to isolate such effects it could only have magnified the effects of the phase-out rates I found. Dr. Hoynes criticizes me for not using a control variable which, if there were any effects, could only have strengthened my finding.

The determinants of labor supply of married couples differ from singles and this should be reflected in the empirical model. (Hoynes 2007, 324)

There is certainly nothing wrong with separating the sample by marital status in order to measure how the effects differ between the two groups, but that was not the question I was seeking to answer.

Finally, why limit the analysis to a random sample of 200 households in the phase out region? The CPS has much larger samples than this and there is no reason to do this with modern computing opportunities. The larger samples will also allow for stratifying results by marital status. (Hoynes 2007, 324)

I reported the results of an empirical investigation undertaken for a project in my graduate studies. It is the only sample I have done. Redoing or enhancing the sample would have created indeterminacy and ambiguity about which data to include. Six hundred cases (200 each in 1993, 1994 and 2006) are more than adequate for statistically significant results.

### CONCLUDING REMARK

I am grateful to Dr. Hoynes for replying to my Comment. My understanding of the empirics of the EITC is being enriched by the exchange. I hope others reading the exchange feel likewise.

## REFERENCES

- Eissa, Nada, and Liebman, Jeffrey.** 1996. Labor Supply Response to the Earned Income Tax Credit. *Quarterly Journal of Economics* 111(2): 605-637.
- Hoynes, Hilary.** 2007. The EITC Disincentive: A Reply to Paul Trampe. *Econ Journal Watch* 4(3): 321-325. [Link](#).
- Scholz, John Karl.** 1996. In-Work Benefits in the United States: The Earned Income Tax Credit. *The Economic Journal* 106 (434): 159-169.
- Trampe, Paul.** 2007. The EITC Disincentive: The Effect on Hours Worked from the Phase-out of the Earned Income Tax Credit. *Econ Journal Watch* 4(3): 308-320. [Link](#).

## ABOUT THE AUTHOR



**Paul Trampe** has worked for the federal government for 19 years, as an economist in the executive branch and an economic policy advisor in the legislative branch. He holds an MA in history from George Mason University and is currently enrolled in the Ph.D. program at George Mason's School of Public Policy. His email is [ptrampe@gmu.edu](mailto:ptrampe@gmu.edu).

[Go to January 2008 Table of Contents with links to articles](#)

## CLASSIC ECONOMIC CRITICISM

---

### Gulphs in Mankind's Career of Prosperity: A Critique of Adam Smith on Interest Rate Restrictions

JEREMY BENTHAM

#### ABSTRACT

#### PREFACE TO THE BENTHAM EXTRACT ON USURY, BY DAN KLEIN

In “Adam Smith and Laissez Faire,” Jacob Viner (1927) concluded: “There is no possible room for doubt, however, that Smith in general believed that there was, to say the least, a strong presumption against government activity beyond its fundamental duties of protection against its foreign foes and maintenance of justice” (219). Smith developed that presumption, however, amidst a medley exemptions and ambiguities. One of the most famous exceptions is Smith’s endorsement of a maximum rate of interest, as was the status quo in his society.

Jeremy Bentham wrote a series of thirteen “Letters” addressed to Smith, published in 1787 as *Defence of Usury* ([link](#)). Here we reproduce a small part of the work. The present extract is less than 10 percent of the entire work, and comes mainly from Letter XIII. I invented the title given above, made a few minor changes in punctuation, and made small alterations to the *Wealth of Nations* quotations so as to conform to the modern standard edition, to which we have inserted page citations.

Bentham’s main argument against the restriction is that “projectors” generate positive externalities. The extract offers economic argumentation involving social embeddedness, asymmetric interpretation, imagination, error and correction, discovery, local knowledge, experimentation and selection, learning by doing, human folly and delusion, critical discussion as a means of testing commercial interpretations and selecting judgments, display of genius and courage as motivation for commercial success, the distinction between voluntary and coercive action, and the moral and cultural merits of liberty.

In a nice essay “From Usury to Interest,” which summarizes Smith and Bentham on usury, Joseph Persky (2007) writes: “Gilbert K. Chesterton (933) for

one, identified Bentham's essay on usury as the very beginning of the 'modern world.' I tend to agree with him" (228).

Bentham's arguments were very influential. "Writers of eminence" moved to abolish the restriction, and repeal was achieved in stages and fully achieved in England in 1854 (Dana 1867, 46).

**Adam Smith's response to Bentham:** There is little evidence as to Smith's reaction. He did not revise the offending passages in *The Wealth of Nations*, but Smith made little or no substantial revisions after the third edition of 1784. The only trail of a reaction is as follows: Smith speaks to W. Adam, who speaks to G. Wilson, who writes to Bentham:

Did we ever tell you what Dr Adam Smith said to Mr William Adam, the Council M.P., last summer in Scotland. The Doctor's expressions were that 'the *Defence of Usury* was the work of a very superior man, and that tho' he had given [Smith] some hard knocks, it was done in so handsome a way that he could not complain,' and seemed to admit that you were right. (George Wilson to Jeremy Bentham, December 4, 1789, quoted in Rae 1895, 423-24)

In the 1790 edition of *Defence of Usury*, Bentham added a preface again addressed to Smith and referred to the report from Wilson: "I have been flattered by the assurance that upon the whole your sentiments with respect to the points of difference are at present the same as mine: but as the information did not come directly from you, nor has the communication of it received the sanction of your authority, I shall not without that sanction give any hint, honorable as it would be to me, and great as the service is which it could not but render to my cause" (quoted in Mossner and I.S. Ross 1977, 402).

It seems that shortly before his death in 1790, Smith made a gift to Bentham of one or both of his own major works (Viner 1965, 19). On the assumption that Smith sent *The Theory of Moral Sentiments*, which seems probable (Persky says he did), Maria Pia Paganelli (2003, 46) speculates that Smith believed in what he had written on usury and sent the *Moral Sentiments* to provide Bentham the larger explanation, having to do with moderation and the maintenance of a moral order. I fancy a somewhat different view, namely, one that sees Smith as being somewhat more libertarian than he let on. I fancy that Smith really favored the liberty maxim 93 percent of the time, if you will, but made it sound like between 83 and 89 percent and fudged quite a lot, because he had achieved a position of cultural royalty within his society, and an air and voice of royalty more generally, and he did not want to upset that position and voice by attacking status-quo Scotland too much. He would let us think that his Scotland gets things mostly right, whether in policy or cultural leadership. If Smith sent the *Moral Sentiments* to Bentham, maybe he did it to remind him of the larger cultural project he was leading—"Well done,

Bentham, but people will less take to the liberty maxim if I give them 95 percent straight up.” Further, and closer to Paganelli, I’d speculate that Smith was telling Bentham that we do not want to unbridle ambition and proud genius, because of the frightful hazards of unleashing them in the governmental realm.

### BENTHAM’S *DEFENCE OF USURY*

ON<sup>1</sup> THIS OCCASION, WERE IT ANY INDIVIDUAL ANTAGONIST I HAD TO DEAL with, my part would be a smooth and easy one. “You, who fetter contracts; you, who lay restraints on the liberty of man, it is for you” (I should say) “to assign a reason for your doing so.” That contracts in general ought to be observed, is a rule, the propriety of which, no man was ever yet found wrong-headed enough to deny: if this case is one of the exceptions (for some doubtless there are) which the safety and welfare of every society require should be taken out of that general rule, in this case, as in all those others, it lies upon him, who alledges the necessity of the exception, to produce a reason for it.

[...] Should<sup>2</sup> it be my fortune to gain any advantage over you, it must be with weapons which you have taught me to wield, and with which you yourself have furnished me: for, as all the great standards of truth, which can be appealed to in this line, owe, as far as I can understand, their establishment to you, I can see scarce any other way of convicting you of any error or oversight, than by judging you out of your own mouth.

[...] [I]f I presume to contend with you, it is only in defence of what I look upon as, not only an innocent, but a most meritorious race of men, who are so unfortunate as to have fallen under the rod of your displeasure. I mean *projectors*: under which invidious name I understand you to comprehend, in particular, all such persons as, in the pursuit of wealth, strike out into any new channel, and more especially into any channel of invention.

It is with the professed view of checking, or rather of crushing, these adventurous spirits, whom you rank with “prodigals”, that you approve of the laws which limit the rate of interest, grounding yourself on the tendency, they appear to you to have, to keep the capital of the country out of two such different sets of hands.

The passage, I am speaking of, is in the fourth chapter of your second book, volume the second of the 8vo. edition of 1784. “The legal rate” (you say [p. 357 of the Glasgow/Oxford University Press/Liberty Fund edition of *WN*]) “it is to be observed, though it ought to be somewhat above, ought not to be much above the lowest market rate. If the legal rate of interest in Great Britain, for example, was fixed so high as eight or ten per cent., the greater part of the money which

---

1 [This is paragraph 5 of Letter I.]

2 [This is Letter XIII.]

was to be lent, would be lent to prodigals and projectors, who alone would be willing to give this high interest. Sober people, who will give for the use of money no more than a part of what they are likely to make by the use of it, would not venture into the competition. A great part of the capital of the country would thus be kept out of the hands which were most likely to make a profitable and advantageous use of it, and thrown into those which were most likely to waste and destroy it. Where the legal interest, on the contrary, is fixed but a very little above the lowest market rate, sober people are universally preferred, as borrowers, to prodigals and projectors. The person who lends money gets nearly as much interest from the former as he dares to take from the latter, and his money is much safer in the hands of the one set of people, than in those of the other. A great part of the capital of the country is thus thrown into the hands in which it is most likely to be employed with advantage.”

[...] Antecedently<sup>3</sup> to custom growing from convention, there can be no such thing as usury: for what rate of interest is there that can naturally be more proper than another? what natural fixed price can there be for the use of money more than for the use of any other thing? Were it not then for custom, usury, considered in a moral view, would not then so much as admit of a definition: so far from having existence, it would not so much as be conceivable: nor therefore could the law, in the definition it took upon itself to give of such offence, have so much as a guide to steer by. Custom therefore is the sole basis, which, either the moralist in his rules and precepts, or the legislator in his injunctions, can have to build upon. But what basis can be more weak or unwarrantable, as a ground for coercive measures, than custom resulting from free choice? My neighbours, being at liberty, have happened to concur among themselves in dealing at a certain rate of interest. I, who have money to lend, and Titius, who wants to borrow it of me, would be glad, the one of us to accept, the other to give, an interest somewhat higher than theirs: why is the liberty they exercise to be made a pretence for depriving me and Titius of ours?

[...] [W]hat<sup>4</sup> your definition is of projectors, and what descriptions of persons you meant to include under the censure conveyed by that name, might be material for the purpose of judging of the propriety of that censure, but makes no difference in judging of the propriety of the law, which that censure is employed to justify. Whether you yourself, were the several classes of persons made to pass before you in review, would be disposed to pick out this or that class, or this and that individual, in order to exempt them from such censure, is what for that purpose we have no need to enquire. The law, it is certain, makes no such distinctions: it falls with equal weight, and with all its weight, upon all those persons, without distinction to whom the term *projectors*, in the most impartial and

3 [Here inserted is paragraph 4 of Letter II.]

4 [This resumes Letter XIII. All remaining text reproduced here is from Letter XIII, in order but with omissions, which are always indicated.]

extensive signification of which it is capable, can be applied. It falls at any rate (to repeat some of the words of my former definition), upon all such persons, as, in the pursuit of wealth, or even of any other object, endeavour, by the assistance of wealth, to strike into any channel of invention. It falls upon all such persons, as, in the cultivation of any of those arts which have been by way of eminence termed *useful*, direct their endeavours to any of those departments in which their utility shines most conspicuous and indubitable; upon all such persons as, in the line of any of their pursuits, aim at any thing that can be called *improvement*; whether it consist in the production of any new article adapted to man's use, or in the meliorating the quality, or diminishing the expence, of any of those which are already known to us. It falls, in short, upon every application of the human powers, in which ingenuity stands in need of wealth for its assistant.

High and extraordinary rates of interest, how little soever adapted to the situation of the prodigal, are certainly, as you very justly observe, particularly adapted to the situation of the projector: not however to that of the imprudent projector only, nor even to his case more than another's, but to that of the prudent and well grounded projector, if the existence of such a being were to be supposed. Whatever be the prudence or other qualities of the project, in whatever circumstance the novelty of it may lie, it has this circumstance against it, viz. that it is new. But the rates of interest, the highest rates allowed, are, as you expressly say they are, and as you would have them to be, adjusted to the situation which the sort of trader is in, whose trade runs in the old channels, and to the best security which such channels can afford. But in the nature of things, no new trade, no trade carried on in any new channel, can afford a security equal to that which may be afforded by a trade carried on in any of the old ones: in whatever light the matter might appear to perfect intelligence, in the eye of every prudent person, exerting the best powers of judging which the fallible condition of the human faculties affords, the novelty of any commercial adventure will oppose a chance of ill success, superadded to every one which could attend the same, or any other, adventure, already tried, and proved to be profitable by experience.

The limitation of the profit that is to be made, by lending money to persons embarked in trade, will render the monied man more anxious, you may say, about the goodness of his security, and accordingly more anxious to satisfy himself respecting the prudence of a project in the carrying on of which the money is to be employed, than he would be otherwise: and in this way it may be thought that these laws *have* a tendency to pick out the good projects from the bad, and favour the former at the expence of the latter. The first of these positions I admit: but I can never admit the consequence to follow. A prudent man, (I mean nothing more than a man of ordinary prudence) a prudent man acting under the sole governance of prudential motives, I still say will not, in these circumstances, pick out the good projects from the bad, for he will not meddle with projects at all. He will pick out old-established trades from all sorts of projects, good and bad; for with a

new project, be it ever so promising, he never will have any thing to do. By every man that has money, five per cent. or whatever be the highest legal rate, is at all times, and always will be, to be had upon the very best security, that the best and most prosperous old-established trade can afford. Traders in general, I believe, it is commonly understood, are well enough inclined to enlarge their capital, as far as all the money they can borrow at the highest legal rate, while that rate is so low as 5 per cent., will enlarge it. How it is possible therefore for a project, be it ever so promising, to afford, to a lender at any such rate of interest, terms equally advantageous, upon the whole, with those he might be sure of obtaining from an old-established business, is more than I can conceive.

[...] [U]nless the stock of well-grounded projects is already spent, and the whole stock of ill-grounded projects that ever were possible, are to be looked for exclusively in the time to come, the censure you have passed on projectors, measuring still the extent of it by that of the operation of the laws in the defence of which it is employed, looks as far backward as forward: it condemns as rash and ill-grounded, all those projects: by which our species have been successively advanced from that state in which acorns were their food, and raw hides their clothing, to the state in which it stands at present: for think, Sir, let me beg of you, whether whatever is now the *routine* of trade was not, at its commencement, *project*? whether whatever is now *establishment*, was not, at one time, innovation?

How it is that the tribe of well-grounded projects, and of prudent projectors (if by this time I may have your leave for applying this epithet to some at least among the projectors of time past), have managed to struggle through the obstacles which the laws in question have been holding in their way, it is neither easy to know, nor necessary to enquire. Manifest enough, I think, it must be by this time, that difficulties, and those not inconsiderable ones, those laws must have been holding up, in the way of projects of all sorts, of improvement (if I may say so) in every line, so long as they have had existence: reasonable therefore it must be to conclude, that, had it not been for these discouragements, projects of all sorts, well-grounded and successful ones, as well as others, would have been more numerous than they have been: and that accordingly, on the other hand, as soon, if ever, as these discouragements shall be removed, projects of all sorts, and among the rest, well-grounded and successful ones, will be more numerous than they would otherwise have been: in short, that, as, without these discouragements, the progress of mankind in the career of prosperity, would have been greater than it has been under them in time past, so, were they to be removed, it would be at least proportionably greater in time future.

That I have done you no injustice, in assigning to your idea of projectors so great a latitude, and that the unfavourable opinion you have professed to entertain of them is not confined to the above passage, might be made, I think, pretty apparent, if it be material, by another passage in the tenth chapter of your first book. “The establishment of any new manufacture, of any new branch of commerce, or

of any new practice in agriculture,” all these you comprehend by name under the list of “*projects*”: of every one of them you observe, that “[it] is always a speculation, from which the *projector* promises himself extraordinary profits. These profits (you add) sometimes are *very great*, and sometimes, *more frequently, perhaps*, they are *quite otherwise*; but in general they bear no regular proportion to those of other old trades in the neighbourhood. If the project succeeds, they are commonly at first very high. When the trade or practice becomes thoroughly established and well known, the competition reduces them to the level of other trades” [131-32]. But on this head I forbear to insist: nor should I have taken this liberty of giving you back your own words, but in the hope of seeing some alteration made in them in your next edition, should I be fortunate enough to find my sentiments confirmed by your’s. In other respects, what is essential to the publick, is, what the error is in the sentiments entertained, not who it is that entertains them.

I know not whether the observations which I have been troubling you with, will be thought to need, or whether they will be thought to receive, any additional support from those comfortable positions, of which you have made such good and such frequent use, concerning the constant tendency of mankind to get forward in the career of prosperity, the prevalence of prudence over imprudence, in the sum of private conduct at least, and the superior fitness of individuals for managing their own pecuniary concerns, of which they know the particulars and the circumstances, in comparison of the legislator, who can have no such knowledge. I will make the experiment: for, so long as I have the mortification to see you on the opposite side, I can never think the ground I have taken strong enough, while any thing remains that appears capable of rendering it still stronger.

“With regard to misconduct, the number of prudent and successful undertakings” (you observe) “is every where much greater than that of injudicious and unsuccessful ones. After all our complaints of the frequency of bankruptcies, the unhappy men who fall into this misfortune make but a very small part of the whole number engaged in trade, and all other sorts of business; not much more perhaps than one in a thousand” [342].

[...] Of the two causes, and only two causes, which you mention, as contributing to retard the accumulation of national wealth, as far as the conduct of individuals is concerned, projecting, as I observed before, is the one, and prodigality is the other: but the detriment, which society can receive even from the concurrent efficacy of both these causes, you represent, on several occasions, as inconsiderable; and, if I do not misapprehend you, too inconsiderable, either to need, or to warrant, the interposition of government to oppose it. Be this as it may with regard to projecting and prodigality taken together, with regard to prodigality at least, I am certain I do not misapprehend you. On this subject you ride triumphant, and chastise the “impertinence and presumption of kings and ministers,” with a tone of authority, which it required a courage like yours to venture upon, and a genius like yours to warrant a man to assume. After drawing the paral-

lel between private thrift and public profusion, “It is” (you conclude) “the highest impertinence and presumption, therefore, in kings and ministers, *to pretend to watch over the oeconomy of private people*, and to restrain their expence either by sumptuary laws, or by prohibiting the importation of foreign luxuries. They are themselves always, and without exception, the greatest spendthrifts in the society. Let them look well after their own expence, and they may safely trust private people with theirs. If their own extravagance does not ruin the state, that of their subjects never will” [346].

[...] [T]o *err* in the way of projecting is the lot only of the privileged few. Prodigality, though not so common as to make any very material drain from the general mass of wealth, is however too common to be regarded as a mark of distinction or as a singularity. But the stepping aside from any of the beaten paths of traffic, is regarded as a singularity, as serving to distinguish a man from other men. Even where it requires no genius, no peculiarity of talent, as where it consists in nothing more than the finding out a new market to buy or sell in, it requires however at least a degree of courage, which is not to be found in the common herd of men. What shall we say of it, where, in addition to the vulgar quality of courage, it requires the rare endowment of genius, as in the instance of all those successive enterprizes by which arts and manufactures have been brought from their original nothing to their present splendor?

