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One Swallow Doesn't Make a Summer: A Comment on Zacharias Maniadis, Fabio Tufano, and John List

Mitesh Kataria[1]

LINK TO ABSTRACT

In their article "One Swallow Doesn't Make a Summer: New Evidence on Anchoring Effects," Zacharias Maniadis, Fabio Tufano, and John List—hereafter, MTL—claim that their "framework highlights that, at least in principle, the decision about whether to call a finding noteworthy, or deserving of great attention, should be based on the estimated probability that the finding represents a true association, which follows directly from the observed *p*-value, the power of the design, the prior probability of the hypothesis, and the tolerance for false positives" (MTL 2014, 278). MTL's article is intended to provide "insights into the mechanics of proper inference" (ibid.). Although I agree with most of the conclusions in MTL (2014), in this comment I raise some important caveats.

Theory and analysis

MTL are interested in the "Post-Study Probability (*PSP*)," which is the probability that a research finding that is statistically significant is true (MTL 2014, 284). Their equation (1), reproduced here as my equation (1), gives a formula for *PSP*:

1. University of Gothenburg, 40530 Gothenburg, Sweden.

$$\text{PSP} = \frac{(1-\beta)\pi}{(1-\beta)\pi + \alpha(1-\pi)} \tag{1}$$

Interpretations of the expression's terms are as follows:

- $\alpha = P(\text{test wrong} \mid H_0)$, the probability that the test statistic rejects H_0 (i.e., erroneously favors H_1) when H_0 is true,
- $1 - \beta = P(\text{test correct} \mid H_1)$, the probability that a research hypothesis is found significant when it is true,
- $\pi = P(H_1)$, the unconditional probability that H_1 is true.[2]

Alternatively, we can write an expression for *PSP* in terms of the probability that the null hypothesis H_0 is true given that the data D provides support for the alternative hypothesis H_1. The probability that a research finding that is statistically significant is false is

$$P(H_0 \mid D) = \frac{P(\text{test wrong} \mid H_0) \cdot P(H_0)}{P(\text{test wrong} \mid H_0) \cdot P(H_0) + P(\text{test correct} \mid H_1) \cdot P(H_1)} = 1 - PSP \tag{2}$$

Note the use of Bayes' theorem.[3] The approach represented in equation (2) is widely applied in medical and psychiatric diagnosis, where all of the terms in right-hand side of the equation are presumably known, including $P(H_0)$, which would be the unconditional probability of the prevalence of a disease in the population. Calculating the *PSP*, therefore, is of great value and provides information on how likely it is that a patient who is given a positive diagnosis actually has a disease.

MTL remind us that the probability of rejecting H_0 when H_0 is true (i.e., the probability of committing type 1 error) is not equal to the probability that the hypothesis H_0 is true when H_0 is rejected. Table 2 in MTL (2014, 286) shows, for example, that if $P(H_0)$ is known and equals 0.99, and $P(\text{test wrong} \mid H_0) = 0.05$, and $P(\text{test correct} \mid H_1) = 0.80$, then Bayes' theorem allows us to calculate the conditional probability $P(H_0 \mid D) = \frac{(0.05) \cdot (0.99)}{(0.05) \cdot (0.99) + (0.80) \cdot (0.01)} = 0.86$, which is the posterior probability that the null is true when the researcher rejects the null. Hence, the *PSP* states that there is only a 14 percent chance, given a statistically significant finding at the 5% level, that there is a true association. Moreover, this estimate is still far from the worst case that is presented. MTL calculate several *PSP*s under the assumption that the priors are in the interval $0.45 < P(H_0) < 0.99$. Based on the general impression from these calculations, MTL conclude that "it is

2. Hence α denotes the probability of a type 1 error, β denotes the probability of type 2 error, and $1 - \beta$ is the power of the test. In repeated random sampling α and β are the long-run frequencies of type 1 and type 2 errors.

3. A more sophisticated approach would require the specification of a prior distribution and not only the prior probability.

not unlikely that the *PSP* after the initial study is less than 0.5, as several plausible parameter combinations yield this result" (2014, 287). That is to say, the conjecture is that $P(H_0|D)$ is higher than 0.5. As mentioned, MTL (2014) suggest that a decision about whether to call an experimental finding noteworthy, or deserving of great attention, should be based on the Bayesian post-study probability since the Classical procedure is shown to have problems.

It follows immediately from Bayes' theorem that $P(D|H_0) \neq P(H_0|D)$. About 20 years ago, in *American Psychologist*, Jacob Cohen (1994) raised this issue in the context of null hypothesis significance testing. Cohen made the point that there could be a chance as low as 40 percent that the statistically significant finding represented a true association even though $P(\text{test wrong}|H_0) = 0.05$, i.e., at a 5% significance level. In the same journal, Galen Baril and Timothy Cannon (1995) replied that, instead of using fabricated data to illustrate how different the probabilities can be, that is, that $P(D|H_0) \neq P(H_0|D)$, it would be more informative to estimate how large the gap between the conditional and reversed conditional probabilities is *likely* to be. In his reply Cohen (1995) made clear that his example was not intended to model null hypothesis significance testing as used "in the real world" but rather to demonstrate how wrong one can be when the logic of null hypothesis significance testing is violated. In light of the claims in MTL (2014), there is a need to revisit the results in Baril and Cannon (1995).