[...] If it be still a question, whether it be worth while for government, by its *reason*, to attempt to control the conduct of men visibly and undeniably under the dominion of *passion*, and acting, under that dominion, contrary to the dictates of their own reason; in short, to effect what is acknowledged to be their better judgment, against what every body, even themselves, would acknowledge to be their worse; is it endurable that the legislator should by violence substitute his own pretended reason, the result of a momentary and scornful glance, the offspring of wantonness and arrogance, much rather than of social anxiety and study, in the place of the humble reason of individuals, binding itself down with all its force to that very object which he pretends to have in view?—Nor let it be forgotten, that, on the side of the individual in this strange competition, there is the most perfect and minute knowledge and information, which interest, the whole interest of a man’s reputation and fortune, can ensure: on the side of the legislator, the most perfect ignorance. All that he knows, all that he can know, is, that the enterprize is a *project*, which, merely because it is susceptible of that obnoxious name, he looks upon as a sort of cock, for him, in childish wantonness, to shie at.—Shall the blind lead the blind? is a question that has been put of old to indicate the height of folly: but what then shall we say of him who, being necessarily blind, insists on leading, in paths he never trod in, those who can see?

It must be by some distinction too fine for my conception, if you clear yourself from the having taken, on another occasion, but on the very point in question, the side, on which it would be my ambition to see you fix.

“What is the species of domestic industry which his capital can employ, and of which the produce is likely to be of the greatest value, every individual” (you say), “it is evident, can, in his local situation, judge much better than any statesman or lawgiver can do for him. The statesman, who should attempt to direct private people in what manner they ought to employ their capitals, would not only load himself with a most unnecessary attention, but assume an authority which could safely be trusted, not only to no single person, but to no council or senate whatever, and which would no where be so dangerous as in the hands of a man who had folly and presumption enough to fancy himself fit to exercise it.”

“To give the monopoly of the home-market to the produce of domestick industry, in any particular art or manufacture, is in some measure to direct private people in what manner they ought to employ their capitals, and must, in almost all cases, be either a useless or a hurtful regulation” [456]—Thus far you: and I add, to limit the legal interest to a rate at which the carriers on of the oldest and best established and least hazardous trades are always glad to borrow, is to give the monopoly of the money-market to those traders, as against the projectors of new-imagined trades, not one of which but, were it only from the circumstance of its novelty, must, as I have already observed, appear more hazardous than the old.

These, in comparison, are but inconclusive topics. I touched upon them merely as affording, what appeared to me the only shadow of a plea, that could be brought, in defence of the policy I am contending against. I come back therefore to my first ground, and beg you once more to consider, whether, of all that host of manufactures, which we both exult in as the causes and ingredients of national prosperity, there be a single one, that could have existed at first but in the shape of a project. But, if a regulation, the tendency and effect of which is merely to check projects, in as far as they are projects, without any sort of tendency, as I have shewn, to weed out the bad ones, is defensible in its present state of imperfect efficacy, it should not only have been defensible, but much more worthy of our approbation, could the efficacy of it have been so far strengthened and compleated as to have opposed, from the beginning, an unsurmountable bar to all sorts of projects whatsoever: that is to say, if, stretching forth its hand over the first rudiments of society, it had confined us, from the beginning, to mud for our habitations, to skins for our cloathing, and to acorns for our food.

I hope you may by this time be disposed to allow me, that we have not been ill served by the projects of time past. I have already intimated, that I could not see any reason why we should apprehend our being worse served by the projects of time future. I will now venture to add, that I think I do see reason, why we should expect to be still better and better served by these projects, than by those. I mean better upon the whole, in virtue of the reduction which experience, if experience be worth any thing, should make in the proportion of the number of the ill-grounded and unsuccessful, to that of the well-grounded and successful ones.

The career of art, the great road which receives the footsteps of projectors, may be considered as a vast, and perhaps unbounded, plain, bestrewed with gulphs, such as Curtius was swallowed up in. [*Ed. note:* Marcus Curtius was a Roman hero. When one day a gap suddenly appeared on the Forum in Rome, an oracle said that it could only be closed by the most precious thing Rome possessed. The wellbeing of the town depended on it. Curtius sacrificed himself by jumping fully armed and mounted on the finest horse into the gap, which then closed itself.] Each requires an human victim to fall into it ere it can close, but when it once closes, it closes to open no more, and so much of the path is safe to those who follow. If the want of perfect information of former miscarriages renders the reality of human life less happy than this picture, still the similitude must be acknowledged...

[...] But to return to the laws against usury, and their restraining influence on projectors. I have made it, I hope, pretty apparent, that these restraints have no power or tendency to pick out bad projects from the good. Is it worth while to add, which I think I may do with some truth, that the tendency of them is rather to pick the good out from the bad? Thus much at least may be said, and it comes to the same thing, that there is one case in which, be the project what it may, they may have the effect of checking it, and another in which they can have no such effect, and that the first has for its accompaniment, and that a necessary one, a circumstance which has a strong tendency to separate and discard every project of the injudicious stamp, but which is wanting in the other case. I mean, in a word, the *benefit of discussion*.

It is evident enough, that upon all such projects, whatever be their nature, as find funds sufficient to carry them on, in the hands of him whose invention gave them birth, these laws are perfectly, and if by this time you will allow me to say so, very happily, without power. But for these there has not necessarily been any other judge, prior to experience, than the inventor's own partial affection. It is not only not necessary that they should have had, but it is natural enough that they should not have had, any such judge: since in most cases the advantage to be expected from the project depends upon the exclusive property in it, and consequently upon the concealment of the principle. Think, on the other hand, how different is the lot of that enterprize which depends upon the good opinion of another man, that other, a man possessed of the wealth which the projector wants, and before whom necessity forces him to appear in the character of a suppliant at least: happy if, in the imagination of his judge, he adds not to that degrading character, that of a visionary enthusiast or an impostor! At any rate, there are, in this case, two wits, set to sift into the merits of the project, for one, which was employed upon that same task in the other case: and of these two there is one, whose prejudices are certainly not most likely to be on the favourable side. True it is, that in the jumble of occurrences, an over-sanguine projector may stumble upon a patron as over-sanguine as himself; and the wishes may bribe the judgment

of the one, as they did of the other. The opposite case, however, you will allow, I think, to be by much the more natural. Whatever a man's wishes may be for the success of an enterprize not yet his own, his fears are likely to be still stronger. That same pretty generally implanted principle of vanity and self-conceit, which disposes most of us to over-value each of us his own conceptions, disposes us, in a proportionable degree, to undervalue those of other men.

Is it worth adding, though it be undeniably true, that could it even be proved, by ever so uncontrovertible evidence, that, from the beginning of time to the present day, there never was a project that did not terminate in the ruin of its author, not even from such a fact as this could the legislator derive any sufficient warrant, so much as for wishing to see the spirit of projects in any degree repressed? The discouraging motto, *Sic vos non vobis* [Thus do ye, but not for yourselves], may be matter of serious consideration to the individual, but what is it to the legislator? What general, let him attack with ever so superior an army, but knows that hundreds, or perhaps thousands, must perish at the first onset? Shall he, for that consideration alone, lie inactive in his lines? "Every man for himself—but God," adds the proverb (and it might have added the general, and the legislator, and all other public servants), "for us all." Those sacrifices of individual to general welfare, which, on so many occasions, are made by third persons against men's wills, shall the parties themselves be restrained from making, when they do it of their own choice? To tie men neck and heels, and throw them into the gulphs I have been speaking of, is altogether out of the question: but if at every gulph a Curtius stands mounted and caparisoned, ready to take the leap, is it for the legislator, in a fit of old-womanish tenderness, to pull him away? laying even public interest out of the question, and considering nothing but the feelings of the individuals immediately concerned, a legislator would scarcely do so, who knew the value of hope, "the most precious gift of heaven."

Consider, Sir, that it is not with the invention-lottery ..., as with the mine-lottery, the privateering-lottery, and so many other lotteries, which you speak of, and in no instance, I think, very much to their advantage. In these lines, success does not, as in this, arise out of the embers of ill success, and thence propagate itself, by a happy contagion, perhaps to all eternity. Let Titius have found a mine, it is not the more easy, but by so much the less easy, for Sempronius to find one too: let Titius have made a capture, it is not the more easy, but by so much the less easy, for Sempronius to do the like. But let Titius have found out a new dye, more brilliant or more durable than those in use, let him have invented a new and more convenient machine, or a new and more profitable mode of husbandry, a thousand dyers, ten thousand mechanics, a hundred thousand husbandmen, may repeat and multiply his success: and then, what is it to the public, though the fortune of Titius, or of his usurer, should have sunk under the experiment?

[...]

### WORKS REFERENCED IN THE PREFACE

- Chesterton, Gilbert K.** 1933. *St. Thomas Aquinas*. New York: Sheed & Ward, Inc.
- Dana, Richard H., Jr.** [1867] 1881. Speech in the House of Representatives of Massachusetts, on the Repeal of Usury. In *Usury Laws: Their Nature, Expedience, and Influence*, 37-58. New York: Society for Political Education. [Link](#).
- Mossner, E.C., and I.S. Ross**, eds. 1977. *The Correspondence of Adam Smith*. Oxford: Oxford University Press.
- Paganelli, Maria Pia.** 2003. In Medio Stat Virtus: An Alternative View of Usury in Adam Smith's Thinking. *History of Political Economy* 35(1): 21-48.
- Persky, Joseph.** 2007. Retrospectives: From Usury to Interest. *Journal of Economic Perspectives* 21(1): 227-236.
- Rae, John.** 1895. *Life of Adam Smith*. London: Macmillan and Co. [Link](#).
- Viner, Jacob.** 1927. Adam Smith and Laissez Faire. *Journal of Political Economy* 35(2): 198-232.
- Viner, Jacob.** 1965. *Guide to John Rae's Life of Adam Smith*. New York: Kelley.

### ABOUT THE AUTHOR



*The Internet Encyclopedia of Philosophy* (accessed December 2007, [link](#)) writes: Jeremy Bentham (1748-1832) “was an English philosopher and political radical. Although he never practiced law, he spent most of his life critiquing the existing law and strongly advocating legal reform. Bentham is primarily known today for his moral philosophy, especially his principle of utilitarianism which evaluates actions based upon their consequences, in particular the overall happiness created for everyone affected by the action. He maintained that putting this principle into consistent practice would provide justification for social, political, and legal institutions. Although Bentham’s influence was minor during his life, his impact was greater in later years as his ideas were carried on by followers such as John Stuart Mill, John Austin, and other consequentialists.”

[Go to January 2008 Table of Contents with links to articles](#)

## ECONOMICS IN PRACTICE



# The Market for Lemmas: Evidence that Complex Models Rarely Operate in Our World

PHILIP R.P. COELHO AND JAMES E. MCCLURE<sup>1</sup>

### ABSTRACT

Many major economic journals publish models that can neither generate operational statements nor be challenged by evidence. Authors sometimes motivate these enterprises by allusions to “stylized facts.” Often, it is only in concluding remarks that authors provide vague directions about how “future research” might allow their results to operate in the realm of evidence. Coelho and McClure (2005, 562-564) present evidence that in the *American Economic Review* 1963 through 1996 “[m]athematically complex articles were less operational and were less likely to be cited in articles containing operational statements.” Empirical research suggests that the probability of subsequent articles appearing with refuting data, or any data, is substantially lower than in less mathematically complex articles.<sup>2</sup> Another reason to doubt that mathematically complex models can generate operational research is that their assumptions are often complicated, substantively obscure, and unworldly.<sup>3</sup>

Economics uses evidence to assess theories. Theories that do not provide evidentiary or testable propositions at reasonable costs are usually disregarded. The ability to formalize refutable statements and find evidence for or against them is *operationalism*. Statements that cannot be operationalized are what Wolfgang Pauli called “not even wrong.” Refutations instruct us on what is wrong; non-operational statements lack this virtue.<sup>4</sup>

1 Miller College of Business, Ball State University. Muncie, Indiana 47306.

2 Klein and Romero (2007) develop several necessary conditions for when a “model” qualifies as a “theory”; examining the main articles in the 2004 volume of the *Journal of Economic Theory* they provide evidence for the “paucity of theory” in *JET*.

3 See Daniel M Hausman (1989, 120-121) on “unrealistic” assumptions, and on “tractability” see Frank Hindriks (2005).

4 The phrase *not even wrong* is being used in the debate over string theory in physics. Peter Woit has au-

Operationalism is not the only way to assess theories. Several authors (such as Coase 1982, 26f; Gibbard and Varian 1978, 669; Hausman 1992; Sugden 2000, 131) allow for a richer and more complex framework, wherein theories and evidence are mutually formulated, and encompassing formulations then judged by broad sensibilities. Voices critical of the emphasis on statistical operationalism often point out that the aspiration of statistical testing is not always attainable, and some conjectures are too important to neglect for that reason. Other forms of empirical argument must sometimes be sought (see Coase 1975, 58).

Here we focus on operationalism, the “stronger” empirical standard, for several reasons. First, operationalism is primarily what economists have in mind when they speak of “empirical work.” Second, weaker standards of evidence are more difficult to replicate and assess. Non-statistical approaches are generally more impressionistic, the details involved in replication typically leave room for great ambiguity. In our view, the profession’s emphasis on statistical operationalism as the marker of “empirical work” may be overdone, but not inordinately. Rather, our complaint is that empirical argumentation all too frequently is neither undertaken nor considered. Further, the ideas represented by complex models typically lack any apparent significance that might legitimately exempt them from operationalist demands.

Deploying evidence is a judgmental endeavor that depends upon such things as the particular application of the theory, the costs of establishing background conditions, and data acquisition. After establishing the conditions, judgments about “size” and “fit” can be considered;<sup>5</sup> this is close to the concerns of D. N. McCloskey’s (1983).

Lemmas are formulated in proofs so complex that it is useful to divide the task into intermediate steps (*Lemma 1, Lemma 2 . . . et cetera*), like a stopover. We use the presence of the term “lemma(s)” as an indicator of mathematical complexity.<sup>6</sup> The term “lemma(s)” has become increasingly frequent in the journal literature.

---

thored a book with the title *Not Even Wrong*. He contends that the resources that academic physics has allocated to string theory have been excessive, because the theory accommodates a range of outcomes so enormous that it is immune from refutation.

5 We have a relatively Popperian (1934) methodological perspective; on Popperian methodology in general, and on the relationship between falsifiability and testability in economic in particular, see Boland (1989).

6 Grubel and Boland (1986, 421) focus on the “quantity” of mathematics, bypassing the issue of the “mix of types” of mathematics used in economics journals. In contrast, our point turns decisively on the type of logical chain in which mathematics is used. However, because lemma usage so often appears in economic analytics, our conclusions are in the same spirit as those of Grubel and Boland.

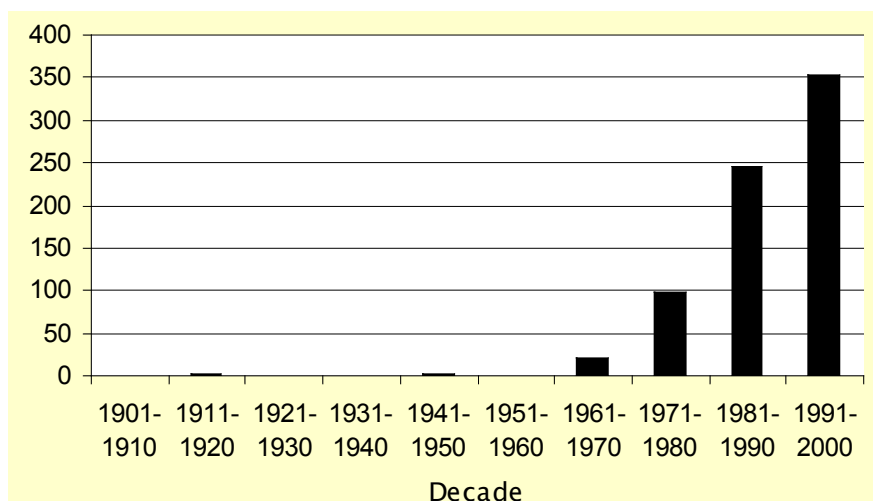
**Figure 1: Lemma Frequency by Decade (*AER*, *EJ*, *JPE*, *QJE*)**

Figure 1 (above) presents evidence on the trend in the usage of lemmas in some top journals in economics. The vertical axis of Figure 1 represents the numbers of articles found per decade in a full-text search of the JSTOR data base that contain either the term *lemma* or *lemmas* in the *American Economic Review*, *Economic Journal*, *Journal of Political Economy* and *Quarterly Journal of Economics*.<sup>7</sup> The appearance of the word “lemma(s)” was rare in the first six decades of the twentieth century, but during the last four decades it became increasingly frequent.<sup>8</sup>

### RADIOACTIVE DECAY IN LONG CHAINS

Alfred Marshall addressed “long trains of deductive reasoning”:

It is obvious that there is no room in economics for *long trains of*

<sup>7</sup> The year 2000 was chosen as the last year of consideration because at the time of our investigation JSTOR did not provide data for all of the four journals considered beyond that year. The first appearance of lemma(s) in any of these journals was in Edgeworth’s (1910) article in *EJ*. The numeric results shown in Figure 1 are: 1900-1910 (*one article*); 1911-1920 (*two articles*); 1921-1930 (*zero*); 1931-1940 (*zero*); 1941-1950 (*two*); 1951-1960 (*one*); 1961-1970 (22); 1971-1980 (98); 1981-1990 (245); 1991-2000 (353). Data for 2001 became available for all four journals subsequent to the creation of this figure. In the year from 2000 to 2001 the JSTOR count of articles in the *AER*, *EJ*, *JPE*, and *QJE* containing the term “lemma” or “lemmas” was 83. The accelerating expansion of the “market for lemmas” in the final four decades of the 20<sup>th</sup> century continued in the first year of the 21<sup>st</sup>.

<sup>8</sup> More inclusive measures of “mathematical complexity” could be presented, but looking at lemma(s) is good enough to illustrate the trend towards publication of articles of increasing mathematical complexity. For a more formal investigation with a more inclusive measure, see Coelho and McClure (2005, 560-561). In the future if the use of the term *lemma* is stigmatized, and authors of lengthy proofs avoid the stigma by calling intermediate steps in lengthy proofs something else (e.g., step 1, step 2, ...), then the value of this proxy for mathematical complexity would be reduced. Given the trends in the usage of the term *lemma* that we document, there is no evidence that its usage is being curtailed currently.

*deductive reasoning*; no economist, not even Ricardo, attempted them. It may indeed appear at first sight that the contrary is suggested by the frequent use of mathematical formulae in economic studies. But on investigation it will be found that this suggestion is illusory, except perhaps when a pure mathematician uses economic hypotheses for the purpose of mathematical diversions; for then his concern is to show the potentialities of mathematical methods on the supposition that material appropriate to their use had been supplied by economic study. He takes no technical responsibility for the material, and is often unaware how inadequate the material is to bear the strains of his powerful machinery. But a training in mathematics is helpful by giving command over a marvelously terse and exact language for expressing clearly some general relations and some short processes of economic reasoning; which can indeed be expressed in ordinary language, but not with equal sharpness of outline. And, what is of far greater importance, experience in handling physical problems by mathematical methods gives a grasp, that cannot be obtained equally well in any other way, of the mutual interaction of economic changes. (Marshall 1920, 644, emphasis added)

Paul Samuelson notes that both Alfred Marshall and John Stuart Mill spoke “of the dangers involved in *long* chains of logical reasoning;” and he explains that:

Marshall treated such chains as if their truth content was subject to *radioactive decay* and leakage—at the end of  $n$  propositions only half the truth was left, at the end of a chain of  $2n$  propositions, only half of half the truth remained, and so forth in a geometric multiplier series converging to zero truth. (Samuelson 1952, 57, emphasis added)

Subsequently, Donald F. Gordon (1955, 160) said: “It is frustrating but nevertheless true that, where mathematics is most likely to be useful, the theory is least likely to be valid, while, where the theory is most likely to be true, complex deduction is generally not needed.” Using an example of a theory relating three distinct variables  $x$ ,  $y$ , and  $z$ , Gordon reasoned: “Again, the relationship between  $x$  and  $y$  may be stable long enough for a shift along that function but not stable long enough for a shift along that function plus a subsequent shift along another  $[z]$ ” (155).

Problems arise if *ceteris paribus* breaks down. As the length of a mathematical chain in an *economic theory* increased, Gordon suggested, it would become increasingly likely that the passage of time would in unpredictable ways compromise

the assumed stability of the chain. The timelessness implicit in multiple mathematical linkages was seen by Gordon as an obstacle to operationalizing complex mathematical theories about economic phenomena.<sup>9</sup>

The contrast between economic analytics (or “pure theory”) and statistical/econometric analytics<sup>10</sup> is informative; in statistical analytics (such as in the development or refinement of a statistical test) assumptions are *not* affected by the passage of time. A century from now, the calculation of a Chi-squared statistic will require the same mathematical steps that are used today. In contrast, the subjects of economic theories are affected by the passage of time; a century from now the income elasticity of the demand for gasoline will have changed.

### EVIDENCE ON MODELS NEVER BEING OPERATIONALIZED

Table 1 provides evidence bearing upon the proposition that mathematical complexity in economic analytics tends not to be operationalized. It lists all articles in the 1980 volumes of the *Journal of Economic Theory* that contained 5 or more lemmas. The columns list how many lemmas each article had, how many citations each article had up to the June 2006, how many of the citing articles had empirical data, how many of the citing articles empirically tested a proposition of the original article, and how many citing articles refuted a proposition of the originating article.<sup>11</sup>

The 12 articles with five or more lemma generated 237 citations to them in the following (approximately) quarter century. Nine of the 237 citing articles contained empirical data, two had empirical data that had something to do with the propositions of the original article, and none had a definitive test leading to an acceptance or rejection of a proposition of the original article.<sup>12</sup> In short, the

9 Wassily Leontief (1971, 1-2) echoed Gordon’s concerns about the timelessness implicit in mathematics: “Uncritical enthusiasm for mathematical formulation tends often to conceal the ephemeral substantive content of the argument behind the formidable front of algebraic signs.”

10 We use the term “economic analytics” to include both models and theories. We understand the distinction between “models” and “theories” and that not all models are theories, and, in conjunction, what we term “statistical/econometric analytics” would usually be called “statistical/econometric theory.” Again, the term “theory” may not always be appropriate because much work is often solely analytic refinements and explorations, as opposed to an explanation.

11 The *ISI Web of Science* was used to identify citations. Our search of this database occurred during the first two weeks of June of 2006. After citations were identified, each citing article was individually inspected to see whether it: (a) contained empirics, (b) attempted a direct assessment of any of the authors’ theoretical propositions, and (c) contained empirical assessments that accepted or rejected any proposition of the authors. Citing articles containing only casual empiricism were not counted as containing empirics, nor did the presence of simulations qualify them as containing empirics. However, citing articles containing data from surveys and/or experiments did qualify them as containing empirics.

12 For the originating (1980) articles listed in Table 1, the average number of lemmas per article is (79/12) or 6.58. For comparison purposes we counted the numbers of articles in *JET* in 2005 having 5 or more lemmas (there were 21 such article), and we counted the numbers of lemmas in these articles (there were 165 lemmas). In the 2005 set of articles the lemmas per article was 7.86. Comparing *JET*

**Table 1: Characteristics of Articles Containing 5 or More Lemmas in the *Journal of Economic Theory* in 1980**

Author(s)	Number of Lemmas in Article	Number of Cites to the Author(s)			
		Total	Containing Empirics	Attempting Direct Empirical Assessment	Empirical Assessments that Accept or Reject
Kalai & Ritz	6	9	0	0	0
Cohen	6	0	0	0	0
Green	5	41	3	1	0
Makowski	8	17	0	0	0
Dubey	6	15	1	0	0
Gaines	7	2	0	0	0
Krass	5	2	1	1	0
Rubenstein	6	13	0	0	0
Flaherty	6	21	2	0	0
Kleinberg	5	6	0	0	0
Balasko & Shell	13	103	2	0	0
Littlechild & Owen	6	8	0	0	0
TOTALS	79	237	9	2	0

Note: The bibliographic information for these articles can be found after the references list at the end of this article.

12 originating articles have to date defined no operational propositions.<sup>13</sup>

Determining whether an article's propositions have been operational-

in 1980 to 2005, there has been an increase in the number of articles using 5+ lemmas and the mean number of lemmas in these articles increased.

<sup>13</sup> An obvious criticism of these empirics is that we may just be showing that the *Journal of Economic Theory* does not publish papers with an empirical relevance. It may be that papers in *JET* that have no lemmas also have no empirical content. This is a criticism that we have addressed elsewhere (Coelho and McClure 2005); there we provide evidence that suggests that greater mathematical complexity is associated with less operationalism. More generally, Boland (1989; especially chapters 2, 3, and 8) has shown that testing in economics, even of relatively simple mathematical models, can require intractably large quantities of data. Similarly, Eric D. Beinhocker (2006, 63) calculates that for a modern economy producing the myriad of goods currently available, and if calculations were made at the

ized requires a fair amount of labor. The five-lemma threshold is the only sample we investigated. Twelve articles are not a large sample, but the results regarding those 12 are suggestive.

### THE MOST CITED TOP-JOURNAL ARTICLES RARELY CONTAIN LEMMAS

Kim, Morse, and Zingales (2006) have compiled a comprehensive list of articles published between 1970 and 2002 in 41 prominent journals in economics (and econometrics) that generated at least 500 citations to them as of June 2006. We accept this list as a proxy for what economists regard as best practice. We recognize that citation is an imperfect proxy for “best practice” or “what mattered most.” Citation counts are open to numerous criticisms (Klein and Chiang 2004, 137-39 summarize the concerns). Still, citation counts are widely considered the “gold standard” in assessing the impact of an article. Despite our misgivings we use the list developed by Kim, Morse, and Zingales to examine the impact of mathematization on article quality.

Using their data we took all the articles that were published in the four top general-interest journals *AER*, *EJ*, *JPE*, and *QJE* that had 500 or more citations, and examined each to count the number of lemmas that appear in the articles. Table 2 summarizes the findings:

**Table 2: Lemma Usage in Most Widely Cited Articles  
in Top General-Interest Economics Journals**

Journal	Total Number of articles*	Number of articles that created at least one lemma
<i>AER</i>	18	0
<i>EJ</i>	4	0
<i>JPE</i>	26	0
<i>QJE</i>	11	1
TOTALS	59	1

\*Data extracted from Table 2 of: Kim, Morse, and Zingales (2006, 15)

Of the fifty-nine articles in *AER*, *EJ*, *JPE*, and *QJE* that have been cited more than 500 times, only one article contained an author-written lemmas.<sup>14</sup> We

---

speed of “70.2 trillion floating-point calculations per second ... then ... it would take a mere 4.5 quintillion years ( $4.5 \times 10^{18}$ ) for the economy to reach general equilibrium after each exogenous shock.”

14 Cho and Kreps (1987) used two lemmas in their publication in the *QJE*.

recognize that it takes decades to accumulate 500 citations and the lemma trend only really started in the 1970s. Still, the results suggest that mathematical complexity has almost never been professionally rewarded with super-high citations and publication in the top general-interest journals.

### LEMMA USAGE IN THE MOST CITED: ECONOMIC VS. STATISTICAL/ECONOMETRIC ANALYTICS

Here we compare lemma usage in two types of articles: economic versus statistical/econometric analytics. We make this comparison because the Gordon hypothesis argues that there is an inverse correlation between mathematical complexity and operationalism in economic analytics. Coelho & McClure (2005, 562-564) provide empirical evidence supporting the Gordon hypothesis. In contrast, there is no reason to expect that the Gordon hypothesis applies to statistical/econometric analytics. We hypothesize that the citation “payoff” to complex mathematics in economic analytics will be smaller than the payoff in statistical/econometric analytics.

Again using the list of articles with 500+ citations, we added four more journals: two top model-building journals, *Review of Economic Studies* and *Journal of Economic Theory*, and two top statistical/econometric journals, *Journal of the American Statistical Association* and *Econometrica*. We examined all the articles on the Kim, Morse, Zingales list from the following 8 journals: *AER*, *Econometrica*, *EJ*, *JASA*, *JET*, *JPE*, *QJE*, and *ReStud*. This produced a list of 108 articles in all. Each article was examined individually to determine whether it: (1) it contained at least one author-written lemma; and (2) was devoted to economic analytics or to statistical/econometric analytics.

The scorings of the 108 are shown in the Excel file linked from Appendix 1, at the end of this paper, and results are displayed in the Table 3. Of the total 108 articles considered, 21 percent had at least one author-created lemma. Contrasting articles concerned with economic versus statistical/econometric analytics, we find that the percentage containing at least one lemma is 11 percent for the former versus 52 percent for the latter.

The frequencies are consistent with our hypothesis: The citation “payoff” to mathematical complexity in economic articles is smaller than in statistical/econometric articles. To assess whether the difference in lemma usage among most widely cited articles is statistically significant depending upon article purpose, a Chi-squared test was conducted using the data in Table 3. The Chi-squared statistic is 20.1. This leads to a rejection of the null hypothesis (that the frequency of lemma usage in the most-cited articles in statistical/econometric analytics is the same as the lemma frequency in articles in economics) at the 1% level.

**Table 3: Contingency Table: Most Widely Cited Articles in Alternative Types of Articles by Lemma Usage**

Article Purpose	Articles with No Lemmas	Articles with at Least One Lemma	TOTAL	Percentage with at Least One Lemma
Economic Analytics	72	9	81	11%
Statistical/ Econometric Analytics	13	14	27	52%
<b>TOTAL</b>	<b>85</b>	<b>23</b>	<b>108</b>	<b>21%</b>

Again, the results of Table 3 are not meant as a direct test of the Gordon hypothesis, instead these results provide insights into the social returns to the usage of mathematical complexity in economic versus statistical/econometric analytics. Among the sample, the apparent return to complexity is significantly lower in economics, both statistically and quantitatively. From the perspective of operationalism, this makes intuitive sense: widely cited statistical/econometric analytics generally supply directly or contribute indirectly to econometric tests and techniques for the manipulation of data. These articles are widely cited because what they supply is useful for examining data in articles that are operationalizing theories.

### CONCLUDING REMARKS

This paper is *not* a general criticism of the usage of mathematics in economics; it is instead about the displacement of operationalism as the core pursuit of economics by the pursuit of mathematical elegance and generality.<sup>15</sup> Pauli's indictment "not even wrong" says "even" because non-operational models are *worse than wrong* whenever they draw resources away from the creation and examination of operational propositions or fail to provide any information, insights, or hypotheses about observational reality.

Alfred Marshall (1920, 1) stated that economics is: "a study of mankind in the ordinary business of life." This is in contrast to the mathematical ideal of gen-

15 In their 1986 analysis of the efficient quantity of mathematics in economics, Grubel and Boland argued: "Our study has one clear-cut conclusion: The editors of economics journals should reduce the space devoted to mathematically oriented material" (439).

erality, elegance, and “pure” theory unblemished by the pursuit of worldly considerations. If we are dealing with the “ordinary business of life” we are unlikely to encounter either absolute “Truth” or the elegance that is sought by purists. In the Marshallian tradition the best we can hope for are conditional statements that are dependent upon time and a host of other circumstances; here the use of mathematics will be tempered by measurements, operationalism, experience, history, and all the nuances that are relevant to the purposes at hand.

During the last century, economists have discussed the implications of mathematical complexity in economic theory. In 1920, Alfred Marshall stated that it was “obvious” that there was “no room in economics for long trains of deductive reasoning.” What was obvious to Marshall was not obvious to the economics profession writ large. In the mid-twentieth century the increasing mathematical complexity of economics led Donald Gordon (1955, 161) to speculate that concerns for operationalism in economics implied that “the practice of proliferating and manipulating functions has gone to somewhat incautious limits.”

The evidence here indicates that mathematical complexity in economics has expanded exponentially beyond the levels that Gordon decried as “incautious.” Mathematical complexity has commanded more resources in economics, yet the additional complexity has generated little in the way of operational propositions.<sup>16</sup> Concerns for operationalism, measurement, empiricism, statistical testing, and history are the focus of an economics discipline that attempts to explain phenomena that exist in the world that real people inhabit.

## APPENDIX

Using the list of 500+ citations articles found in Kim, Morse, Zingales (2006; Table 2), we examined the 108 articles published in *American Economic Review*, *Econometrica*, *Economic Journal*, *Journal of the American Statistical Association*, *Journal of Economic Theory*, *Journal of Political Economy*, *Quarterly Journal of Economics*, and *Review of Economic Studies* to determine whether it: (1) it contained at least one author-written lemma; and (2) was devoted to economic analytics or to statistical/econometric analytics. The results are summarized in our Table 3 above. The Excel file linked here contains the details. [Link](#).

---

<sup>16</sup> In explaining *why* mathematical complexity spreads, Gordon Tullock (2005, 47) reasoned that it spreads in fields where opportunities for original research are limited relative to the number of people in the field. “One symptom of the existence of this condition is the development of very complex methods. Calculus will be used where simple arithmetic would do, and topology will be introduced in place of plane geometry. In many fields of social science these symptoms have appeared.”

## REFERENCES

- Beinhocker, Eric D.** 2006. *The Origin of Wealth*. Boston: Harvard Business School Press.
- Boland, Lawrence A.** 1989. *The Methodology of Economic Model Building*. London: Routledge.
- Cho, In-Koo, and David M. Kreps.** 1987. Signaling Games and Stable Equilibria. *Quarterly Journal of Economics* 102(2): 179-222.
- Coase, Ronald H.** [1975] 1994. Economists and Public Policy. In his *Essays on Economics and Economists*, 47-63. Chicago: University of Chicago Press.
- Coase, Ronald H.** [1982] 1994. How Should Economists Choose? In his *Essays on Economics and Economists*, 15-33. Chicago: University of Chicago Press.
- Coelho, Philip R.P., and James E. McClure.** 2005. Theory versus Application: Does Complexity Crowd Out Evidence? *Southern Economic Journal* 71(3): 556-565.
- Edgeworth, F. Y.** 1910. Application of Probabilities to Economics—II. *The Economic Journal* 20(79): 441-465.
- Gibbard, Allan, and Hal R. Varian.** 1978. Economic Models. *The Journal of Philosophy* 75: 664-677.
- Gordon, Donald F.** 1955. Operational Propositions in Economic Theory. *Journal of Political Economy* 63(2): 150-161.
- Grubel, Herbert G., and Lawrence A. Boland.** 1986. On the Efficient Use of Mathematics in Economics: Some Theory, Facts, and Results of an Opinion Survey. *Kyklos* 39(3): 419-442.
- Hausman, Daniel M.** 1989. Economic Methodology in a Nutshell. *Journal of Economic Perspectives* 3(2): 115-127.
- Hausman, Daniel M.** 1992. *The Inexact and Separate Science of Economics*. Cambridge: Cambridge University Press.
- Hindriks, Frank A.** 2005. Unobservability, Tractability and the Battle of Assumptions. *Journal of Economic Methodology* 12(3): 383-406.
- Kim, E. Han, Adair Morse, and Luigi Zingales.** 2006. What Has Mattered to Economics since 1970? *Journal of Economic Perspectives* 20(4): 189-202.
- Klein, Daniel B., with Eric Chiang.** 2004. The Social Science Citation Index: A Blackbox—with an Ideological Bias? *Econ Journal Watch* 1(1): 134-65. [Link](#).
- Klein, Daniel B., and Pedro P. Romero.** 2007. Model building versus Theorizing: The Paucity of Theory in the *Journal of Economic Theory*. *Econ Journal Watch* 4(2): 241-71. [Link](#).
- Leontief, Wassily.** 1971. Theoretical Assumptions and Nonobservable Facts. *American Economic Review*. 61(1): 1-7.

- Marshall, Alfred.** [1920] 1964. *Principles of Economics*. 8<sup>th</sup> Ed. London: MacMillian & Co.
- McCloskey, Deirdre N.** 1983. The Rhetoric of Economics. *Journal of Economic Literature* 21(2): 481-517.
- Popper, Karl.** [1934] 1959. *Logic of Scientific Discovery*. New York: Science Editions.
- Samuelson, Paul A.** 1952. Economic Theory and Mathematics—An Appraisal. *American Economic Review* 42(2). *Papers and Proceedings of the Sixty-fourth Annual Meeting of the American Economic Association*: 56-66.
- Sugden, Robert.** 2000. Credible Worlds: The Status of Theoretical Models in Economics. *Journal of Economic Methodology* 7(1): 1-31.
- Tullock, Gordon.** 2005. *The Organization of Inquiry*. Durham: Duke University Press.
- Woit, Peter.** 2006. *Not Even WRONG*. New York, NY: Best Books.

#### ARTICLES LISTED IN TABLE 1

- Balasko, Yves and Karl Shell.** 1980. The Overlapping-Generations Model, I: The Case of Pure Exchange without Money. *Journal of Economic Theory* 23(3): 281-306.
- Cohen, Susan.** 1980. Incentives, Iterative Communication, and Organizational Control. *Journal of Economic Theory* 22(1): 37-55.
- Dubey, Pradeep.** 1980. Nash Equilibria of Market Games: Finiteness and Inefficiency. *Journal of Economic Theory* 22(2): 363-76.
- Flaherty, M. Therese.** 1980. Dynamic Limit Pricing, Barriers to Entry, and Rational Firms. *Journal of Economic Theory* 23(2): 160-82.
- Gaines, Robert E.** 1980. Existence of Solutions to Hamiltonian Dynamical Systems of Economic Growth with Marxian Saving Hypothesis. *Journal of Economic Theory* 23(1): 1-14.
- Green, Edward J.** 1980. Noncooperative Price Taking in Large Dynamic Markets. *Journal of Economic Theory* 22(2): 155-82.
- Kalai, Elud, and Zvi Ritz.** 1980. Characterization of the Private Domains Admitting Arrow Social Welfare Functions. *Journal of Economic Theory* 22(1): 23-36.
- Kleinberg, Norman L.** 1980. Fair Allocations and Equal Incomes. *Journal of Economic Theory* 23(2): 189-200.
- Krass, Iosif.** 1980. Properties of Von Neumann-Gale Cooperation Models,” *Journal of Economic Theory* 23(1): 82-110.
- Littlechild, S.C., and G. Owen.** 1980. An Austrian Model of the Entrepreneurial Market Process. *Journal of Economic Theory* 23(3): 361-79.
- Makowski, Louis.** 1980. A Characterization of Perfectly Competitive Economies with Production. *Journal of Economic Theory* 22(2): 208-21.

**Rubenstein, Ariel.** 1980. Stability of Decision Systems Under Majority Rule. *Journal of Economic Theory* 23(2): 150-59.