The starting point in Baril and Cannon (1995) is that statistical power cannot be sufficiently good to detect all effect sizes. Assuming that the effect sizes follow a standard normal distribution centered at zero and that scientists only detect and consider effect sizes $|d| > 0.2$ as relevant (d is what is known as Cohen's effect size, i.e., it is the difference between means divided by the standard deviation), approximately 16 percent could be considered as equivalent to H_0 being true.[4] Baril and Cannon make use of an estimate from Joseph Rossi (1990) that the average statistical power for moderate effect sizes (i.e., $d > 0.2$) is 0.57. Finally, the conventional $P(\text{test wrong}|H_0) = 0.05$ is applied. Using Bayes' theorem, we now have: $P(H_0|D) = \frac{(0.05)\cdot(0.16)}{(0.05)\cdot(0.16)+(0.57)\cdot(0.84)} = 0.016$, that is, the *PSP* states that there is a 98.4 percent chance that the statistically significant finding will represent a true association. Such a statement would mean that the probability of H_0 being true given a significant test is 0.016, which is not very different from 0.05 which is, in turn, the probability of a significant test given that H_0 is true. Clearly, $0.016 \neq 0.05$, but still the conditional and reversed conditional probabilities are shown to be not very different once a parameter space different from that adopted in MTL

4. The point that economists *should* consider economic significance together with statistical significance is raised by McCloskey (1985). In case absolute substantive significance is hard to corroborate, Cohen's d statistic offers a relative measure that facilitates sample size planning and power analysis.

(2014) is adopted. The example also shows that the Classical significance test can be even more conservative than realized. Although it is possible that estimates (e.g., statistical power) are different in economic experiments compared to psychological experiments, using estimates from a related field can still be useful as a first approximation. Also note that even if we assume that the statistical power takes a considerably lower value of 0.20, the *PSP* then equals 0.95 which means that there is a 95 percent chance that the statistically significant finding will represent a true association. More crucial to our results is that we assumed that scientists are willing to consider economic significance instead of hunting only for statistical significance, such assumption affirming a norm about how to apply classical statistics.[5]

Remember that MTL assumed priors in the range of $0.45 < P(H_0) < 0.99$ to calculate *PSP*, a range that is obviously far off from the neighborhood of $P(H_0) \approx 0.16$, and they show that there, even in the absence of other biases such as research competition and research misconduct, the Classical framework leads to an "excessive number of false positives" (2014, 278) compared to what is stated in the significance level.[6] But MTL's conclusion that we should embrace the Bayesian framework seems exaggerated. The conclusion is based on this selective empirical support that only considers $0.45 < P(H_0) < 0.99$ and excludes the neighborhood of $P(H_0) \approx 0.16$, a neighborhood that is appreciated to be a more realistic estimate and that would change their main result.

At this point we have not even taken into account that the prior could be biased but instead we have postulated that it is a known, a postulation that is in line with the simulation in MTL (2014). But this should not go uncommented, because therein lies the real rub. Postulating that the unconditional probability is known facilitates assessment of the probability that a research hypothesis that is statistically significant is true. But this probability is feasible only in the Bayesian framework.

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5. To understand the need of such norm, consider an economic experiment with a control and an experimental treatment. As soon as the experimental treatment has a non-zero percent of subjects that behave differently in the experimental treatment, retrieving a statistically significant result is only a matter of choosing the right sample size. A non-zero threshold, e.g., $|d| > 0.2$, adds a constraint on substantive significance. Choosing an appropriate threshold is of course a non-trivial task.

6. MTL's conclusion that Classical statistics leads to an "excessive number of false positives" is reached under the definition that the benchmark probability of false positives is the probability that H_0 is true when H_0 is rejected. The significance level in Classical statistics on the other hand measures the probability to reject H_0 when H_0 is true (i.e., error of the first kind). Hence the claim that Classical statistics leads to an "excessive number of false positives" is another way to claim that there is a positive difference between the conditional and reversed conditional probabilities. Importantly, there is no "excessive number of false positives" if we apply the standard definition in Classical statistics that the probability of false positives is the probability of error of the first kind.

In medicine the aim is to find the conditional probability that an individual patient who is given a positive diagnosis actually has the disease, and the unconditional probability, that is, prevalence in the population, is considered to be known or available. For economic hypotheses, the unconditional probability $P(H_0)$ is hardly ever known. Bayesian statistics cope with this problem by assuming that the prior probability is a subjective belief, possibly mistaken, and subject to revisions.

This assumption, that the prior probability $P(H_0)$ is a possibly mistaken belief, facilitates a move from the Classical to a Bayesian framework, even when the prior is unknown. What is worth emphasizing is that based on a single experiment and using prior beliefs we do not necessarily estimate the unbiased $P(H_0 \mid D)$ in the Bayesian framework. Going back to the example of Baril and Cannon (1995), remember that the conditional probability was calculated to be $P(H_0 \mid D) = \frac{(0.05) \cdot (0.16)}{(0.05) \cdot (0.16) + (0.57) \cdot (0.84)} = 0.016$, and it was assumed that the unconditional probability is known and equals 0.160. Let us instead assume that the unconditional probability is unknown and that the subjective beliefs are that the prior corresponds to $P(H_0) = 0.99$. In this case, $P(H_0 \mid D) = \frac{(0.05) \cdot (0.99)}{(0.05) \cdot (0.99) + (0.57) \cdot (0.01)} = 0.897$. Hence, although $P(D \mid H_0) = 0.05$ is close to the correct benchmark of $P(H_0 \mid D) = 0.016$, the conditional probability based on subjective beliefs is considerably higher, namely at $P(H_0 \mid D) = 0.897$. The example demonstrates that it is easy to come up with counterexamples to MTL's (2014) simulation and thereby show that the Bayesian framework does not necessarily perform better than the Classical framework, and might even perform worse, in estimating $P(H_0 \mid D)$. In the example above, the *PSP* calculation underestimates the probability that a statistically significant research finding is true.[7]