#### ABOUT THE AUTHORS



**Philip R. P. Coelho** is a Professor of Economics in the Miller College of Business at Ball State University. He has published in the *American Economic Review*, *Journal of Economic History*, *Southern Economic Journal*, *Economic Inquiry*, and other journals. His current research interests are the economic effects of morbid diseases, the biological basis of economic behavior, and ethics and behavior in business. His email is 00prcoelho@bsu.edu.



**James E. McClure** (Ph.D., Purdue University, 1983) is a Professor of Economics in the Miller College of Business at Ball State University. His publications appear in journals such as *Economic Inquiry*, *Journal of Economic Behavior and Organization*, and *Southern Economic Journal*. His primary research focus is applied microeconomics. His email is jmcclure@bsu.edu.

[Go to January 2008 Table of Contents with links to articles](#)

## ECONOMICS IN PRACTICE: FOLLOW-UP

---

### “Theory” and “Models”: Terminology Through the Looking Glass

ROBERT S. GOLDFARB<sup>1</sup> AND JON RATNER<sup>2</sup>

#### ABSTRACT

**A COMMENT ON:** DANIEL B. KLEIN AND PEDRO P. ROMERO, “MODEL BUILDING VERSUS THEORIZING: THE PAUCITY OF THEORY IN THE *JOURNAL OF ECONOMIC THEORY*,” *ECON JOURNAL WATCH* 4(2), MAY 2007: 241-271. [LINK](#).

‘Model’ is a ubiquitous term in economics, and a term with a variety of meanings.

—Kevin Hoover 1995, 33.

\*\*\*

“When I use a word,” Humpty Dumpty said in rather a scornful tone, “it means what I choose it to mean, neither more nor less.”

“The question is,” said Alice, “whether you CAN make words mean so many different things.”

—Lewis Carroll 1871, 188

**WHAT DO CONTEMPORARY ECONOMISTS MEAN BY THE TERMS “THEORY” and “model”?** Of course, the meaning of those terms is known by every economist, just as Humpty Dumpty presumably knows precisely what he means. But is the term “theory” in fact a matter of broad consensus among economists? Ditto for the term “model.” The Hoover epigraph above suggests the answer may be “no.”

Klein and Romero’s recent (2007) interesting article in this journal sparked

---

<sup>1</sup> Department of Economics, George Washington University. Washington, D.C. 20052.

<sup>2</sup> Westat. Rockville, MD 20850.

We thank Marco Cipriani, Don Parsons, Herman Stekler and Lawrence White for comments on an earlier draft.

these questions for us, although the questions are peripheral to their main concerns. They evaluate articles in the *Journal of Economic Theory* for what one might call “intellectual usefulness.” They find that many *JET* articles earn very poor usefulness ratings. Regarding these ratings, we agree in general with both the method and the substance.

In reading their article, though, we noted that the Klein-Romero use of “theory” and “model” differed markedly from how we use those terms in a paper on a related topic.<sup>3</sup> In fact, their usage seems one-sided, omitting what we argue below is a widely-used meaning of the term “model.” This suggests that divergent usages by economist-authors may be widespread, confronting readers with considerable ambiguity. Our sense is that clarity and understanding can be enhanced by sorting out the competing uses in economics of “theory” and “model.” This note takes a step toward such a sorting-out.

Our thesis is that the terms “theory” and “model” are incapable of carrying the diverse characteristics different economists ascribe to them. As a result, descriptive clarity may require perhaps eight separate terms, but only two terms—“model” and “theory”—are currently available and used. A classificatory scheme we propose below should shed light on this possibility, at least for models.

### THE KLEIN-ROMERO DELINEATION OF “MODEL” AND “THEORY”

What do Klein-Romero mean by “theory” and “model”? They define a model as follows:

By “model” we mean a system of functions and conditions that yield formal results, such as classes of equilibria within the model. The specific type of model-building that has been central to 20<sup>th</sup> century economics is a mathematical system of “agents” who maximize explicit functions subject to constraints, yielding equilibria. As many have noted, it is a kind of story-telling.... Nowadays, the term “model” is generally used by economists to mean a formal, explicit system using mathematical representation. That is how we use the term here. (Klein and Romero 2007, 243-244)

How is the term “theory” related to “model” – and what else should these terms be related to? The authors spell out three of their concerns about the usage of these terms. First, they cite Leijonhufvud’s observation that the terms “models” and “theory... have been widely used as interchangeable in the profession” (Lei-

---

<sup>3</sup> We examined several economic literatures in order to evaluate whether a fruitful dialogue exists in economics between conceptual analyses (“modeling”/“theory”) and empirical work (Goldfarb and Ratner 2006).

jonhufvud 1997, 193). They reject this equating of the two terms, asserting that a “model is neither necessary nor sufficient for theory”<sup>4</sup> (244). A second related point is that, in economics, the term “theorist” usually means “model builder.” Klein-Romero also reject this conjoining, since it suggests that “Hume, Smith, Marx, Menger, Keynes, Coase, Schelling etc etc did not do theory” (244).<sup>5</sup> Third, they argue that a theory involves more than technical/analytical desiderata: “[S]cientific culture understands theory to entail requirements of importance and usefulness”<sup>6</sup> (244).

In sum, after defining “model,” Klein-Romero argue that “theory” has a higher normative status than “model.” Moreover, a theory does not require a “model” and a “model” is not sufficient for a “theory.” However, they leave the term “theory” undefined. They do specify three requirements a model must meet in order for them to deem it a theory:

1. The model is “an at-least-partial or potential description of the conditions and mechanisms giving rise to X,” where X is “some real-world phenomena.” They call this the “Theory of what?” criterion.
2. “The proponent believes and tries to persuade us that X is of import” and is inadequately understood, so there is “[a] need for better explanation.” This is the “Why should we care?” criterion.
3. “The proponent makes a case that his explanation merits attention and resources.” This is the “What merit in your explanation?” criterion.

Notice that this set of requirements for a *model-based* theory has three characteristics: First, “models” are theory wannabes. Only a really good model gets promoted to theory status.<sup>7</sup> Second, the idea that “theory” might operate at a very general level, while “models” might be specific *applications* of a theory (a theoretical framework) is missing. Third, this usage makes no allowance for the possibility that models are sometimes (though not always) *a link between theory frameworks and the activities of empiricists*.

4 We agree that the terms need to be distinguished.

5 Not treating the terms as equivalent seems sensible. On “who is a theorist,” compare Robert Solow (1997), whose viewpoint we discuss extensively below. “Keynes more or less invented macroeconomics. He was not much of a model builder himself” (49). Solow clearly considers Keynes a theorist but not “much of” a model-builder.

6 We are not persuaded that the understanding of “theory” that Klein-Romero ascribe to “scientific culture” is anywhere near universal. As we hope to show, scientific culture (at least among economists) encompasses different groups (“subcultures”) that are likely to have differing interpretations of “theory.” These several conceptual interpretations coexist, however uneasily. More broadly, “theory” and “model” bear more meanings than in the Klein-Romero universe.

7 We are reminded of the Star-Kist Tuna commercial of several years ago. Charlie the Tuna wants to be a Star-Kist tuna, but he is rejected because he is not good enough for Star-Kist (“Sorry, Charlie...”). So models in the Klein-Romero view *aspire* to be theories. But to succeed they must meet the Star-Kist test: if a model is not “good enough,” it **cannot become** a theory (“Sorry, Model...”).

We suggest that these three characteristics of Klein-Romero's usage are not shared by, and appear antithetical to, substantial and important uses of the terms "model" and "theory" in economics. We develop this argument using two examples: (i) Robert Solow's use of the term "model" in an article that tries to explain to noneconomists what economists do; and (ii) economists' widespread use of the general terms "price theory," "game theory" and "growth theory"; these terms have implications for delineating one concept of "theory" and for the model/theory distinction.

### HOW ECONOMISTS USE "MODEL" AND "THEORY" —TAKE ONE: EXAMPLES FROM SOLOW

A use of the terms "theory" and "model" quite different from Klein-Romero's has been espoused by Robert Solow:

Today, if you ask a mainstream economist a question about almost any aspect of economic life, the response will be suppose we model that situation and see what happens....A model is a deliberately simplified representation of a much more complicated situation.... The idea is to focus on one or two causal or conditioning factors, exclude everything else, and hope to understand how just these aspects of reality work and interact. There are thousands of examples; the point is that modern mainstream economics consists of little else but examples of this process. (Solow 1997, 43)

Solow develops three illustrative examples, which are instructive for comparing his view of 'model' to Klein-Romero's: modeling the effect of taxation on the willingness to work; explaining inventory fluctuations as an ingredient in understanding business cycles; and explaining trade patterns among nations. Notice that each of these modeling efforts is aimed at shedding light on an arguably important *empirical phenomenon*.<sup>8,9</sup>

8 In Solow's view, what makes for a "good" model is the amount of "understanding" generated: "A good model makes the right strategic simplifications. In fact, a really good model is one that generates a lot of understanding from focusing on a very small number of causal arrows...."

9 This linkage to important phenomena is a point of contact between Solow and Klein-Romero as well as a point of difference. Solow relates 'models' and modeling to a focus on an important empirical phenomenon. Klein-Romero say that a model is close to being a theory when the model meets the "theory of what?" test—when it attempts to illuminate or explain an empirical phenomenon—and when the model meets the "why should we care?" test—a measure of the importance of the phenomenon modeled. However, unlike Solow, who seems content with treating models of important empirical phenomena simply as models, Klein-Romero are interested in rewarding models that pass these two tests—plus a third—with the label 'theory.'

Recall Klein-Romero's description (quoted more fully above) of a model as "a mathematical system of 'agents' who maximize explicit functions subject to constraints, yielding equilibria.... Nowadays, the term "model" is generally used by economists to mean a formal, explicit system using mathematical representation." In contrast, Solow argues that this particular kind of mathematical representation is *not a requirement* for a model. A model can instead, for example, be described "in diagrammatic form" (46). Note that this point about diagrams is perfectly consistent with standard classroom usage. When teaching introductory or intermediate microeconomics and putting the simple demand-supply diagram on the board, many of us describe that diagram as a *model* of price-quantity determination in a market.

The Solow discussion is antithetical to the Klein-Romero idea that models typically strive—or should strive—to become theories. The kinds of models Solow describes are instead attempts to illuminate specific economic phenomena, and in Solow's view are provoked by the growing availability of data, and the puzzles that data present:

I have a different hypothesis to suggest—that technique and model-building came along with the expanding availability of data, and each reinforces the other. Each new piece of information about the economy, especially if it is quantitative information, practically sits there and asks for explanation. Someone will eventually be clever enough to see that it is now feasible to construct a model. Reciprocally, alternative models have to compete... They compete on the basis of their ability to give a satisfying account of some facts. Facts ask for explanations, and explanations ask for new facts. (Solow 1997, 47)

So what is the relationship between the kinds of models Solow describes and the notion of theory in the Solow discussion? He is at pains to argue that economics is in the main *not* mere mathematical formalism. It is "technical," but not "formalist." He sees "formalist theory" as mathematical formalism, largely unrelated to the modeling activities that occupy the majority of applied economists:

The past 50 years has indeed seen formalist economics grow and prosper. But it has not grown very much. Only a small minority within the profession practices economic theory in this style. To tell the truth, not many more pay any attention at all to formalist theory. Generally speaking, formalists write for each other. The formalist school contains some extraordinarily able people, and of course attracts economists who not only are talented at economics of a certain kind but enjoy it. It is not surprising, therefore,

that outsiders think that there is a lot of formalism in economics, just as half a cup of blood spread around a bathroom makes it look like a scene from *Psycho*. Nevertheless, it is an illusion. Modern mainstream economics is not all that formal. (43)

From this point of view, Klein-Romero's proposal to rename the *Journal of Economic Theory* as the *Journal of Economic Model-Building* is misguided. A more appropriate title-change might be the *Journal of Formalist Economics*.

In summary, two things distinguish the Solow description of models from the Klein-Romero view of "the specific type of model building that has been central to 20<sup>th</sup> century economics...a mathematical system of 'agents'." First, *the kinds of models Solow describes are in large part attempts to shed light on specific empirical phenomena*. While in these models the manner of exposition and much of the reasoning is often mathematical, the motivation behind the models is to make better sense of some feature of the observed world. Second, models as described by Solow do not aspire to be "promoted" to theories, since they have a much more concrete, limited, and specific purpose. Note that the kind of model Solow describes fits the first item in Klein-Romero's list of what a *theory-worthy* model would contain ("a description of...some real world phenomena"). It does not, however, fit the way those authors describe the *actual* modeling "central to 20<sup>th</sup> century economics."

We certainly are not denying the existence of the kind of (formalist) modeling from which Klein-Romero seek to remove the verbal crown of 'theory'. Instead, we are noting that a major subset of modeling activity—the kind described by Solow as *typifying* what much of what goes on in economics—is omitted from the Klein-Romero characterization of what they see as "central to 20<sup>th</sup> century economics." A commenter on an earlier draft of this paper suggested describing the two kinds of models as "applied" and "unapplied." We return later in the paper to this issue of how to categorize these disparate classes of models.

What about the relation between "model" and "theory"? Solow does not really address the *general* relationship between models and the unmodified term "theory." Instead, a particular kind of theory—"formalist theory"—is dismissed as basically unconnected to modeling. Note that classifying a subset of theory as "formalist" is consistent with the hypothesis posed at the beginning of this article that terms like "theory" and "models" are overworked. Solow modifies the term "theory" to limit it, carving out a subset of theory by adding the adjective "formalist."<sup>10</sup>

10 In a methodological survey of "Economics at the Millennium," Goldfarb and Leonard (2002) present a taxonomy of types of economists. They describe pure theorists as "in the business of logically deducing the implications of a set of behavioral axioms taken as fundamental (Hahn in *Economic Journal* 1991, 47), an enterprise describable as Euclidean in spirit... Pure theorists prove theorems and lemmas. Most of game theory is of the pure theory type. The connection between pure theory and the economy runs from tenuous to none. At its most archetypal (for example, the Nobel-prize-winning 1959 work of Gerard Debreu) pure economic theory does not even purport to have empirical consequences. As theorist Ariel Rubinstein puts it... 'pure theory does not pretend to predict or advise... the most [it] can

So Solow does not really address the model-theory relationship, while Klein-Romero seem to view models as theory wannabes. In the next section, we propose one *possible* relationship between models and theory that we think is both plausible and consistent with one widespread usage.

### HOW ECONOMISTS USE “THEORY” AND “MODEL” — TAKE TWO: THEORY AS ENCOMPASSING VARIOUS MODELS

Our thesis in this section is simple: *A widespread use of “theory and “model” is that “theory” is a broad conceptual approach— as in “price theory”— while “models,” typically in mathematical (including graphical) form, are applications of a theory to particular settings and/or represent explorations of different sets of assumptions conditionally allowable by the theory approach.* Thus, for example, the term “price theory models” has a fully understandable and standard meaning.

Support for this usage is found in the titles of classic graduate-level texts: Milton Friedman published several editions of his microeconomics lectures, and titled various iterations *Price Theory: A Provisional Text* and *Price Theory*. George Stigler published several editions of *The Theory of Price*. Gary Becker’s lectures on microeconomics at Columbia, as originally transcribed by his students, were published as *Economic Theory*. More recent graduate texts include Andreu Mas-Colell, Michael Winston, and Jerry Green’s *Microeconomic Theory*. Each of these books contains discussions of *various models that apply price theory* in particular contexts. Indeed, almost any price theory text will propose different models of monopoly behavior, monopsony, etc. For example, in Becker’s published lectures, he sets out a *model* of an irrational consumer and compares the implications of this *model* for the existence of market demand curves to the implications of models that assume rationality.

As for “price theory models,” so for “game theory models” or “growth theory models.” Such models are specific applications of a general theory framework. The notion of a specific game theory model of duopoly is, we believe, self-evidently understandable within the economics profession. The same is true for the term “Cournot model.” One of us took a course entitled “Growth Theory” in graduate school many decades ago. The instructor, a very distinguished economist, proceeded class after class to set forth various competing growth theory models. All the models were part of growth theory, but some models were far more “attractive” than others.<sup>11</sup>

This idea can be illustrated more concretely with the example of human

---

do is to clarify the concepts we use’ ” (Goldfarb and Leonard (2002, 22).

11 On one occasion, one of the co-author’s fellow students was extremely frustrated by what he viewed as the ridiculous assumptions required for one of these models. In a memorable line, he left the class, muttering under his breath, “It’s your model, you play with it.” Some growth theory models were “better” than other growth theory models.

capital theory, a general approach to modeling individuals' decisions to "invest in themselves." That approach has led to a major reorientation in the issues on which labor economics focuses. Its development is associated with major contributions from Gary Becker and Jacob Mincer. Within human capital theory are numerous specific models. A particularly influential one was Mincer's so-called "simple schooling model." That model tried to explain variation in incomes using *only* variations in schooling. Later models, provoked in part by the possible inadequacy of considering only schooling, added other explanatory variables meant to proxy, for example, on-the-job training. There are in fact numerous models that fall under the heading "human capital theory models."

Note that these models often involve an attempt to apply the general insights of human capital theory in a way that *allows confrontation with actual data*. Sherwin Rosen wrote an appreciation of Mincer's major contributions to the human capital theory literature that appeared in the *Journal of Economic Perspectives* (1992). Rosen noted that a standard empirical procedure in labor economics is now widely known as "Mincering the data." Recall that the idea of "models as a link from theory to data" is absent from, or at least not obvious in, the Klein-Romero concept of models.

So one possible, and we believe widely-used, way of conceptualizing "theory" versus "models" is that "theory" represents a general approach, while models are ways of specifying and applying that approach to more focused situations. Notice further that our use of the terms is descriptive, not normative or evaluative. Particular theories and models may be attractive ("good") or unattractive ("bad") but the title of "theory" by itself has no strong normative connotation in this usage.

Many examples are available showing that the just-described usage of "theory" and "models" is common when economists actually do economics. The appendix contains four specific examples.

### WHAT IS TO BE DONE? — A TAXONOMY OF MODELS

We suggested above that the term "model" (and, analogously, the term "theory") may be unable to carry all the weight of competing possible interpretations. We explore this idea further by suggesting a number of related-but-differentiated meanings associated with the term "model." Consider first two broad but mutually inconsistent categories of models.

#### 1. *Abstract, formal theory model/evaluative-interpretive "toy" model/"unapplied" model.*

The Arrow-Debreu model is an example of an abstract, formalized, mathematical representation of production, exchange, and consumption. Such a model

is not intended to refer to any particular economy in historical time, nor is it intended to yield testable predictions about variables of interest. Instead, it is meant to illuminate the logical implications of certain primitive concepts, such as whether a competitive equilibrium exists as a logical matter, to be established by mathematical reasoning. This conception appears to be consistent with the sense of “model” in Klein-Romero: see the quotation early in this article, which describes what they mean by “model.”

In his discussion of the term “model” as having a variety of meanings, Kevin Hoover (1995) suggests a related but not precisely identical category: “evaluative or interpretive models” (which he opposes to “observational models”).<sup>12</sup> He goes on to describe a subclass of evaluative-interpretive models, so-called “toy models:”

A toy model exists merely to illustrate or to check the coherence of principles or their interaction. An example of a toy model is the overlapping-generations model with money in its simplest incarnations. No one would think of drawing quantitative conclusions about the working of the economy from it. Instead one wants to show that models constructed on its principles reproduce certain qualitative features of the economy and suggest other qualitative features that may not have been known or sufficiently appreciated. (33)

This kind of model is “a testbed for general principles” (33). As noted above, a commenter suggested the general term “unapplied model” to encompass this category.