The conceptual difference between the Classical and Bayesian frameworks regarding prior beliefs about $P(H_0)$ also deserves to be mentioned. In Classical statistics a probability is the long-run relative frequency, while in the Bayesian framework a probability is the degree of the belief. Although posterior $P(H_0 \mid D)$ undeniably has an appealing interpretation, it is only available through Bayes' theorem, which R. A. Fisher rejected with the motivation that it requires one to "regard mathematical probability not as an objective quantity measured by observable frequencies, but as measuring merely psychological tendencies, theorems respecting which are useless for scientific purposes" (Fisher 1937, 7).

7. By incorporating subjective beliefs into the inference process, the risk of introducing errors or biases that would not otherwise be present is inevitable. On the other hand, the Bayesian approach is particularly useful when one has strong prior knowledge of a situation and wants to summarize the accumulated evidence.

Although Fisher's position may be perceived as extreme, I mention it to place the difference between the Classical and Bayesian approach in an historical context.

Conclusions

Based on what is presented in Maniadis, Tufano, and List (2014), the conclusion that only a Bayesian analysis provides "proper inference" seems exaggerated. The assumption that the unconditional probability $P(H_0)$ is known[8] implies that the Bayesian approach can only be better but never worse than the Classical approach in their simulation. Once we relax this assumption by allowing for subjective beliefs, it is no longer trivial to decide whether the Classical or the Bayesian framework is better. MTL combined the assumption that the unconditional probability is known with a selective empirical setup that also favors the Bayesian framework by excluding many instances where the problems of the Classical approach are small. Such moves do, of course, make the simulation in MTL (2014) great for demonstrating the pitfalls of the Classical framework.

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8. While MTL (2014) make use of different values of $P(H_0)$ to calculate the difference between conditional and reversed conditional probabilities, in each calculation it is assumed that $P(H_0)$ is known (unbiased), which makes the Bayesian approach into the benchmark from which any observed deviations under the Classical approach are interpreted as bias.

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About the Author

Mitesh Kataria is senior researcher in the Strategic Interaction Group at the Max Planck Institute of Economics, Jena, Germany, and associate senior lecturer in the Department of Economics at the University of Gothenburg, Gothenburg, Sweden. His current research concerns are in applied (environmental and welfare) economics, empirical behavioral economics, and experimental economics. His email address is mitesh.kataria@economics.gu.se.

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One Swallow Doesn't Make a Summer: Reply to Kataria

Zacharias Maniadis[1], Fabio Tufano[2], and John A. List[3]

LINK TO ABSTRACT

In his comment, Mitesh Kataria (2014) makes three main points about a specific part of our paper (Maniadis, Tufano, and List 2014), namely about Tables 2 and 3. In our paper, we employ these tables in order to illustrate the idea that very inconclusive post-study probabilities that a tested phenomenon is true may result from novel, surprising findings. The main arguments in Kataria (2014) are the following:

> First, if $P(H_0)$ is unknown, as is often the case with economic applications, the post-study probability can lead to even worse inference than the Classical significance test, depending on the quality of the prior. Second, the simulation in Maniadis et al. (2014) ignores previous assessments of $P(H_0)$ and instead utilizes a selective empirical setup that favors the use of post-study probabilities. … [Third,] contrary to what Maniadis et al. (2014) argue, their results do not allow for drawing general recommendations about which approach is the most appropriate. (Kataria 2014, abs.)

We believe that our work might have been misunderstood by Kataria. Moreover, it seems that some of his claims are not supported by relevant empirical evidence.

1. University of Southampton, Southampton SO17 1BJ, UK.
2. University of Nottingham, Nottingham NG7 2RD, UK.
3. University of Chicago, Chicago, IL 60637.

In Maniadis, Tufano, and List (2014), our basic aim is to draw on the general problem of the credibility crisis in disciplines other than economics (Ioannidis 2005; Bettis 2012; Jennions and Moller 2002), and to convey the disquieting news to economists by relying on insights and tools from the life sciences literature. While conveying the troubling news, we also emphasize the good news that usually it takes only a few independent replications to advance considerably the credibility of empirical exercises. We wish to understand how confident one should be in the published empirical findings in economics. Simply put, we are not discarding classical significance testing, just arguing that we should be interpreting it accurately. For an educated assessment of the empirical evidence we need to know not just whether tests were significant but also the value of key variables such as research priors and statistical power. Admittedly, these variables are not easy to estimate, and in economics it is often, even typically, the case that there is not much relevant evidence. But this is exactly our point: We wished to show that if we wish to assess how confident we are in our findings, evidence is lacking in critical dimensions. Given the recent evidence pointing to non-replicability in several life sciences (Ioannidis 2012), such lack of evidence may cause serious questions to be raised about economics as well (see Ioannidis and Doucouliagos 2013; Alexander 2013).