## 2. *Models of observed phenomena/ “applied” model.*

John Sutton provides the following quote from John von Neumann<sup>13</sup>:

By a model is meant a mathematical construct which, with the addition of some verbal interpretations, describes observed phenomena. The justification of such a mathematical construct is solely and precisely that it is expected to work (Sutton 2000, 35).

---

<sup>12</sup> In a previous footnote, we cited Solow’s view that Keynes, who “more or less invented macroeconomics,” was not much of a model builder. Solow goes on to say “*The General Theory* was and is a very difficult book... It contains several distinct lines of thought that are never quite made consistent. It was an extraordinarily influential book...but we learned not as much from it...as from a number of explanatory articles ...(that) reduced one or two of those trains of thought to an intelligible *model*, which for us became ‘Keynesian economics’ ” (48). This rendition is consistent with the Hoover notion of some models as explanatory-interpretive.

<sup>13</sup> Sutton does not provide a specific citation for the von Neumann quote.

This concept of “model,” which stresses the tie to empirical phenomena, is clearly inconsistent with the prior “abstract formal theory model” concept. It is related to and consistent with Mary Morgan’s (1998) idea of models as “mediating” between theories and data. Kevin Hoover (1995) uses the term “observational models.” He further notes that “[O]ne commonly speaks of an econometric model. Here one means the concrete specification of functional forms for estimation” (1995, 33).

While the two above categories are mutually inconsistent, a third category includes a different expositional mechanism consistent with both of the above categories:

### *3. Diagrammatical iterations of both of the above model types.*

Graphs are in fact mathematics, of course, but common usage sometimes inaccurately distinguishes “diagrammatic” from “mathematical.” Both of the model categories described above can in some cases be presented diagrammatically rather than “mathematically in the nondiagrammatic sense.” Simple supply-demand diagrams are a staple of economics pedagogy and frequently show up in articles. Ditto for indifference-curve or isoquant analyses.

Liebenstein’s 1950 article, “Bandwagon, Snob and Veblen Effects in the Theory of Consumers’ Demand,” works almost entirely with diagrams. Francis Bator wrote two historically important articles developing welfare economics; one of them, entitled “The Simple Analytics of Welfare Maximization,” is a marvelous, largely diagrammatic exposition. Samuelson’s “A Diagrammatic Exposition of a Theory of Public Expenditure” is yet another historically important example. The fact that these articles are “old” does not change the fact that models of both of the above types can be developed graphically; their vintage testifies instead to a change in style of exposition in economics in recent decades.

This change in style—the fact that many articles are now largely “mathematical in the nondiagrammatic sense”—should not obscure the fact that graphical analysis is central to some modeling applications. For example, one’s understanding of the complexity of the Earned Income Tax Credit (EITC), and how its effects must be analyzed, is immeasurably improved by a diagram showing the complex way in which the EITC rules change the individual’s work-leisure constraint. The same constraint can of course be expressed algebraically, but the complexities required for the analysis are much harder to intuit and keep straight without the diagram.<sup>14, 15</sup>

14 One of us recently co-authored an article analyzing alternative motivations for dieting. The model uses u-shaped indifference curves (and even circular indifference curves) between weight and food. Additional pounds become a “bad” beyond the individual’s desired weight, giving rise to the u-shaped indifference curves. The important (linear upward-sloping) constraint comes from a biological relationship, well-documented in the physiology literature, between weight and food intake. Different motivations for dieting are set off by different life events or by the endogenous effect on the constraint from aging. The analysis is

Having specified three broad categories of models, we propose some additional less general subcategories, each with examples.

- *“Conceptual orientation/technique” models.* These types of models are recognizable by the conceptual orientation they embody and the technique(s) they employ. In some, the conceptual orientation is a more marked feature, in others, the technique. Most if not all of the following are distinguished by both elements: Behavioral economics models, game theory models, econometric models, structural/reduced form models, calibration models, computable general-equilibrium models, input-output models/linear programming models, two- (or multi-) sector models, models of bounded rationality, Austrian school models, rational expectations models.
- *Substantive area/problem category models.* These include human capital models, growth theory models, business cycle models, overlapping generations models, market structure/oligopoly/dominant firm models, the kinked demand curve model, entry-deterrence models, labor supply models, public goods models, capital-asset pricing models, options pricing models, tax incidence models, exhaustible resource models, common pool models, inventory models, information cascade models, firm location models, fishery models, epidemic models, models of altruism.
- *“Named” models.* Examples include Harris-Todaro models, Harrod-Domar models, Tiebout models, Phillips curve models, Mincer’s simple schooling model, Schumpeterian models, the Cournot model, the Stackelberg model, Hotelling models.

These three categories are in increasing order of specificity, so that, for instance, named models will each belong to one of the entries in the previous two subcategories. Thus, Hotelling models are instances of firm location models. Furthermore, models in these three subcategories do not necessarily fall neatly into one and only one of the previous three general model categories (“abstract, formal theory model,” “models of observed phenomena,” etc.). As a result, complex cross-classifications are possible.

Our discussion of models versus theories is “economics home-grown” in the sense that it represents our generalizations and inferences about the use of these terms in economics from our perspective as economists. However, a dif-

---

entirely diagrammatic and much easier to intuit because it is diagrammatic. See Goldfarb *et al.* (2006).

15 A commenter on an earlier draft of this paper suggested the following provocative hypothesis for the decline in diagrammatic analysis. When doing comparative statics, one can in fact derive results from diagrams (this is consistent with the example cited in the previous footnote). Recent economics, however, has a very large component of dynamic analysis. It is often impossible to *derive* useful results about dynamics using diagrams, though diagrams may be usable to *illustrate* some dynamic results.

ferent, far broader perspective on our subject is offered by the philosophy of science. This involves considering how philosophers of science who focus on physics, biology, etc. conceptualize the terms ‘theory’ and ‘model.’ Unfortunately, the arguments of philosophers of science are sometimes complex, expressed in terms internal to their discipline, and not easy to apply to economics. For excellent discussions of work in the philosophy of science and its implications for economics, see Mary Morgan (1998) and W. Wade Hands (2001, especially 343-352). Morgan notes that:

Older treatments in the mainstream philosophy of science defined models in terms of their logical and semantic connections with theories, where the latter are the real focus of interest. (The topic has also been beset by definitional changes which hinder attempts at simple exposition). Thus the conventional account from the logical positivist tradition defined theories as uninterpreted formal systems: sets of sentences in a formal language characterized by its syntactic structure (such as an axiomatized system). An interpretation constitutes a model of the theory if and only if all the sentences in the model are also true in the theory (the formal system). This account of models has not proved very useful in the philosophy of economics. (316)

However, the description Morgan provides of the work of the philosopher Nancy Cartwright bears a similarity to the “home-grown economics” view of models we have espoused above. As Morgan describes Cartwright’s position:

In Cartwright’s account, models are idealizations or approximations in the sense that they are only partially realistic accounts of the world, but the descriptions they offer are sufficient to describe certain aspects of the phenomena. They are also required to map onto the mathematical representation of the fundamental theory, although...not necessarily in full... Although the idealization literature focuses on theorizing, in effect models appear inevitable in this procedure: models can be identified as things you get as you idealize towards theory away from reality or concretize from idealized theory to the economic reality. Models are not of the theory or of data or of phenomena... rather they form a middle element between theory and the world, incorporating different degrees of both. The process of modeling becomes the main activity of both economic theorizing and economic application. (318)

It is also worth noting Morgan’s (2002) use of the following phrase: “this

mid-century...way of fitting theories to the world via models” (7). That is, in this view, and for one type of model, models *fit theories to the world*.

## CONCLUSION

This discussion has yielded several results:

1. Klein-Romero’s attempt at terminological reformation regarding “theory” faces a hard slog, given the facts of how the term is used among economists. Specifically, treating theories as models that in certain respects are desirable/preferred while regarding “non-theory” models as negative—as merely theory wannabes—does not jibe with these two terms’ common usage in modern economics. Models that meet Klein-Romero’s three criteria may well have greater merit than those that do not. Nonetheless, economists’ usage of “theory” is more catholic than would be permitted by their three criteria. In fact, this last point underlines a difference in purpose between Klein-Romero and us: their stance is prescriptive. They want economists to reform their use of the term “theory.” Our stance is mostly descriptive: we are interested in how economists use that term and the range of its usage. To the minimal extent that we are prescriptive, we suggest ways to enhance clarity of communication among economists.
2. There are models and then there are models. A number of economists distinguish between two types of models: those that involve abstract theorizing, largely devoid of empirical referents and empirical implication, and those that attempt to connect or “mediate between” theory and data. We consider this an important and fruitful distinction, in part because it enhances the quality of economists’ communication. Our 2006 paper relies on that distinction; we think that incorporating it would enhance Klein-Romero’s interesting analysis and argument.
3. The term “model” seems to be overburdened and hence incapable of conveying the meaning the user intends. We noted above a distinction between two types of model (“abstract” or “interpretive/toy” or “unapplied” versus “empirically oriented” or “observational” or “applied”). In fact, the term “model” has many and varied uses in economics, some antithetical to others. Consequently, to foster effective communication, categories are needed that distinguish different types of models. We suggested above one very tentative taxonomy.
4. While our view of modeling is “home-grown” in the sense that it stems from observing what economists do, it seems consistent with at least one view from the philosophy of science associated with Nancy Cartwright.

# APPENDIX: SOME SPECIFIC EXAMPLES OF HOW THE TERMS “THEORY” AND “MODEL” ARE USED WHEN ECONOMISTS DO ECONOMICS

*Sutton’s “Taxi Supply-Demand” and Auction Model Examples.* John Sutton’s Gaston Eyskens Lectures at the University of Leuven were published by Leuven University Press and MIT Press in 2000 under the title *Marshall’s Tendencies: What Can Economists Know?*. Sutton asks, “Is it possible to find economic models that work?” (xvi). His treatment of this question provoked considerable interest. One indication of this interest is that the April 2002 issue of *Economics and Philosophy* contains a symposium on Sutton’s views, with contributions by two of the major writers on economic methodology, Kevin Hoover and Mary Morgan, and three well-known econometricians, including Franklin Fisher.

While Sutton does not explicitly address the “models versus theory” issue, several of his examples contain an implicit and useful-for-us view of that relationship. Consider first his “taxis-at-airports” example, provoked by his observation on a visit to San Diego that there were long lines of taxis waiting for passengers. His taxi driver:

Counted on only four fares a day with a two- to three-hour wait each time. It wasn’t hard to figure out what had gone wrong. The city fathers, responding to the prevailing fashion for “deregulation,” had abolished restrictions on the number of licenses. Fares remained about the same as before...and new drivers entered the business[until income was driven down to that of alternative occupations.] (Sutton 2000, 2)

Later in the book, Sutton considers what model might explain why fares failed to drop. His point is that:

the elementary competitive model of supply and demand is the wrong model here for one of its key assumptions is that consumers enjoy full information on rival firms’ prices. For the taxicab market, this is rarely a good assumption. In the case of San Diego, it is badly wrong. The appropriate model for this market is one that distinguishes two groups of consumers, “informed” and “uninformed.” (88)

The implication of this example for “theory versus models” is that “price theory” *applied to a particular market and circumstance generates alternative models* for explaining the behavior of that market.

A second Sutton example involves game theory applied to auctions:

During the past ten years, the study of auctions has attracted an unusual degree of interest among applied game theorists. One reason...lies in the fact that, in an auction, the rules of the game are specified explicitly, so we are close to knowing the true model of the situation. It is not fully known, however, since we do not usually know the value each bidder places on the item, nor is this information available to rival bidders. (47)

Sutton then describes a setting in which this “not fully knowing” problem is minimized: bidding for drilling rights in offshore tracts. He then describes the results of modeling *this specific case*.

As in the taxi example, the point here is that game theory generates the analysis of auctions. But the application of game-theoretic auction theory to concrete cases involves the need for a *model* of each concrete case.

*Theory versus Models of Price-Quantity Determination.* Provoked by reading a previous draft of this paper, a colleague offered the following interpretation of theory versus model, a reading complementary to the Sutton taxi and auction examples. The colleague suggested that price theory implies that prices and quantities are determined by the interaction of supply-side and demand-side factors. Note that this general description is broad enough to include a variety of market structures, not just perfectly competitive markets. A *model* of price-quantity determination in a specific assumed-close-enough-to-competitive market would involve specifying actual demand and supply functions for that market.

*Lind's Analysis of Rent Control Models.* Hans Lind (2007) presents an analysis of a series of eight models of rent control that appeared between 1997 and 2003 in several of the major urban, regional and real estate journals. Each model presents an analysis in which rent control may lead to Pareto improvements because of special conditions in the housing market or special provisions of the control legislation. He criticizes these modeling efforts, arguing that they add nothing to our knowledge of the actual effects of rent control, in part because each analysis fails to provide telling empirical evidence that the conditions postulated by the model hold in a number of actual local markets.

Lind's analysis is relevant to our issue of models versus theories. The existence of a series of models of rent control differing in specific assumptions about “the world” illustrates yet again the idea that models are specific applications to specific (often, market) situations on which the analyst is trying to shed light. The models Lind describes are typically applications of microeconomic *theory*.<sup>16</sup>

---

16 One could in fact coin the term “rent control theory,” which would be a category containing and organizing a series of alternative models of rent control. Such a category would be analogous to our use above of the term “game-theoretic auction theory.” To continue the analogy, this theory category would be “price-theoretic rent control theory.” An interesting question is: Under what conditions would this kind of intermediate category be helpful, in the sense of adding value beyond what the term “price-theoretic rent control models” tells us?

*A Modeling Interpretation of the Leontief Paradox.* Bledin and Shewmake (2004) have proposed the following modeling interpretation of the relation between the Heckscher-Ohlin framework and the Leontief Paradox:

Once the input-output model represents the American economy in this way, Leontief can apply foreign trade statistics to measure the factor requirements for US international trade.

In providing this measurement, the input-output model mediates between Heckscher-Ohlin Theory and the world. While the Heckscher-Ohlin Theorem suggests that a capital abundant American economy will export capital-intensive goods, international trade theory provides no mechanism to assess this conclusion. Nevertheless, Leontief's input-output model 'enables us to narrow the frustrating gap between theory and observation' (Leontief 1953, 67) by facilitating an empirical test of the Heckscher-Ohlin Theorem. (468)

Bledin and Shewmake show that a test of a theoretical proposition, the Heckscher-Ohlin Theorem, is made possible by the input-output model, a model not even from the same international-trade-theory-framework that generates the theorem. Once again, a model is used to connect a theory-result to the empirical world.

## REFERENCES

- Becker, Gary S.** 1971. *Economic Theory*. New York: Alfred Knopf.
- Bledin, Justin and Sharon Shewmake.** 2004. Research Programs, Model-Building, and Actor-Network-Theory: Reassessing the Case of the Leontief Paradox. *Journal of Economic Methodology* 11(4): 455-476.
- Bator, Francis.** 1957. The Simple Analytics of Welfare Maximization. *American Economic Review* 47(March): 22-59.
- Carroll, Lewis.** [1871] 2003. Through the Looking Glass. Reprinted in *Alice's Adventures in Wonderland and Through the Looking Glass*. Signet Classics.
- Friedman, Milton.** 1962. *Price Theory: A Provisional Text*. Chicago: Aldine.
- Friedman, Milton.** 1976. *Price Theory*. Chicago: Aldine.
- Goldfarb, Robert S., and Thomas C. Leonard.** 2002. Economics at the Millennium. *Society* 40(1): 24-36.
- Goldfarb, Robert S., Thomas C. Leonard, and Steven Suranovic.** 2006. Alternative Motivations for Dieting. *Eastern Economic Journal* 32(1): 115-132.

- Goldfarb, Robert S., and Jonathan Ratner.** 2006. Exploring Different Visions of the Model-Empirics Nexus: Solow versus Lipsey-K-S. Mimeo. Presented at the ASSA annual meetings, Chicago, January 2007, and the History of Economics Society meetings, George Mason University, June 2007. (Forthcoming, *Journal of Economic Methodology*).
- Hahn, Frank.** 1991. The Next Hundred Years. *Economic Journal* 101(404): 47-50.
- Hands, W. Wade.** 2001. *Reflection Without Rules: Economic Methodology and Contemporary Science Theory*. Cambridge, UK: Cambridge University Press.
- Hoover, Kevin.** 1995. Facts and Artifacts: Calibration and the Empirical Assessment of Real-Business-Cycle Models. *Oxford Economic Papers* New Series, 47(1): 24-44.
- Klein, Daniel B., and Pedro P. Romero.** 2007. Model Building versus Theorizing: The Paucity of Theory in the *Journal of Economic Theory*. *Econ Journal Watch* 4(2): 241-71. [Link](#).
- Leontief, Wassily.** [1953] 1986. Domestic Production and Foreign Trade: the American Capital Position Re-Examined. Reprinted in his *Input-Output Economics*, 2<sup>nd</sup> Edition, 65-93. New York: Oxford University Press.
- Liebenstein, Harvey.** 1950. Bandwagon, Snob and Veblen Effects in the Theory of Consumers' Demand. *Quarterly Journal of Economics* 64(2): 183-207.
- Leijonhufvud, Axel.** 1997. Models and Theories. *Journal of Economic Methodology* 4(2): 193-198.
- Lind, Hans.** 2007. The Model and the Story Told: An Evaluation of Mathematical Models of Rent Control. *Regional Science and Urban Economics* 37(2): 183-198.
- Mas-Colell, Andreu, Michael D. Whinston, and Jerry R. Green.** 1995. *Microeconomic Theory*. New York; Oxford: Oxford University Press.
- Morgan, Mary S.** 1998. Models In *The Handbook of Economic Methodology*, ed. John Davis, D. Wade Hands, and Uskali Maki, 316-321. Cheltenham, UK: Edward Elgar.
- Morgan, Mary S.** 2002. How Models Help Economists to Know (Symposium on Marshall's Tendencies 1). *Economics and Philosophy* 18(12): 5-16.
- Rosen, Sherwin.** 1992. Distinguished Fellow: Mincering Labor Economics. *Journal of Economic Perspectives* 6(2): 157-170.
- Rubinstein, Ariel.** 1998. *Modeling Bounded Rationality*. Cambridge Mass: MIT Press.
- Samuelson, Paul A.** 1955. Diagrammatic Exposition of a Theory of Public Expenditure. *Review of Economics and Statistics* 37(4): 350-356.
- Solow, Robert.** 1997. How Did Economics Get That Way, and What Way Did It Get? *Daedalus* 126(Winter): 39-58.
- Stigler, George.** 1946, 1952, 1966, 1987. *The Theory of Price*. New York: Macmillan.
- Sutton, John.** [2000] 2002. *Marshall's Tendencies: What Can Economists Know?* Leuven and Cambridge, Mass: Leuven University Press and MIT Press. (Page numbers from 2002 paperback)

### ABOUT THE AUTHORS



**Robert Goldfarb** is a professor of economics and public policy at George Washington University. Prior to that, he was an assistant professor at Yale University. His early research and publications were largely in labor economics, covering such topics as geographical wage dispersion and the effects of immigration laws. Since the mid-1980s, his research interests have had three branches: economic methodology (e.g., problems in testing economic theories), economics and ethics (e.g., the status of claims that microeconomics should incorporate moral norms); and applied microeconomics and public policy (e.g., why people choose harmful behaviors). He has published widely in academic journals on all these topics. His email is [gldfrb@gwu.edu](mailto:gldfrb@gwu.edu).