Whereas Kataria claims that "for economic hypotheses, the unconditional probability $P(H_0)$ is hardly ever known" (Kataria 2014, 8), we suggest that the issue of such knowledge accumulation needs to be regarded as endogenous. If the investigator's frame of analysis disregards the variable $P(H_0)$, there is no need to estimate it. Other disciplines have developed meta-analytic methods that can be fruitfully employed in economics for estimating the relevant variables (Cooper, Hedges, and Valentine 2009). Replication has a key role in these methods.

To encourage such a structured approach, we illustrated with Tables 2 and 3, using Bayesian language, the fact that we should be cautious of new evidence and—as we argue later in Maniadis, Tufano, and List (2014)—that we should also increase our efforts to replicate original studies. We clearly note in the paper that the combinations of parameter values used in Tables 2 and 3 should be thought of as applying to novel and surprising findings (Maniadis, Tufano, and List 2014, 278, 286 n. 27). So these combinations were *truly selected* to illustrate what happens in the case of such findings. Moreover, we acknowledged the difficulty of pinpointing those combinations exactly (ibid., 286). Essentially, the degree to which our discipline is characterized by such combinations of priors and power is an empirical question. We hope that the message of the tables itself will encourage work on this underexplored question. Once more, we view as one of our key messages that we lack sufficient evidence to evaluate the credibility of much work in our field. We join others in prompting economists to grapple with such questions as: What is

a reasonable estimate for the typical prior in each subfield of economic research? What is the typical power of a research study? How common is replication in economics and how common should it be?

Given the scarcity of relevant empirical studies, we find the particular configurations suggested by Kataria (2014) somewhat unsupported by the evidence. In particular, there seems to be no empirical foundation for the claims that "effect sizes follow a standard normal distribution centered at zero and…scientists only detect and consider effect sizes $|d| > 0.2$ as relevant" (Kataria 2014, 6). Despite this, Kataria claims that "the neighborhood of $P(H_0) \approx 0.16$ … is appreciated to be a more realistic estimate" (ibid., 7). Estimating $P(H_0)$ is a difficult empirical question that would require much more research. With respect to power, Kataria mentions evidence from the related field of psychology, namely Joseph Rossi (1990), who estimated that the average power for medium effect sizes is equal to 0.57. However, it is not clear on which evidence the assumption of medium effect sizes is based. Furthermore, more recent evidence reveals that typical power in psychology is about 0.35, even if we assume that the average effect size $|d|$ is equal to 0.5 (Bakker, van Dijk, and Wicherts 2012).

The spirit of our paper is to encourage work such as the very recent paper by Le Zhang and Andreas Ortman (2013). They retrospectively estimated the power of several experimental designs reported in Christoph Engel's meta-analysis of dictator games (Engel 2011), and they found that the median level of power was less than 0.25. It is important to note the critical role of meta-analysis for generating this piece of new evidence. The point is not to argue in the absence of evidence but to try to accumulate the necessary evidence. As economists, we hope that our field is very credible, but we need to provide empirical evidence using the relevant tools.

At this point we need to acknowledge the important issue of "previous assessments of $P(H_0)$," although Kataria mentioned it without justification. As we said in Maniadis, Tufano, and List (2014), we aimed to make a claim about novel, surprising results. We do believe that many types of economic research are more grounded in theory than research in other social sciences, so for them "surprising" results may not be as important for publication. In fact, Brad DeLong and Kevin Lang (1992) found that $P(H_0)$ is very close to zero for a set of hypotheses published in top economic journals in the 1980s. If their interpretation—that the referee process somehow manages to filter true associations—is correct, that would be reassuring for the credibility of the economics profession. As DeLong and Lang (1992) acknowledge, however, there are alternative interpretations for their findings, such as the existence of selection issues and data mining in the discipline, so their optimistic interpretation should be taken with caution. There is a need for further research on the matter, following the seminal analysis of DeLong and Lang

(1992). We are particularly interested in the field of experimental economics, where we worry that "surprising" findings might be more frequently published.[4]

From the previous arguments it should be clear that in Maniadis, Tufano, and List (2014) we did not put forward any general recommendation about which inference approach, Classical or Bayesian, is the most appropriate. In fact, in the context of the current "publish or perish" culture (see, e.g., Fanelli 2010) and the related structure and incentives of the economics knowledge system (Oswald 2007; Glaeser 2008; Young, Ioannidis, and Al-Ubaydli 2008), we merely resort to Bayesian language to argue in favor of a much more careful interpretation of Classical inference.

Summing up, we believe that studying systematically the factors that affect the credibility of empirical findings might have an important role to play in economics. Meta-analysis and Bayesian tools are of central importance for conceptualizing the problem and quantifying key variables, and should not be ignored by economists. Our point was not to argue in favor of a specific configuration of parameter values, but to show that we cannot ignore factors such as priors and power, because if we do, something can go very wrong with economic research.

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4. We would welcome more empirical evidence on this and related issues.

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About the Authors

Zacharias Maniadis is Assistant Professor of Economics at the Economics Division, School of Social Sciences, University of Southampton. His research interests include microeconomics, experimental economics, behavioral economics, and political economy. His email address is Z.Maniadis@soton.ac.uk.

Fabio Tufano is Assistant Professor of Economics at the University of Nottingham. His research interests include psychology and economics, experimental economics, and game theory. His email address is fabio.tufano@nottingham.ac.uk.

John A. List is Homer J. Livingston Professor in Economics and the College at the University of Chicago. His research interests include experimental economics, field experiments, education, youth violence, economics of charity, environmental economics, experiments in firms, multi-unit auctions, and neuro-economics. His e-mail address is jlist@uchicago.edu.

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Should the Modernization Hypothesis Survive Acemoglu, Johnson, Robinson, and Yared? Some More Evidence

Hugo J. Faria[1], Hugo M. Montesinos-Yufa[2], and Daniel R. Morales[3]

LINK TO ABSTRACT

Seymour Martin Lipset (1959, 75) wrote, "The more well-to-do a nation, the greater the chances that it will sustain democracy." Lipset (1959; 1960) is frequently interpreted as having advanced "the modernization hypothesis," a claim that income and education are, in a statistical sense, predictors of democracy.[4] This claim is supported by a vast empirical literature spanning several decades in the areas of comparative politics and political economy,[5] although some literature has

1. Instituto de Estudios Superiores de Administración, Caracas, Venezuela; Barry University, Miami Shores, FL 33161. We wish to thank Jim Gwartney and two anonymous referees for very helpful comments.
2. Universidad Simón Bolívar, Caracas, Venezuela; Instituto de Estudios Superiores de Administración, Caracas, Venezuela.
3. Barna Business School, Santo Domingo, Dominican Republic.
4. The modernization hypothesis refers to changes in social and political institutions induced by development. These changes include the demographic transition, political development, secularization, increased urbanization, diminished share of agriculture in total GDP, and greater levels of education and life expectancy, among others (see McCleary and Barro 2006a; 2006b). Paldam and Gundlach (2012; 2013; also Gundlach and Paldam 2009) encompass these cultural, social, political, economic, and demographic transformations under the rubric of the Grand Transition. Accordingly, we are studying a subset of the several transitions associated with development, namely the transition from autocracy to democracy. Precursors of the modernization literature include Aristotle (in his *Politics*) on political development or democratization, and Hume (1757) and Marx (1859) on secularization.
5. *See* Barro (1999); Boix and Stokes (2003); Bollen (1980); Burkhart and Lewis-Beck (1994); Glaeser, Ponzetto, and Shleifer (2007); Inglehart (1997); Jackman (1973); Londregan and Poole (1996); McCrea and Cnudde (1967); and Muller (1988), among others.

challenged the claim that higher levels of income induce democratic transitions.[6]

The modernization hypothesis, understood as political development, is investigated in several papers by Daron Acemoglu, Simon Johnson, James Robinson, and Pierre Yared (2005; 2008; 2009), a team of authors hereafter referred to as AJRY. Using mainly panels of countries spanning the period 1960–2000, they find no correlation between income and democracy after controlling for country specific factors and world trends, that is, after allowing for country and time effects, and likewise for education and democracy. AJRY (2009) interpret their country fixed effects results as being consistent with the critical junctures hypothesis.[7] The fixed effects, AJRY say, are "capturing the impact of time-invariant, historical variables simultaneously affecting the evolution of income and democracy" (AJRY 2009, 1057). Put differently, fixed effects proxy for country-specific differences in institutional quality that ultimately account for the observed correlation between income and democracy.

Econometric specifications used in the 2008 and 2009 papers by AJRY always include among the independent variables income and a proxy for democracy, both lagged, and in the 2005 paper they always include education and democracy, also both lagged. The democracy variable measures quality of *political* institutions, but AJRY do not control for *economic* institutions. More specifically, AJRY do not include a variable to control for the level of inclusiveness of economic institutions.

Income is at least to some extent a result of the interplay between economic and political institutions. In our view, economies tend to grow if political and economic institutions induce a stable environment where private property of the vast majority of the population is protected, creating incentives to work, innovate, invest, and allocate resources efficiently.[8] We strive to incorporate such mechanisms in comparative political development research by including, in addition to democracy and income, an index of economic freedom as a proxy for

6. Przeworski and Limongi (1997) and Przeworski, Alvarez, Cheibub, and Limongi (2000) contend that their evidence is not favorable to what they call the "endogenous" version of the modernization hypothesis. However, criticisms by Boix and Stokes (2003), Inglehart and Welzel (2005), and Epstein et al. (2006) contend that by using correct standard errors to estimate significance levels, incorporating new evidence whereby the ratio of regime switches to democracy against regime switches to autocracy increases, and expanding the sample back to 1850, there is clear support for the modernization hypothesis. See also Voigt (2011) for a brief discussion on the democracy causality issue.

7. Exponents of the critical junctures hypothesis are Moore (1966) and O'Donnell (1973).

8. Jong-A-Pin and De Haan (2008; 2011) report episodes of growth acceleration which are preceded by economic liberalizations. Additional evidence supportive of a beneficial effect of democratic institutions on growth is uncovered by Mobarak (2005), who finds that democracies enhance growth through the channel of reduced volatility given the inverse relation between political development and volatility. See also Persson and Tabellini (2009) for the role of democratic capital in stimulating growth by enhancing democracies' stability.

capitalist institutions which are crucial for development along with human capital.[9] A central indicator of economic freedom is quality of the legal infrastructure, in particular extent of the rule of law and independence of the judiciary, both of which can in the spirit of Lipset be interpreted as proxies for some social requisites for political development.