**Jonathan Ratner** is a senior economist at Westat, an employee-owned research firm in Rockville, Maryland. Previously, he was an Assistant Director at the Government Accountability Office (GAO), a congressional agency, and an assistant professor at Wellesley College and the State University of New York at Albany. He also served on detail from GAO as a staff member for the U.S. Senate Finance Committee. His academic publications address topics in health economics, macroeconomics, defense economics, and forecasting. He is the author or co-author of numerous GAO reports on health economics and policy. He has testified at congressional hearings and appeared on NBC Nightly News. His email is [jonratner@westat.com](mailto:jonratner@westat.com).

[Go to January 2008 Table of Contents with links to articles](#)

## CHARACTER ISSUES



### Left Out: A Critique of Paul Krugman Based on a Comprehensive Account of His *New York Times* Columns, 1997 through 2006

DANIEL B. KLEIN WITH HARIKA ANNA BARLETT<sup>1</sup>

#### ABSTRACT

Admittedly, the range within which I acknowledge mental activity as competent and beyond which I reject as superstition, fatuity, extravagance, madness, or mere twaddle, is determined by my own interpretive framework.

—Michael Polanyi (1962, 318-19)

**THE ACCOMPLISHMENTS OF PAUL KRUGMAN ARE PRODIGIOUS. HE HAS WRIT-**ten or edited more than 25 books, 40 scholarly articles, and 750 columns at the *New York Times*, where he continues to write a twice-weekly column. Krugman received the John Bates Clark Medal in 1991 for his research in international trade. He taught at Yale, MIT, and Stanford prior to joining the faculty of Princeton. His eminence as a public-intellectual economist in the United States today is unsurpassed.

By providing regular commentary on American politics and policy, Krugman answers a vital calling. He admirably bypasses several common restraints. He leaves behind inhibitions about being “normative.” He is refreshingly outspoken. His discourse is plain and natural. Although his writings focus on economics, he does not let economics confine him. He takes up important issues even if economics is secondary, and in treating an issue he argues beyond the economics. More generally, he boldly assumes the role of one who takes up the most important things. He assumes the character of one who will do his best on whatever is of utmost importance. Individuals who assume that lofty role are rare, and still rarer are those who do it with any substantial success. Krugman is truly excep-

---

<sup>1</sup> Department of Economics, George Mason University, Fairfax, VA 22030.

tional.

Inherent in the mountain-top position is a kind of independence that often borders on madness. The thinker develops a unique and creative take on the world of affairs and culture. He can work to make it more or less responsible, but he can never step outside of himself to test and re-examine it from some other mountain top. Friends and colleagues will doubt some of the interpretive leaps, and their closeness may depend on their not thinking or commenting critically. In the crucial judgments that make his take characteristic, the thinker functions in a kind of isolation. Nor can anyone else establish her sensibilities as enlightened. The lofty will always remain somewhat distant and disconnected—a predicament indicated by how much Nobel economists disagree on policy.<sup>2</sup> If there are enlightened answers to policy issues, then some of the Nobels are wrongheaded, and most likely the wrongness stems from delusion at deep levels of interpretation—of the world and of themselves.

Harika Barlett and I<sup>3</sup> have made a complete review of Krugman's *New York Times* columns 1997 through 2006—in all, 654 columns. Here I interpret his ideological sensibilities. I think they are quite wrongheaded, but that claim is not something I attempt to defend. I do not dispute isolated statements. My critique assesses the 654 *NYT* columns as a whole. I argue that the pattern of policy positions and arguments do not square with his purported concern for general prosperity and the interests of the poor. There are contradictions between what Krugman makes himself out to be and certain patterns of his policy statements. Some of the evidence lies in statements made. But the more important evidence lies in patterns of statements not made. Because Krugman assumes the role of addressing the most important things, because Barlett and I have made a complete survey of his *NYT* columns 1997 through 2006, and because the omissions are flagrant, I may treat omissions as evidence of Krugman's ideological character and sensibilities.

Krugman is best interpreted as a committed social democrat and Democratic partisan. My main contention is that his social-democratic impetus sometimes trumps people's interests, notably poor people's interests. The tension surfaces in what Krugman has written about immigration and the threat it poses to the US welfare state. But the tension is found in his writings on several topics, and, importantly, in omissions in his writings. Krugman has almost never come out against extant government interventions, even ones that expert economists

---

2 As a group, economists disagree on policy more than any other group of social scientists (Klein and Stern 2005, 286).

3 *About the authorship of this paper and the appendices:* I conceived of this project and invited Harika Barlett, a PhD student at my home institution of George Mason, to pursue it as course work. She collected, collated, abstracted, and managed the 654 *NYT* columns, she drafted what has turned into Appendix 2, and she did the extensive work contained in the Excel files. Because I am the senior author and because the main article is composed by me, I have used the first-person singular. The authorship of the "Taking Stock" Appendix is listed as Barlett and Klein.

seem to agree are bad, and especially so for the poor.

Of course, Krugman might reply that advancing the social-democratic ethos is necessary to improving well-being. Indeed, Krugman has suggested that, because of political dynamics, promoting the long-term interests of poor people depends on promoting a social-democratic ethos in the United States and, more particularly, the Democrats over the Republicans.<sup>4</sup> I maintain that the tension between Krugman's *NYT* corpus and economic betterment is strong enough to present problems either way. If Krugman would deny that there is significant tension, then he functions irresponsibly, in ways indicated below. If Krugman would admit that, to some extent, he is ready to sacrifice poor people's interests for the sake of social-democratic values, then he has to admit conflict among relevant values and give up posturing to the effect that he has been a voice of unbiased research and has stood above any ideological interpretation of affairs.

The commitment to a social-democratic ethos as against poor people's interests is by no means specific to Krugman. He typifies something much wider, the establishment sort of social-democratic mentality as manifested in the United States. The principal reason that I scrutinize Krugman is that he is brilliant, outspoken, relatively candid, industrious, and highly visible and influential. Investigating him is a way of investigating the larger cultural phenomenon. Like any vital thinker, Krugman opens himself to public examination. Moreover, he is known to impeach people's motives, scruples, and psychology.

Krugman's first *NYT* op-ed appeared in February 1997 and he wrote three more during the following years. Since the beginning of 2000, he has written a twice-weekly op-ed column. His 654 op-ed articles 1997 through 2006 are available electronically at the *New York Times* website ([link](#)).<sup>5</sup> At the end of this paper is a link to an Excel file (Appendix 1) containing Krugman's 654 articles, itemization of topics, quotations of policy suggestions and judgments, and notes about Krugman's judgments.

### TAKING STOCK OF THE 654 COLUMNS

Paul Krugman's 654 *New York Times* articles 1997 through 2006 covered the topics listed in Table 1. In addition to the Excel sheet linked as Appendix 1, Barlett and I have written an extensive review of the main themes and policy judgments found in the 654 columns—a simple “book report”—that helps to demonstrate the seriousness of our treatment of the material. We decided not to include that review in the present, already bulky, paper, but we provide it as a separate docu-

4 Examples of columns in which Krugman suggests that his position on a policy is affected by the resultant political dynamics include that of 3/31/06 (on immigration), 2/27/04 (on protectionism), and 6/10/05 and 2/27/06 (on inequality).

5 Alternatively, one may go to the “Unofficial” Krugman archive ([link](#)).

ment, linked as Appendix 2. It enhances this paper by showing more thoroughly that the content of the columns fit the interpretations given here.

**Table 1: Paul Krugman's *NYT* Articles 1997 through 2006**

Topic	Number of articles
Taxation/tax cuts, government programs, budget deficit, and fiscal responsibility	124
Monetary policy	31
Economic, growth, and income inequality	64
New economy and the stock market bubble	17
Globalization and free trade	21
Oil prices	8
Appointments/nominations of leaders at major financial institutions	7
Social security reform/privatization	41
Regulation/deregulation	35
Health care system	32
Microsoft's monopoly case	7
Corruption and accountability in government and business	82
Elections	30
Government's role in emergency management	10
National security, Iraq war and war against terrorism	81
Global warming and disinformation	5
Others	59
<b>TOTAL</b>	<b>654</b>

### LOOKING OUT FOR THE POOR

Most every ideology maintains that it would serve the poor better than the status quo does. But that is not the same as saying that it claims to make poor people's interests its sole or uppermost goal.

The liberal tradition of Adam Smith, Frédéric Bastiat, Herbert Spencer, William Graham Sumner, Friedrich Hayek, and Milton Friedman maintains that a regime of private ownership and preponderant laissez-faire works out rather well for people regardless of their level of wealth or income. That tradition opposes any extensive welfare state. Do the classical liberals maintain that welfare-state

policies—in the US context, tax progressivity and programs like Social Security, Medicare, and free services like schools—are bad for the less well off? The answer is unclear. They might contend that welfare-state policies ill-serve the poor, for concomitant effects on morals, culture, political dynamics, incentives, and the service sectors involved. Like Smith, they generally believe that distributive justice involves virtues that should be pursued voluntarily.<sup>6</sup>

My perspective is classical liberal. I caution the reader not to slip into thinking that classical liberalism characterizes the foils Krugman sets against himself, chiefly Republican politicians. By and large, they do not represent classical liberalism, first, because they are politicians, and secondly, because they are Republicans.

The Left tradition, from Marx to the modern social democrats, also says its platform would better serve the poor. It has always highlighted distributional conflict, the capitalists versus the workers, or the rich versus the poor. With the welfare state at the center of its agenda, modern social democracy is strongly committed to the idea that governmentally required redistribution advances the interests of the poor. But social democracy does not necessarily make that a supreme political value. It too pursues a wider, open set of sensibilities, including morals and culture, and, probably, in the end, tends to maintain that governmentally-required redistribution is a virtuous characteristic of the polity, a characteristic that will serve the moral and spiritual well-being of all classes of society, at least once they come to accept the idea.

### KRUGMAN'S CONCERN FOR THE POOR

Krugman exhibits the leftist tendency to focus on distributional politics and to favor greater government-required redistribution. In an autobiographical essay, Krugman (1995) writes: "It was, in a way, strange for me to be part of the Reagan Administration. I was then and still am an unabashed defender of the welfare state, which I regard as the most decent social arrangement yet devised."

Krugman also exhibits the leftist tendency of fashioning oneself as looking out for the poor. When Krugman criticizes some policy, he will likely say it hurts the poor and serves some group of rich. That refrain pervades Krugman's articles on the income-tax cut, estate taxes, Social Security reform, corporate scandals, and emergency management, and it appears in many other topics, including the execution of the Iraq war. The following is characteristic: "the end result was a redistribution of the tax burden away from the haves toward the have-nots" (1/11/02).

And when Krugman favors some policy, he will likely say it advances the interests of the poor. About reforms in Britain he said: "But there's no denying

---

<sup>6</sup> My belief that such was Adam Smith's view is based on a wide array of things that Smith said and did not say, but in particular passages in *The Theory of Moral Sentiments* (1790, 78-85, 175-76, 269-70, 327).

that the Blair government has done a lot for Britain's have-nots. Modern Britain isn't paradise on earth, but the Blair government has ensured that substantially fewer people are living in economic hell. Providing a strong social safety net requires a higher overall rate of taxation than Americans are accustomed to ..." (12/25/06).

Krugman never proclaims that, for him, the supreme political value is advancing the interest of the poor. Still, that there might be tensions between helping the poor and other cherished goals is something that Krugman almost never acknowledges—although one exception, immigration policy, is found and treated here. Krugman carries on as though his sensibilities coincide so neatly with that goal that there is little tension to address. Concern for the poor, therefore, comes across as emblematic of what Krugman stands for.

### THE SOCIAL-DEMOCRATIC POLITICAL ETHOS

I propose to interpret Krugman by recourse to some broader conjectures about human penchants and the nature of certain political ideologies. The present investigation serves as a context for developing and testing those conjectures.

One such conjecture concerns the appeal of social democracy. I contend that concern for the poor is much less central or primary than is usually claimed. What I see as more primary is the making of identities and feelings of solidarity and togetherness based on the mythology of cooperative, collective endeavor. Acting together toward common ends and commonly experiencing the narrative make for an approximation of common knowledge (Chwe 2001), an imagined mutual coordination of sentiment, and an imagined community (Anderson 1991). Part of the penchant is a yearning for sentiment to encompass all the people, at least in the imagination—what I have elsewhere termed "the people's romance" (Klein 2005). Thus, the impetus to pursue collectively goal X is not so much the achieving of X as the collective doings supposedly done to achieve X. The penchant for encompassing sentiment by way of collective endeavor may well have origins in the evolutionary environment (Hayek 1978; 1988). In some respects, the classical-liberal tradition has sought to subdue the political and coercive assertion of such penchants. Liberalism (here and henceforth, in the original sense of the term) is a resistance to ambitious schemes for collectivist endeavor and experience. Those with strong collectivist penchants or otherwise playing upon them find that it is strategically effective to choose an X that thwarts resistance. An optimal X would likely have the following features: first, a strong, immediate emotional appeal, playing directly on the natural human impulses toward sympathy and compassion; second, a plausible argument that the goal cannot be well met by voluntary practices; and third, in as much as the political endeavor is not actually effective in advancing X, a murkiness in assessing the effectiveness of the political efforts to advance X. It is hard to imagine an official goal that better meets

these conditions than that of helping the least well off and otherwise protecting disadvantaged groups from supposed and ill-defined exploitations and injustices. I do not mean to suggest that any leader of the spirit ever thought in terms of such optimization, sat down to solve it, and arrived at the answer of redistribution and helping the disadvantaged. But we understand that, in the economy and in culture, sometimes circumstances adopt behaviors that correspond to how an optimizing agent would adapt to circumstances (Alchian 1950).

The “people’s romance” interpretation suggests, then, that, in the social democratic mentality, what is more primary than better conditions for the poor is the collective endeavors supposedly aimed toward that end. What is more primary than any equality achieved is the equalizing. What is more primary than any help rendered is the supposed helping. What is more primary than any education achieved is the supposed educating. It is the doing—collective and supposedly cooperative—that primarily animates the action.

One might look at the problem somewhat differently. Suppose that it were understood that the collectivist impulse could not be much subdued; suppose one had to act subject to the constraint that there was bound to be an official collective endeavor and an official X. Suppose further that it was a classical liberal who was to select and fix the X subject to such constraints. There may be no better solution, in his eyes, than the X that social democracy has selected and exalted. In that hypothetical, the official endeavors of social democracy may be testament of liberalism’s constrained success. Social democracy as we know it may be the form of the people’s romance with which liberalism is best able to co-exist.<sup>7</sup>

I say the doing is “supposedly cooperative.” Collectivist penchants face serious challenges from liberal sensibilities against coercion and domination. That is why the social democratic mentality depends on precepts or tacit beliefs that deny or reinterpret those aspects of the agenda. There have emerged superstitions that hold that the rules of the polity are a matter of consent (“no one is forcing you to be here”), and that the government is the agent of the people. As Tocqueville (1840, 693-94) observed, democratic superstitions allow citizens to feel that they are above the government and yet subservient to and a part of a larger entity. Implicit are the ideas that the polity is an encompassing organization, the government is the appointed manager, and government rules are the terms and conditions of that organization, like the rules that employers specify in employment contracts or landlords specify in rental contracts. The state collectivity is overlord, the true owner of all property within the polity. Thus, the ugly aspects of the social democratic vision are interpreted away. People who choose to be in the polity are agreeing to the rules of the organization and at least passively choosing to cooperate in its goals.

Franklin Roosevelt personified and now symbolizes the American mentality

---

<sup>7</sup> Elsewhere I explore whether advancing liberty can function as such an X, and conclude in the negative (Klein 2005, 24-31).

of state collectivism. In his first inaugural address he expressed it nicely:

If we are to go forward, we must move as a trained and loyal army willing to sacrifice for the good of a common discipline. We are, I know, ready and willing to submit our lives and property to such discipline, because it makes possible a leadership which aims at a larger good. I assume unhesitatingly the leadership of this great army. ... I shall ask the Congress for the one remaining instrument to meet the crisis—broad executive power to wage a war against the emergency, as great as the power that would be given to me if we were in fact invaded by a foreign foe.<sup>8</sup>

### **KRUGMAN PROPOUNDS A SOCIAL-DEMOCRATIC POLITICAL ETHOS**

Krugman does not wax at length about the moral and cultural virtues of statist endeavor, but time and again in passing remarks he propounds a social-democratic political ethos.

Krugman idolizes Franklin Roosevelt, and clearly for his assertion of government as the leading force in society: “FDR’s mission in office was to show that government activism works” (9/16/05). “Franklin Roosevelt, in his efforts to combat economic woes, was famously willing to try anything until he found something that worked” (3/12/04). Krugman extols FDR’s “huge expansion of federal spending, including a lot of discretionary spending by the Works Progress Administration,” and notes that the administration avoided corruption (9/16/05). Krugman also extols the centralization of powers formerly exercised by decentralized governments (9/16/05).

The goal was to help the have-nots. “Franklin Roosevelt favored the interests of workers” (8/18/06). Krugman regards “the public safety net FDR and LBJ created” to be one of the great defining achievements of America (5/13/05). “Moderates and liberals want to preserve the America FDR built” (2/8/05). But the appreciation goes beyond the creation of the welfare state. Krugman celebrates the overcoming of inhibitions: “The reason World War II accomplished what the New Deal could not was simply that war removed the usual inhibitions. Until Pearl Harbor Franklin Roosevelt didn’t have the determination or the legislative clout to enact really large programs to stimulate the economy. But war made it not just possible but necessary for the government to spend on a previously inconceivable scale, restoring full employment for the first time since 1929” (9/13/02).

Krugman writes of George W. Bush: “Indeed, in crucial respects he’s the anti-FDR. President Bush subscribes to a political philosophy that opposes gov-

---

<sup>8</sup> Quoted at p. 41 in Schivelbusch (2006), which I strongly recommend.

ernment activism—that's why he has tried to downsize and privatize programs wherever he can" (9/16/05). Krugman explains the motives of the anti-FDR forces: Social insurance programs "protect Americans against the extreme economic insecurity that prevailed before the New Deal. The hard right has never forgiven FDR (and later LBJ) for his efforts to reduce that insecurity, and now that the right is running Washington, it's trying to turn the clock back to 1932" (2/8/05).

Again, the lynchpin is the magical role made of democracy. It determines and articulates the collectivity's uppermost decisions, sets the collective goals, and demonstrates that the politician, even the President, is subservient to the ordinary citizen. In an election-day column in 2004, Krugman quotes a correspondent from Florida: "To see people coming out—elderly, disabled, blind, poor; people who have to hitch rides, take buses, etc.—and then staying in line for hours and hours and hours[,] Well, it's humbling. And it's awesome. And it's kind of beautiful."

Krugman follows:

Yes, it is [beautiful]. I always get a little choked up when I go to the local school to cast my vote. The humbleness of the surroundings only emphasizes the majesty of the process: this is democracy, America's great gift to the world, in action.

But over the last few days I've been seeing pictures from Florida that are even more majestic. They show long lines of voters, snaking through buildings and on down the sidewalk: citizens patiently waiting to do their civic duty. Those people still believe in American democracy; and because they do, so do I. ... [I]t's already clear that the people of Florida—and, I believe, America as a whole—have refused to give in to cynicism and spin.

Far from being discouraged by what happened in 2000, they seem to realize more than ever—and better than those of us in the chattering classes—what a precious thing the right to vote really is. And they are determined to exercise that right. ....

Regardless of their politics, most Americans understand that this is a crucial election, and that never before has their vote mattered so much for the nation's destiny. ... [T]he more people vote, the more vital is our democracy. ... By coming to the polls, citizens are literally giving a vote of confidence in American democracy. And in so doing, they are proving themselves wiser than some of those they elected. ... Above all, though, I want to see democracy vindicated, and the stain of 2000 eradicated, by a clean election in which as many people as possible get to cast their votes, and have those votes counted.