AJRY's basic results hinge on specifications which control for lagged democracy and country and time fixed effects, and in this context of limited residual variability they attempt to assess if income exerts an independent effect on democracy, and likewise for education.[10] Needless to say this problem of little variance of democracy left to be explained by income or education is not mitigated and can be aggravated by the inclusion of economic institutions to the extent that economic freedom impacts democracy.[11]

Moreover, economic freedom and income are highly correlated, as are economic freedom and education. The collinearities between income and economic freedom and between education and economic freedom, like the inclusion of economic freedom, reduce the likelihood of uncovering a statistically significant impact of income and/or education on democracy. Thus we are stacking the cards against the modernization hypothesis, and in this sense our tests are more demanding than those performed by AJRY.

A final reason for including economic freedom is that a research strand in the economics literature argues that economic freedom is a necessary condition for political freedom.[12] Economic freedom may be an important channel in explaining democracy that has gone missing in the modernization literature.

Another trait that distinguishes this paper from those of AJRY is methodological. Part of our empirical strategy is the application to our sample of the System Generalized Method of Moments estimator developed by Richard Blundell and Stephen Bond (1998). The System GMM estimator is particularly suited for identification tasks where the variables are highly persistent, which is the case with

9. For recent supportive evidence see Ashraf and Galor (2013), and for a fresh summary of the literature on deep determinants of economic development and the role of institutions see Spolaore and Wacziarg (2013).

10. To better appreciate the issue of reduced variability left to be explained by income, see Benhabib, Corvalan, and Spiegel (2011), who document that in democracy regressions using country five-year panels, the inclusion of lagged democracy along with country and time fixed effects accounts for 81% of total variation of the democracy variable, leaving little variability to be explained by income. A similar point is made by Paldam and Gundlach (2012, 164 n. 21): "This empirical model [referring to the specification used by AJRY (2008)] leaves virtually nothing to be explained by income, and consequently the effect of income becomes insignificant, and is declared spurious."

11. Indeed, components of economic freedom such as rule of law can promote democratization.

12. Among early proponents of this research strand are Friedman (1962) and Hayek (1944). The view has found recent empirical support in Lawson and Clark (2010).

income, education, and democracy.

We apply System GMM techniques to an unbalanced panel of countries spanning the 1970–2010 sample period,[13] using quinquennial data and after controlling for economic freedom and democracy. We find that education and income predict democracy. Also, applying OLS to our data set and using a specification that captures long-run changes in democracy, we obtain results that support the modernization hypothesis.

The research in this paper is related to a number of recent studies, some of them motivated by the papers by AJRY (2005; 2008; 2009). First and perhaps the closest to ours, is a paper by Benedikt Heid, Julian Langer, and Mario Larch (2011), which finds support for the modernization hypothesis using the System GMM technique. But unlike Heid, Langer, and March (2011), we control for economic institutions and address the role of education as a driver of modernization.

Jess Benhabib, Alejandro Corvalan, and Mark Spiegel (2011) report evidence favorable to the modernization hypothesis after employing panel nonlinear estimation methods that account for censored democracy data. As previously mentioned we find support for the modernization hypothesis using linear estimation methods, also used by AJRY (2005; 2008; 2009). However, our results rely on the Blundell-Bond System GMM estimator which is employed by neither AJRY (2005; 2008; 2009) nor Benhabib, Corvalan, and Spiegel (2011). Further, education is treated by Benhabib et al. (2011) as another covariate in addition to income in their main specification. We also perform regressions displaying horse races between income and education. In line with Lipset (1959; 1960) we attempt to evaluate education's predictive power of democracy independently of income.[14] Consequently, we present regression specifications containing income but excluding education and, symmetrically, specifications that include education and exclude income.[15]

Carles Boix (2011) argues that AJRY's results are partly driven by the post-WWII sample period in which the effect of income on democracy is particularly weak. He finds support for modernization in long-run panels that use fixed effects spanning eighty or more years. By contrast, we find support for the Lipset hypothesis using a sample that focuses on recent decades commencing in 1970 and ending in 2010.

Eric Gundlach and Martin Paldam (2009) employ the Polity index as a proxy for democracy and use a sample that spans the period from 1820 to 2003.

13. Thus, similar to AJRY, we focus on a recent sample.

14. Lipset (1959) viewed education as a necessary condition for democracy.

15. *See* Glaeser, Ponzetto, and Shleifer (2007) for a theoretical development in which education is modeled as having a causal impact on democracy.

Estimating OLS and Two-Stage Least Squares cross-country regressions for each of the 184 years intervening between 1820 and 2003, Gundlach and Paldam find evidence that buttresses the democratic transition view. These scholars use this long-run procedure because in their view five-year panels offer a horizon too short to test the democratic transition hypothesis. Gundlach and Paldam write: "The Grand Transition view and the Democratic Transition hypothesis are about long-run trends that can be best handled by pure cross-section estimates, not by a combination of fixed effects and lagged adjustment over a short time horizon" (2009, 349-350).[16] Nonetheless and as previously indicated, we find support for the modernization thesis using panels with a five-year frequency.

Paldam and Gundlach (2012) use the Gastil index as a proxy for democracy. They apply country and time fixed effects in a balanced panel of countries spanning the 1972–2008 period with frequencies of 18, 12, and five years. They find support for the modernization hypothesis using five-year panels and restricting the sample to the pre-1989 period.[17] Like Gundlach and Paldam (2009), Paldam and Gundlach (2012) do not control for economic institutions,[18] and they do not use dynamic specifications such as were employed by AJRY. Nonetheless, after applying OLS and IV methods to long-run cross-country specifications, both papers report strong support for the modernization hypothesis.

Daniel Treisman (2012) provides evidence which suggests that the impact of development on democracy takes place over a 10- to 20-year time span. The finding is particularly strong after 19th-century data is included. Treisman writes: "The new point I emphasize here is that the link between income and democracy is clearest and strongest *in the medium to long run*—i.e. panels of 10 to 20 years" (2012, 7, emphasis in original). Moreover, similar to Boix (2011) and to AJRY, Treisman (2012) reports that over the 1960–2000 period income does not predict democracy in panels of one-, five-, 10-, 15- and 20-year frequencies. However, Treisman does not apply Blundell-Bond methods to any sample period.

Ghada Fayad, Robert Bates, and Anke Hoeffler (2012) applied a Pooled Mean Group estimator (PMG), augmented with averages of all variables in the model to proxy for time-common factors, to a sample of countries with obser-

16. A similar argument is articulated by Paldam and Gundlach (2012, 152), who interpret their Granger causality test results as revealing "that the short to medium run is probably not well suited to identifying the main direction of causality between income and democracy."

17. Unfortunately, Paldam and Gundlach (2012) do not indicate if the estimated standard errors used to assess the statistical significance of regression coefficients are robust to the presence of arbitrary heteroskedasticity and/or autocorrelation, or are clustered by countries. Reporting the type of standard error is relevant because the significance test may be invalid and the estimated p-value may change depending on the type of standard error used.

18. *See* Gundlach and Paldam (2009) for a justification of institutions-free analyses of democratic transition.

vations that commence in 1955 and end in 2007.[19] They find that income is negatively and significantly related to democracy. Parameter estimates associated with world income and world democracy enter positively and significantly, predicting greater democratization at the country level. Figure 1 in Fayad, Bates, and Hoeffler (2012, 5) graphs world democracy and world per capita income starting in 1960 and ending in 2008 for a sample of 105 countries. World income mostly rises over the sample period whereas the democracy index falls during 15 consecutive years from 1960 through 1975. Yet, over the following 33 years the world democracy index rises along with income. Thus the Fayad, Bates, and Hoeffler (2012) Figure 1 is generally consistent with the modernization hypothesis.

Furthermore, the heterogeneous PMG estimator used by Fayad, Bates, and Hoeffler (2012) estimates individual country coefficients thus requiring long time series for each country included and excluding countries with a time-invariant dependent variable.[20] Due to this long time series requirement, countries that transition to democracy such as the Czech Republic, Estonia, Latvia, Lithuania, and Slovak Republic are not included in the sample. Among the time-invariant consistent democracies excluded are Australia, Austria, Belgium, Canada, Denmark, Finland, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Sweden, Switzerland, United Kingdom, and United States. Among the consistent autocracies excluded are Cuba, Libya, and Vietnam. Fayad, Bates, and Hoeffler (2012, 14) write: "Both the sample choice and the methodology thus led us to our results." In other words, the methodology constrains the sample, leaving out potential important sources of information.

Additionally, Fayad, Bates, and Hoeffler show in Table 2 (2012, 11) that, both for their sample of 105 countries and for AJRY's sample, OLS fixed effects estimates of income per capita are insignificant only when conditioning on year fixed effects. As previously mentioned, the PMG estimator does not allow for year effects.

The OLS Pooled Error Correction Model (PECM) admits controls for country and time fixed effects, however. The results of Fayad, Bates, and Hoeffler's main sample using OLS (PECM) are shown in their Table 5 (2012, 14). In three out four different lag structure models, income per capita at the country level enters

19. The PMG estimator does not allow for year fixed effects because parameters are estimated separately for each country. To correct for this shortcoming of the PMG methodology, the Fayad et al. (2012) model is augmented with world income and democracy.

20. In other words, regression coefficients are calculated for every country in the sample, as opposed to, say, OLS, which estimates one slope coefficient for all the countries. The PMG estimator by design eliminates time-invariant dependent variables. This is not so extraordinary (e.g., the fixed-effect methods employed by AJRY also by design discard time-invariant variables, which appear frequently among explanatory variables).

significantly negative, albeit only at a 10% significance level. They write: "However, estimating the pooled error correction model while using the AJRY (bigger) sample yields long-run coefficients on income per capita that are insignificant, regardless of the number of lags" (ibid., 13).

Interestingly, when Fayad, Bates, and Hoeffler apply the PMG estimator without accounting for their proxies for time effects, "the coefficient on income per capita is instead positive and significant" (2012, 12 n. 11). Overall, this evidence they offer may lead one to suspect that their results are also sensitive to the methodological procedure due to the technical impossibility of controlling for year effects using annual dummies when the PMG method is employed.