And all the evidence says that's what the American people want, too.  
May all of us get our wish. (11/2/04)

Thus, Krugman openly displays the aesthetic sensibility served by democratic rites and superstitions. Although Krugman often lambastes sitting politicians, he affirms the validity and functionality of the democratic process: "The truth is that the government delivers services and security that people want. Yes, there's some waste—just as there is in any large organization" (12/29/06).

The rules of the polity-organization are forged in the "social contract." To explain what the Social Security debate is all about, Krugman tells a fable:

There once was a land where people lived only two years. In the first year they worked; in the second year they lived off their personal savings.

There came a time when the government decided to help out the elderly. So it instituted a system called Social Security. Every young, working individual would pay a tax, which would be used to pay benefits that same year to each older, retired individual. (10/11/00)

Krugman explains that "Social Security has never been run like a simple pension fund. It's really a social contract" (3/5/02). Then, he continues, "an ambitious politician came along, declaring: 'It's your money!'" and seeking to renege on the contract (10/11/00).

The same social-democratic worldview is evident when Krugman writes of health insurance: "If Truman had succeeded [in creating a national health insurance system], universal coverage for everyone, not just the elderly, would today be an accepted part of the social contract" (6/13/05). Later, Medicare, a compact covering a portion of the population, was achieved: "America decided 35 years ago to guarantee health care to older citizens" (9/10/00).

The presuppositions extend not merely to the tax take and government programs. In order for social democrats to view the minimum wage law and myriad similar regulations as NOT coercion, as NOT incursions on freedom, they must hold that such rules are like the contractual rules within an organization, which implies that all resources are the property of the state or people. For example, Krugman quotes approvingly one member of the finance industry: "Financial markets are as much a social contract as is democratic government." Krugman adds, "Yet there is a growing sense that this contract is being broken" (5/17/02).

A corollary is that individual liberty or freedom is not a meaningful concept. To acknowledge individual liberty would be to acknowledge bona fide individual *ownership* of self and other resources. By searching on the terms "freedom" and

“liberty,” I confirm that, with but one exception<sup>9</sup>, in the 654 columns Krugman never accords any validity to those concepts—indeed, he occasionally slights them (e.g., 7/4/05). And when he advocates what liberals would regard as contraventions of the principles, for example, when he advocates an increase in the minimum wage (7/14/06), he says nothing to indicate that it constitutes a restriction on freedom of contract and hence individual liberty. The minimum wage is a rule one agrees to in being in the organization.

### SOCIAL DEMOCRACY AND THE POOR

Social democracy—fashioned as “liberalism” in the United States—has received a lot of good press. One reason is that the press through the twentieth century became increasingly dominated by social democrats, as did most other political and cultural institutions.<sup>10</sup> They have generally validated, endorsed, and celebrated the image of social democracy as a system that serves the poor and helps the disadvantaged. Social democracy certainly deserves some of that image. But the genuine aspects of the image should not blind us to the ways in which social democracy works systematically against the poor. Here I highlight some that relate to Krugman's columns.

- Its myths and superstitions are perennially opposed by the Locke-Smith conceptions of ownership, liberty, and coercion. In consequence, social democracy cannot but help to work to disparage and subvert such notions—it is necessary to the overcoming of classical-liberal “inhibitions.” The result, especially since the 1930s, has been the great attenuation of such restraints and the corresponding unbridling of statist impulses in the domain of policy. Two kinds of impulses deserve special mention: first, impulses that appeal to our innate collectivist penchants; second, impulses to garner special privileges for one's own group (sometimes called rent-seeking). Often, the result is a multi-lateral struggle for powers and immunities—if only to try to avoid being tread upon by the powers unleashed. The interventions that have spilled out are often extremely bad. They often curtail betterment generally—and, by the power of compounding, a diminished growth rate will mean significantly diminished “poor” living standards in a soon actual and thereafter ever lesser than would have been *henceforth*. But very often the

<sup>9</sup> The single exception pertains to a restriction outside of the American polity: “the Argentine government has imposed drastic restrictions on economic freedom. Most notably, residents are now limited to withdrawing \$1,000 per month from their bank accounts” (12/11/01).

<sup>10</sup> Those developments have not, however, been principally guided by an invisible hand. In general, cultural development does not exhibit strong invisible-hand properties, particularly when coercion plays a large role. One reason is that, in the contest between statism and liberalism, the latter, being a philosophy of voluntarism, is much less inclined or able to use government to gain cultural power and to assert its ideas and values. The plainest illustration is schooling, both K-12 and higher education.

badness can be understood in terms of injury now, especially to the poor.

- But, reading the social democrats, you'd scarcely know it. They need to affirm that interventions express the collective will. The democratic process, though imperfect and subject to abuse, basically works.<sup>11</sup> The implication is that status-quo interventions tend to be in the neighborhood of the right policy. Thus, any longstanding intervention enjoys a presumption of rightness. The burden of proof is thrown onto anyone who would challenge status-quo interventions. Interventions that hurt society in general and the poor in particular, such as the public school system or "consumer protection" restrictions, at least have the passive support of social democrats. They sometimes push for new interventions in health care, etc., but they only rarely call for repeal or liberalization of existing interventions—although, I admit, that the more "progressive" sort (as opposed to the more establishment sort) sometimes favors liberalization in drugs, prostitution, immigration, and a few other issues, and often strongly opposes military actions. For most social democrats, the spirit of abolitionism is alien and offensive. Indeed, in his allusions to the times of FDR, the 1950s, and the Clinton years, Krugman's attitude is often nostalgic and complacent (e.g., 9/8/06).
- Besides being complicit in extant interventions, social democrats are especially partial toward government programs to inculcate the mythology of "the people" engaged in collective endeavor. Social Security is us taking care of us. It is a part of the collectively produced "social safety net," which safeguards all of us in all of our activities, thus spanning life within the polity. But aside from redistributive policies, there are programs like the government ownership and operation of schools, the postal system, transit systems, and so on that will generally have the support of social democrats for their mythological properties of collective endeavor and experience. And, inversely, as the people's romance depends on the focalness of government power, social democrats often show jealousy and hostility towards independent centers of cultural power and experience, from private schooling to shopping malls to private corporations and private concentrations of wealth, and discomfort with private means of withdrawing from the collective experience, such as home schooling and private automobility. The cultural greediness of social democracy often has policy consequences that especially hurt the poor, consequences that social democrats rarely acknowledge.
- The people's romance defines "the people" by the polity. Souls across the border just don't count for much, even though much poorer than Americans.

---

<sup>11</sup> Elites in general tend to affirm that the way things are basically works—maybe because their selfhood and elite status rest on such structures and their legitimation.

Although social democrats generally favor free trade and globalization, they are much less comfortable with any significant liberalization in immigration. They might say that it harms poor working Americans. More significantly, it jeopardizes the popularity, if not the fiscal viability, of the welfare state. In general, letting the concern for the poor extend beyond the border would blur the myths of encompassing endeavor and experience and would upset the justifications based on democracy, since any notion of “the people” as all humanity would mean that souls beyond the border are “disenfranchised.” The key myths would unravel.

- Also, there are cultural consequences. The people’s purpose according to social democracy—raising up the poor, helping the disadvantaged, making conditions and opportunity more equal—are degrading in the way they categorize people into “rich” and “poor” and demeaning specifically to those categorized as “the poor,” “the have-nots,” etc., as they are presumed to be highly dependent on statist sustenance. Rather than calling, as Smith did, for more liberty “to enable [the people] to provide such a revenue or subsistence for themselves,”<sup>12</sup> social democrats almost never call for more liberty but rather call for renewed collective efforts.

### KRUGMAN AND POOR NON-AMERICANS

During the 1990s, Krugman wrote regularly for *Slate*. His *Slate* writings, earlier books such as *The Accidental Theorist* (1998), and earlier *NYT* writings often communicated basic liberal economics. By the end of 2006, however, the long experience of writing for habitual *NYT* readers and receiving their feedback had worsened his discourse. His *NYT* writings show increasing reliance on partisan prejudice and emotion, pandering to collectivist penchants, and statism on the issues. One topic evincing such deterioration is immigration.

In 2000 and 2001 Krugman favored immigration without much qualification: “I am one of those people who feel that immigration is a good thing—most of all for the immigrants, but good for America too” (5/23/01). The view rested principally on “mundane economic arguments” (5/23/01), but also on American demographics (4/16/00; 6/21/00; 5/23/01). Krugman likened the anti-immigration movement to the ignorant anti-globalization movement and suggested that racism lies behind anti-immigration attitudes (5/23/01). Similarly, Krugman had earlier criticized anti-globalization activists and protectionists as “working against the interests of most of the world’s poor” (5/21/2000; 4/22/01).

By March 2006, his view had changed: “the crucial divide isn’t between legal and illegal immigration; it’s between high-skilled and low-skilled immigrants. High-skilled immigrants—say, software engineers from South Asia—are, by any

<sup>12</sup> Smith (1776, 428).

criterion I can think of, good for America. But the effects of low-skilled immigration are mixed at best” (3/31/06). He comes to the following policy conclusion: “Realistically, we’ll need to reduce the inflow of low-skill immigrants” (3/27/06).

Krugman’s illiberalism flows from the social-democratic ethos. He now minimizes the spontaneous benefits of liberal immigration: “First, the net benefits to the US economy from immigration, aside from the large gains to the immigrants themselves, are small” (3/27/06). In that column devoted to immigration, the only recognition of the benefits to the immigrants is that “aside.” The remainder speaks of jeopardy to the American people. “Because Mexican immigrants have much less education than the average U.S. worker, they increase the supply of less-skilled labor, driving down the wages of the worst-paid Americans.” In consequences, “many of the worst-off native-born Americans are hurt by immigration—especially immigration from Mexico.” The competition that Mexican immigrants pose to low-skilled Americans upsets the romance of helping America’s “have-nots” and of equalizing American conditions.

But labor competition is not Krugman’s main concern. “[M]odern America is a welfare state, even if our social safety net has more holes in it than it should—and low-skilled immigrants threaten to unravel that safety net” (3/27/06). Immigrants “increase the demand for public services, including health care and education. Estimates indicate that low-skilled immigrants don’t pay enough in taxes to cover the cost of providing these services” (3/31/06). But the fiscal burden is not large. “[T]he political threat that low-skill immigration poses to the welfare state is more serious than the fiscal threat” (3/27/06).

Politics “with high immigration tend, other things equal, to have less generous welfare states than those with low immigration” (3/31/06). The mechanism he highlights is that most low-skilled immigrants are not citizens and cannot vote: “a political system in which many workers don’t count is likely to ignore workers’ interests: it’s likely to have a weak social safety net and to spend too little on services like health care and education” (3/31/06). Another mechanism, not made explicit by Krugman, might be that if Americans believe that immigrants consume the benefits, they will be less favorable toward welfare statism.

Deeper ideological concerns become apparent as Krugman discusses the political nature of the problem. Once inside the country, immigrants become among us, a part of the political organization. “Since we aren’t going to deport more than 10 million people, we need to integrate those people into our society” (3/31/06). “Basic decency requires that we provide immigrants, once they’re here, with essential health care, education for their children, and more” (3/27/06). But they are “disenfranchised.”

A guest-worker program, even with a clear route to citizenship, would be a “violation of democratic principles” because “it could create a permanent underclass of disenfranchised workers” (3/31/06; 3/27/06). Democracy is violated when Roberto, the Mexican immigrant, works and lives but does not vote in the

United States. "Surely this would be a betrayal of our democratic ideals, of government of the people, by the people" (3/31/06).

Allowing Roberto that option weakens the superstitions that the people are defined by the polity, that they stand above the government by virtue of participating in elections, that they universally express their collective will, and that the resultant policies constitute a social contract. Allowing entry and existence within the United States to a disenfranchised Roberto "betrays our moral and democratic principles" (3/27/06). So long as Roberto neither works nor lives in the United States, it matters not that he is disenfranchised, because he is not a soul who counts as being among "the people."

Krugman's focus on voting, as opposed to, say, freedom, health, wealth, opportunity, the pursuit of happiness, or cultural cross-fertilization, verges on what Bryan Caplan (2007) has termed "democratic fundamentalism." Krugman writes, "we already have a large disenfranchised work force, and it's growing rapidly. The goal of immigration reform should be to reverse that trend" (3/31/06).

Roberto is typically much poorer than "poor" Americans. He wants the option of working and living in the United States. That option might be extremely important to him and his family. Here and elsewhere, the people's romance trumps concern for the poor.

#### **KRUGMAN FALLS SILENT: LIBERALIZATIONS THAT WOULD SIGNIFICANTLY HELP THE POOR**

Krugman's 654 columns quite regularly advocate or at least vaguely support government intervention. Examples relate to the following policy areas: immigration, the minimum wage, unions, health care provision, health insurance, Sarbanes-Oxley, financial markets, telecommunications regulation, media ownership, energy conservation and fuel efficiency, disaster insurance, disaster response, electricity provision, foreign aid, global warming, and of course taxation and the "social safety net" programs. (Appendix 1 details Krugman's support for intervention.)

Krugman has claimed, "I'm not an opponent of markets. On the contrary, I've spent a lot of my career defending their virtues" (11/14/05). The 654 columns provided Krugman ample opportunity to be pro-intervention on some issues and pro-liberalization on others.

A comprehensive analysis of the 654 columns shows, however, that Krugman has really sided with liberalization only on the following issues: rent control (6/7/00); US agricultural subsidies (5/7/02); international trade (e.g., 3/8/02; 3/24/02; 6/11/02; 11/28/03); mildly on high-tech anti-trust enforcement including the Microsoft case (often arguing that the government just cannot do anything to improve matters, e.g., 7/12/00; 10/22/00; 6/24/01; 7/1/01; 11/4/01); etha-

nol mandates and subsidies/tax breaks (6/25/00); NASA manned-space flight (it is only the manning of ships that he opposes; 2/4/03); European labor-market restrictions (3/29/00; 5/3/00); and the Terry Schiavo case (3/29/05).<sup>13</sup>

Thus, Krugman has sided with liberalization only rarely. And when it comes to established interventions, there are only two cases, rent-control and agricultural subsidies, each treated in but a single column, on which Krugman has ever advocated liberalization. Moreover, since the close of 2002 there has been no new and significant espousal of liberalization.

A great many policies in the United States contravene Smith's natural liberty. They are often so baneful on net, and so clearly so, that anyone, almost regardless of his or her professed values, ought to be strongly opposed to at least a goodly number of them. Yet the public culture presumes rightness in the status quo. That convention might arise in part from difficulties in agreeing on *which* policies should be opposed. Still, a decided opposition to many existing interventions should emanate from any individual who is informed and forthright in discourse. Yet such individuals of any significant prominence are rare. We understand why politicians refrain from criticizing the status quo. We are less candid about the extent to which very similar mechanisms apply more generally—pundits and intellectuals, too, will usually lose out on any significant prominence or establishment success if they openly challenge the presumption of the status quo. Public culture in the United States is itself highly politicized and taboo-ridden. Institutions such as the conventional media, K-12 schooling, and academia, and, more generally, the public culture, coordinate on a broad groupthink centered on the status quo and enmeshed in “liberal versus conservative” memes, which relate closely to the contest between the two parties. Reiteration, indoctrination, and practice turn it into a pervasive mentality, making a self-reinforcing, path-dependent cultural system. Anyone who operates accordingly is, regardless of leanings this way or that, abiding by a conventional mentality or sensibility. Krugman illustrates the status-quo mentality, as do most prominent pundits and intellectuals. Krugman's failure to challenge and oppose status-quo interventions is typical, but, again, it flies in the face of his professed concern for the poor, his pretensions of forthrightness, and his pretensions of standing above ideological commitments and biases.

This part of my critique turns especially on two things: First, because Krugman has long held the station of twice-weekly *NYT* columnist, and because he presents himself as a free-ranging public intellectual, it is reasonable to say that Krugman not merely is free to address what he thinks are the most important policies but even is expected to do so. He had ample opportunity to write now and then on whatever policies he thinks especially deserving of criticism, particularly for hurting the poor. Second, the review that Harika Barlett and I have made

---

13 Additionally, in a column on aid to Katrina victims, Krugman supported the issuing of housing vouchers over public housing (10/3/05), and criticized the administration for trying to cut the housing voucher program.

of the 654 columns is comprehensive. We speak authoritatively on what Krugman has not said in those columns.<sup>14</sup>

Here I examine Krugman's silence first by raising a few noteworthy cases and then by examining Krugman's record on a list of 57 potential federal liberalizations and privatizations.

**K-12 Schooling.** Krugman speaks often of equality and mobility, and relates them to education: "the way to mitigate inequality is to improve our educational system" (2/27/06). He has repeatedly expressed dissatisfaction with the school system, writing, "one key doorway to upward mobility—a good education system, available to all—has been closing. More and more, ambitious parents feel that a public school education is a dead end" (11/22/02), and, "public schools for those who can't afford to live in the right places have gotten worse" (5/21/00). There are few issues of more vital importance, especially to the poor, than schooling. Yet, remarkably, in 654 columns Krugman himself never says anything about why the public school system performs poorly or how to improve matters. I would contend that the poor performance of the public school system is easily explained by basic principles—the lack of choice, responsiveness, competition, private ownership, entrepreneurship, and so on. Moreover, the system lacks the cooperative spirit that comes especially from bottom-up voluntarism. Improvement is elementary: shift subsidization to the user and allow private schools to enter and displace government schools. A system of school vouchers would continue to subsidize any positive externality of education and would beat the present socialist system in nearly every dimension—quality, innovation, cooperation, and helping the poor. But there is one dimension in which the socialist system beats a voucher system: the people's romance and the inculcation of statist norms and attitudes. Government schooling—"common" in the sense of encompassing and universal—is one of the primary collectivist endeavors of the people's romance. The 13-year experience accustoms children and teenagers to government power and focalness. And the public schools greatly influence their ideas and beliefs. Surely it is for reasons such as these that Krugman falls silent on school vouchers. The only mention is the following: "And the administration continues to believe that 'financialization' is the way to go on just about everything, from school vouchers to Social Security" (8/17/01). It is hard to interpret silence, but presumably Krugman remains loyal to the public school system. As with immigration, the people's romance trumps concern for the poor.

**Interventions that eliminate lower rungs from the economic ladder.** A common trope in liberal economics, developed notably by Walter Williams (1984), is that a free system offers abundant opportunity to gain work experience, make contacts, and discover and develop own abilities, all constituting an "economic

---

14 Besides cataloging and analyzing every column in the Excel file linked at Appendix 1, we have confirmed our statements by putting all 654 columns into a single Word document easily searchable on statement keywords.

ladder,” but that many interventions eliminate low rungs of the ladder by privileging certain parties against low-positioned would-be competitors. Rich kids have family support and social capital to lift them up and grab hold of the remaining rungs; poor kids often do not. The leading example of such rung-removing intervention is occupational licensing, which directly affects more than 20 percent of US workers (Kleiner 2006). It is a prime example of banned-till-permitted “consumer protection” regulation. Economic analysis of the policy is extensive and quite devastating—approached from any angle, it tends to show that government’s one advantage and unique capability, the power of coercion, really does nothing to assure quality and safety that voluntary practices and tort law, working through myriad channels, cannot, yet has large ill consequences. Occupational licensing, it has been argued, reduces availability, selection, innovation, and quality received by consumers, while increasing prices and incomes of practitioners (Kleiner 2006). It makes it harder for poor people to mount and ascend the economic ladder and, by shifting labor supply functions, depresses wages in fields not subject to licensing. Other interventions that remove low-positioned rungs include union privileges and the minimum wage, but occupational licensing is the most significant in that economists who study and judge the policy mostly reach a conclusion in favor of liberalization (on medical licensing, see Svorny 2004). Yet Krugman never addresses the policy. In fact, in all of his utterances about the tribulations of the poor, he never points to any existing intervention as a livelihood obstacle. When Krugman writes, “Can anything be done to spread the benefits of a growing economy more widely?,” he makes but one suggestion: “A good start would be to increase the minimum wage” (7/14/06).

**The Food and Drug Administration.** There is probably no set of federal policies of greater moment to the public’s health than Congress’s blanket ban on new drugs and medical devices and the assignment to the Food and Drug Administration to consider whether to permit them and what manufactures may say about them. Scholarly evaluation of the system has been extensive. Many studies credibly argue that the existing system, relative to a more liberal system, is extremely injurious to the public’s health. Virtually all economists who express a policy judgment favor liberalization, including Gary Becker, Milton Friedman, Sam Peltzman, Peter Temin, and Kip Viscusi.<sup>15</sup> Again, the analysis is quite devastating, and in just the ways that an economist should expect for a banned-till-permitted “consumer protection” system. Some of the social losses are loosely identifiable and even quantifiable, and can be temperately described as tremendous and tragic. As with most regulatory failures, the damage arguably falls disproportionately on the poor, who are least able to cope, for example by traveling abroad for banned therapies or working their way to “compassionate use” access.

---

15 For a review of the scholarly literature on the FDA, and a compendium of 22 economist quotations favoring liberalization, see the extensive website [www.FDAReview.org](http://www.FDAReview.org) (Klein and Tabarrok 2002).

But nowhere<sup>16</sup> in the 654 columns does Krugman address the issue.<sup>17</sup> Indeed, he wrote, “we need to put aside our anti-government prejudices and realize that the history of government interventions on behalf of public health, from the construction of sewer systems to the campaign against smoking, is one of consistent, life-enhancing success” (7/8/05). Krugman might be ignorant of the economic analysis of banned-till-permitted systems in drugs and occupational licensing, but those issues are so momentous and the arguments for liberalization so compelling that we justly suspect deep-seated bias.

**On Liberalization, Krugman Is Called Out on Strikes.** Schooling, occupational licensing, and the FDA are just a few of the fat pitches that Krugman ignored. The number of missed opportunities to call for liberalization is practically endless. Why doesn't Krugman give half a column to the National Organ Transplant Act of 1984, which kills many and prevents poor people (and non-poor people) from selling a kidney? Why doesn't he protest restrictions on reproductive solutions and adoption services, which cause many couples to remain childless and unhappy? Why doesn't he write about drug prohibition, which massively incarcerates poor people and spreads violence and disorder particularly in poor neighborhoods? Why doesn't he protest, in addition to rent-control, other major housing and land-use restrictions that drive up housing costs? Why doesn't he call for the liberalization of transit services including shuttle vans, express buses, taxis, and spontaneous ride-share systems, which would reduce costs, enhance mobility, and add rungs to the economic ladder?

A list of 57 potential federal liberalizations and privatizations were presented to the economics faculty of George Mason University, who were asked to rank them in terms of their deservingness of reform discussion in the *Economic Report of the President*. Details are contained in Klein and Clark (2006, 477-481).<sup>18</sup> The 57 potential federal reforms included 35 liberalizations and 22 privatizations. The top ten liberalizations were: diminish trade restrictions, reduce agriculture subsidies and regulations, reduce FDA restrictions, reduce anti-trust enforcement and restrictions, reduce regulations on healthcare facilities and professionals, repeal restrictions on competitive mail delivery, liberalize drug prohibition, repeal laws that require banks to keep tabs on customers and report activity to the government, revisit Sarbanes-Oxley, and liberalize anti-discrimination laws.

We examined the 654 columns for treatment of the 57 potential reforms.

16 The closest he came to the issue are the following moments: Krugman expressed concern that dietary supplements were insufficiently controlled, whilst scorning fears of genetically-modified foods (3/22/00). Also, in a column critical of Ralph Nader, Krugman wrote: “When my arthritis stopped responding to over-the-counter remedies, I brought it back under control with a new regime that included the anti-inflammatory drug Feldene. But Mr. Nader's organization Public Citizen not only tried to block Pfizer's introduction of Feldene in the 1980's; it also tried to get it banned in 1995, despite what was by then a firm consensus among medical experts that the drug's benefits outweighed its risks” (7/23/00).

17 In a column subsequent to our review period, Krugman affirms FDA control and vaguely calls for more control (5/21/07).

18 The survey itself is available [here](#), and the Excel sheet containing the results is available [here](#).

The analysis and scoring is presented in the Excel sheet linked at Appendix 3, and the results are shown in Table 2.

**Table 2: Krugman's Record in Treating 57 Potential Federal Reforms**

<b>Krugman expresses</b>	<b>35 Potential Federal Liberalizations</b>	<b>22 Potential Federal Privatizations</b>
Support	2	0
Mild support	1	0
Opposition (interventionism)	10	4
Neither, on balance	2	0
Never addresses the issue	20	18
<b>TOTAL</b>	<b>35</b>	<b>22</b>

Krugman claims, “I admire the virtues of free markets as much as anyone” (9/2/2003). Yet Krugman at least tacitly supports status-quo interventions, while actively supporting many new ones. Although he claims to admire free markets, in the task of elucidating their virtues, to expose the unintended consequences of a wide variety of extant interventions, Krugman, aside from the issue of international trade, has been nearly a total loss. Krugman's silence on many of the issues, such as school vouchers, cannot be excused as ignorance. The logic of liberalization is too compelling, the import too great, the status of debate too high, that even if Krugman doubts that the liberalization would help the poor, the opportunity to address the debate and explain his doubts is overripe. The silence should be interpreted as elision. I chalk up Krugman's illiberalism to a status-quo mentality framed by “liberal versus conservative” memes, and, more particularly, a social-democratic ethos biased towards government intervention, especially those long sanctified by “our” democratic processes.

The “left out” method could be applied to other intellectuals who present themselves as addressing the most important things. For example, Dani Rodrik (2007) has said, “I look at the world and see some government programs that work and others that fail,” yet his communication of liberal economics is meager at best. I suspect that a thorough analysis of his writings would produce results like those for Krugman. The method may be applied beyond the left, although doing so will require alterations in step with professed goals and values. Many conservatives can be shown not to care about liberty as much as they make out.

**KRUGMAN'S POSTURE AS BEING ABOVE IDEOLOGICAL COMMITMENT AND BIAS**

Krugman's concern for poor people is secondary to his brand of public ethos. Sometimes he makes the primacy of ethos explicit, as when he writes, "The argument over Social Security privatization [is] a debate about what kind of society America should be" (3/15/05). I have demonstrated that Krugman is committed to supporting a social-democratic ethos, and that he interprets issues and information through social-democratic lenses. The demonstration has proceeded, first, by examining his affirmations of people's-romance type collectivist sentiments and values, and secondly, by showing that the patterns of his policy judgments fit a social-democratic agenda much better than a concern for general prosperity or poor people's interests.

The contradiction between Krugman's ideological worldview and his supposed concern for general prosperity and poor people could be easily resolved. All Krugman would need to do is be more candid about the primacy he gives to the social-democratic ethos. There would be nothing illegitimate in declaring that, faced with the trade-offs between vying characteristics of the polity and public culture, he is willing to make the necessary sacrifices. Such a posture would be natural, candid, and coherent. Some would add, morally and intellectually defensible. Indeed, many communitarians and collectivists have openly opposed liberalism and even prosperity.

If Krugman were to declare his specific commitments and aesthetic sensibilities, it would, however, run him into a second contradiction. Krugman habitually postures as though he somehow stands above ideological commitments and biases. He casts "ideology" as an aspersion, especially at politicians and think-tank personnel. He often accuses "conservatives" etc. of being blinded by free-market or anti-government ideology (e.g., 7/26/00; 7/8/05; 1/27/06). He called the people being considered for positions in the new administration "professional ideologues, who currently earn a living by repeating conservative slogans" (12/13/00).

In contesting the notion that government purchase of private-sector assets would politicize markets Krugman says: "But that's ideology, not analysis" (2/14/01). Krugman recurrently juxtaposes the poison of ideology with wholesomeness: "Ideology and cronyism take complete precedence over the business of governing" (5/15/06). The Clinton administration selected staff "notable more for their ability than their ideological fervor" (12/13/00). He hopes that leadership positions be staffed by "people who have made their reputations independent of their politics" (12/13/00).

Krugman naively writes as though leadership, policymaking, and discourse about the human condition can be separated from deep-seated ideological sensibilities. He especially favors academics—"academic research in economics is by and large carried out without strong political bias" (4/23/00), and he defends academia against charges of ideological bias (4/5/05). Krugman presumes that

the cultural and intellectual world that encircles him and the *New York Times* readership is somehow devoid of deep-seated preconceptions and commitments. He writes: “Moderates and liberals want to preserve the America FDR built. Mr. Bush and the ideological movement he leads...want to destroy it” (2/8/05). Thus, ideology is placed in contrast not only to analysis, ability, academic research, and “the business of governing,” but also to moderation and conserving “traditional social insurance programs” (5/15/06). “Liberals” like Krugman and the implied *New York Times* reader aren’t ideological, they’re just reasonable.

Although Krugman makes plain his partisanship and shows some candor about representing an ideology,<sup>19</sup> mainly Krugman presents himself as above ideology. He never faces up to the trade-offs and commitments that go with his ideology. He scarcely acknowledges that it often sacrifices other values, including general prosperity, poor people’s interests, and liberty.

### FINAL SPECULATION: THE GOVERNING-SET MENTALITY

Krugman propounds a social-democratic ethos, places undue faith in government and politics, and gives the presumption to the status quo. He opposes a classical-liberal ethos and systematically slights or elides the strong arguments for liberalization. In all that, I think Krugman is wrongheaded.

I have suggested that, in doing so, he appeals especially to the people’s romance. But is the people’s romance what steers Krugman? Yes, I suspect, to some extent. But to some extent I suspect that Krugman and many others push the people’s romance as a way of promoting the collectivism that they favor for other reasons as well. I see another kind of penchant in play, a penchant that gives rise to a mentality particularly of people of high strata who are chiefly concerned with being among what they regard to be the top of the pyramid of culture and power. Robert Nozick (1986) has suggested that “[t]he intellectual wants the whole society to be a school writ large, to be like the environment where he did so well and was so well appreciated.” Nozick suggested that “wordsmith” intellectuals resent “capitalism” for not according them the high status they come to feel entitled to from their experience in school. I am inclined to see such high-strata statist intellectuals as indulging the mythology of society as organization because that mythology gives structure and vision to the yearning to see oneself as part of the governing set—a mentality betokened in phrases like “the best and the brightest.” It is a mentality of those whose selfhood places them “near the top,” and who from such high station gaze upward. That such a penchant would be selected for in the environment of evolutionary adaptation is certainly plausible. It’s good to be the alpha male or one of his close companions. To my mind, Krugman typifies the profile. I find especially telling the enmity he holds toward

<sup>19</sup> As when he wrote, “it matters a lot which party is in power—and more important, which ideology” (8/18/06).

Republicans in power. He seems to resent not being among or not being able to identify with the people at the top. I suspect that Krugman's ideological direction has been determined more by a will to see oneself a part of what one perceives to be society's leadership than by infatuation with the people's romance. That penchant contributes to his dedication to a kind of politics that, given his setting and personal history, serves him in pursuing such sense of self and that, by delineating and inculcating a "society" that like an organization has and requires "leadership," accommodates the governing-set mentality itself.

## APPENDICES

Appendix 1: Excel file containing Krugman's 654 articles, itemization of topics, quotations of policy suggestions and judgments, and notes about Krugman's judgments. [Link](#). (4.75MB!)

Appendix 2: Taking Stock of Paul Krugman's 654 *New York Times* Columns 1997 through 2006, by Harika Anna Barlett and Daniel B. Klein; an extensive review of the main themes and policy judgments found in the 654 columns. [Link](#).

Appendix 3: Excel file containing the investigation of Krugman's treatment of 57 potential federal liberalizations/privatizations. [Link](#).

## REFERENCES

- Alchian, Armen.** 1950. Uncertainty, Evolution and Economic Theory. *Journal of Political Economy* 58(3): 211-221.
- Anderson, Benedict.** 1991. *Imagined Communities: Reflections on the Origin and Spread of Nationalism*. Rev. ed. London: Verso.
- Caplan, Bryan.** 2007. *The Myth of the Rational Voter: Why Democracies Choose Bad Policies*. Princeton: Princeton University Press.
- Chwe, Michael Suk-Young.** 2001. *Rational Ritual: Culture, Coordination, and Common Knowledge*. Princeton: Princeton University Press.
- Hayek, F.A.** 1978. The Atavism of Social Justice. In his *New Studies in Philosophy, Politics, Economics, and the History of Ideas*. University of Chicago Press.
- Hayek, F.A.** 1988. *The Fatal Conceit: The Errors of Socialism*. Chicago: University of Chicago Press.
- Klein, Daniel B.** 2005. The People's Romance: Why People Love Government (As Much as They Do). *Independent Review* 10(1): 5-37. [Link](#).
- Klein, Daniel B., and Michael J. Clark.** 2006. A Little More Liberty: What the *JEL*

Omits in Its Account of What the *Economic Report of the President* Omits. *Econ Journal Watch* 3(3): 466-483. [Link](#).

**Klein, Daniel B., and Charlotta Stern.** 2005. Professors and Their Politics: The Policy Views of Social Scientists. *Critical Review: An Interdisciplinary Journal of Politics and Society* 17(3-4): 257-303. [Link](#).

**Klein, Daniel B., and Alexander Tabarrok.** 2002. Is the FDA Safe and Effective? An extensive website sponsored by the Independent Institute (FDAREview.org). [Link](#).

**Kleiner, Morris M.** 2006. *Licensing Occupations: Ensuring Quality or Restricting Competition?* Kalamazoo, Michigan: W.E. Upjohn Institute for Employment Research. [Link](#).

**Krugman, Paul.** 1995. *Incidents from My Career*. [Link](#).

**Krugman, Paul.** Various years. Op-ed articles at the *New York Times*. [Link](#).

**Krugman, Paul R.** 1998. *The Accidental Theorist: And Other Dispatches from the Dismal Science*. New York: Norton.

**McCarty, Nolan, Keith T. Poole, and Howard Rosenthal.** 2006. *Polarized America: The Dance of Ideology and Unequal Riches*. Cambridge, MA: MIT Press. [Cited only in Appendix 2]

**Nozick, Robert.** [1986] 1997. Why Do Intellectuals Oppose Capitalism? Reprinted in *Socratic Puzzles*. Cambridge: Harvard University Press. [Link](#).

**Polanyi, Michael.** 1962. *Personal Knowledge: Towards a Post-Critical Philosophy*. Chicago: University of Chicago Press.

**Rodrik, Dani.** 2007. Irreconcilable Differences? *Dani Rodrik's Weblog*, 10 August. [Link](#).

**Schivelbusch, Wolfgang.** 2006. *Three New Deals: Reflections on Roosevelt's America, Mussolini's Italy, and Hitler's Germany, 1933-1939*. New York: Picador.

**Schuler, Kurt.** 2005. Ignorance and Influence: U.S. Economists on Argentina's Depression. *Econ Journal Watch* 2(2): 234-278. [Cited only in Appendix 2] [Link](#).

**Smith, Adam.** [1776] 1976. *An Inquiry into the Nature and Causes of the Wealth of Nations*, ed. by R.H. Campbell and A.S. Skinner. Oxford: Clarendon Press/Liberty Fund. [Link](#).

**Smith, Adam.** [1790] 1982. *Theory of Moral Sentiments*, ed. D.D. Raphael and A.L. Macfie. Oxford: Clarendon Press/Liberty Fund. [Link](#).

**Svorny, Shirley.** 2004. Licensing Doctors: Do Economists Agree? *Econ Journal Watch* 1(2): 279-305. [Link](#).

**The Unofficial Paul Krugman Archive.** [Link](#).

**Tocqueville, Alexis de.** 1969. *Democracy in America*. Edited by J. P. Mayer. Translated by G. Lawrence. New York: Doubleday.

**Williams, Walter E.** 1984. *The State against Blacks*. New York: McGraw-Hill.

**ABOUT THE AUTHORS**



**Daniel Klein** is professor of economics at George Mason University and associate fellow at the Ratio Institute in Stockholm. He is the chief editor of *Econ Journal Watch*. His email is [dklein@gmu.edu](mailto:dklein@gmu.edu).



**Harika Anna Barlett** is a Ph.D. student at George Mason University. She has an MBA degree from American University and a Master of City Planning Degree from Middle East Technical University (Ankara, Turkey). Her email address is [hbickici@gmu.edu](mailto:hbickici@gmu.edu).

[Go to January 2008 Table of Contents with links to articles](#)

## THE SOUNDS OF SILENCE

Critiques published in *EJW* often focus on particular works or doings. In such cases, we invariably invite the commented-on parties to reply.

In the table below, column A lists authors of critiques. The hyperlinks take you to the critique.

In column B are listed individuals who probably should have replied to the critique, but didn't. (It's never too late—the invitation remains open.)

—Daniel Klein

<i>Issue</i>	<b>A</b> <i>Commenting author(s)</i>	<b>B</b> <i>Commented-on parties who probably should have replied to the critique, but didn't</i>
1(1)	<a href="#">Jane Shaw</a>	Paul Collier and Jan Willem Gunning
1(1)	<a href="#">Edwin D. Maberly and Raylene M. Pierce</a>	Sven Bouman and Ben Jacobsen
1(2)	<a href="#">Peter Gordon and Lanlan Wang</a>	Rafael La Porta, Florencio Lopez-de-Silanes, Andrei Shleifer, Robert Vishny
1(2)	<a href="#">Susan Anderson and Peter Boettke</a>	<i>Journal of Economic Development</i> editors Pranab Bardhan, Mark R. Rosenzweig, and Carlos Vegh
1(3)	<a href="#">Peter Minowitz</a>	William D. Grampp
1(3)	<a href="#">James Forder</a>	Alan Blinder
1(3)	<a href="#">Fabio Rojas</a>	George Akerlof and Rachel Kranton
2(2)	<a href="#">Richard Timberlake</a>	Barry Eichengreen and Peter Temin
3(3)	<a href="#">Daniel Klein and Michael Clark</a>	Joseph Farrell, Jonathan Gruber, and Gordon Hanson
4(1)	<a href="#">Christopher Coyne and Steve Davies</a>	Kris James Mitchener and Marc Weidenmier; Niall Ferguson and Moritz Schularick
4(2)	<a href="#">Daniel Klein and Pedro Romero</a>	<i>Journal of Economic Theory</i> editors Jess Benhabib, Alessandro Lizzeri, and Karl Shell
4(2)	<a href="#">Ian Vasquez</a>	Anne Krueger
4(2)	<a href="#">Charles Blankart and Gerrit Koester</a>	Daron Acemoglu; (Torsten Persson and Guido Tabellini were invited to reply, and they pointed to their response to the same authors' similar criticism, in <i>Kyklos</i> 2006.)
4(3)	<a href="#">Bruce McCullough</a>	<i>Journal of Money, Credit and Banking</i> editors Pok-Sang Lam, Deborah Lucas, Masao Ogaki, and Kenneth D. West

[Go to January 2008 Table of Contents with links to articles](#)