In light of the findings of Treisman (2012) and particularly Boix (2011), a potentially interesting robustness check of the Fayad, Bates, and Hoeffler (2012) findings may be to expand the sample coverage to earlier periods. This extension may alleviate sample attrition containing relevant information.[21]

Data attributes and sources

Our sample period comprises the years 1970 through 2010. In this regard our paper follows a common practice in the democratization literature of using data less afflicted by measurement problems than data prior to the Second World War.

We pool cross-section data with time series data in order to exploit the time dimension of the data, allowing us to investigate the impact over time of variables which proxy for socio-economic development, such as real income and human capital, on democracy. Specifically, exploiting the within-country variation in the data permits us to evaluate whether, as a country becomes more socio-economically developed, relative to its mean, it also turns out to be relatively more democratic.

Our dependent variable and proxy for democracy measures is the Index of Political Rights from Freedom House published in 2010. In the Index, "political rights" include the existence of free and fair elections, competitive parties, an opposition that plays an important role in the political process, and whether those who are elected rule, among others. "Political rights are rights to participate meaningfully in the political process. In a democracy this means the right of all adults to vote

21. A recent paper that addresses the relation between education and governmental quality and, indirectly, the relation between education and democracy is Botero, Ponce, and Shleifer (2013). As they write: "Most studies find that education and development lead to improved government (e.g., Barro 1999, Glaeser et al. 2004, Bobba and Coviello 2007, Castello-Climent 2008, Murtin and Wacziarg 2011), although some disagree (Acemoglu et al. 2005). In this paper, we ask *why* the quality of government improves with education and development, assuming that it does" (Botero et al. 2013, 2).

and compete for public office, and for elected representatives to have a decisive vote on public policies" (Gastil 1991, 7).[22] The Index of Political Rights goes from one to seven, where one indicates most politically free and seven least free.

Our independent variables are lagged democracy, log of real income per capita, human capital, and the Economic Freedom of the World (EFW) index. Real income per capita is provided by the World Development Indicators published in 2010 by World Bank. Human capital is provided by Robert Barro and Jong-Wha Lee (2010)[23] and measures average years of education of the population 25 years and older. The EFW index—inspired by Milton Friedman, built over the years since 1997 by James D. Gwartney and Robert Lawson, and published by the Fraser Institute—is our proxy for capitalism. The EFW index contains the following areas: (1) "Size of Government: Expenditures, Taxes, and Enterprises"; (2) "Legal Structure and Security of Property Rights"; (3) "Access to Sound Money"; (4) "Freedom to Trade Internationally"; and (5) "Regulation of Credit, Labor, and Business" (Gwartney, Lawson, and Hall 2011). Thus the index controls for trade, inflation, regulation, government spending, taxes, rule of law, and quality of the judiciary. The ratings for the components of the EFW index range from zero to ten with higher ratings indicating more economic freedom. The summary ratings are an aggregation of the five area ratings, and they almost always fall within a range between three and nine.

Empirical strategy and results

Using quinquennial panels and an unbalanced panel of countries from 1975 to 2010 we estimate the following regression model:[24]

$$PF_{i,t} - PF_{i,t-1} = \alpha \cdot PF_{i,t-1} + \beta \cdot EFW_{i,t-1} + \theta \cdot Y_{i,t-1} + \tau \cdot HK_{i,t-1} + \delta_i + \mu_t + \varepsilon_{it} \quad (1)$$

where change in political rights[25] is regressed against lagged political rights to capture persistence in democracy and also potentially mean-reverting dynamics. The main parameters of interest are θ associated with initial-period income, and τ associated with initial-period human capital. Specification (1) allows for country fixed effect dummies, with δ_i to control for country idiosyncratic time-constant factors, and for time period dummies, with μ_t to control for world trends in

22. The late Raymond Gastil directed Freedom House from 1977 to 1988 and made a decisive contribution to its indexes on political rights and civil rights, which are now published yearly.
23. This metric of human capital updates the Barro and Lee (2000) data set and corrects for measurement errors in educational attainment and takes account of criticisms made by Cohen and Soto (2007).
24. This functional form is used by Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004).
25. $PF_{i,t}$ stands for level of political rights in country i at year t.

democracy. Finally, $\varepsilon_{i,t}$ is a zero-mean error term, which captures the variation in democracy not explained by model (1).

Results shown in Table A indicate that, applying fixed-effects OLS over non-overlapping five-year periods comprising thirty-five years, initial-period (lagged) political rights (in columns 1 and 2) enter negative and statistically significant at a 1% level, suggesting the presence of mean reversion. Controlling for income (column 1) and human capital (column 2), the regression coefficient associated with economic freedom is negative and significant at a 5% level, consistent with the view that higher levels of economic freedom induce more democratic change. Income (column 1) enters significantly though with a positive sign, inconsistent with the modernization hypothesis implying that development leads to less democracy, whereas human capital (in column 2) does not predict democracy at a 5% significance level.

Column (3) presents our first horse race results between income and education. Human capital does not predict democracy. Income, however, enters significantly predicting *less* democracy.

According to AJRY, conditioning on fixed effects captures the spirit of the critical junctures hypothesis to the extent that it accounts for the effect of unobserved heterogeneity associated with time-invariant historical factors impacting both political and economic development. In their sample, income loses significance controlling for fixed country effects and time dummies, which is consistent with the critical juncture hypothesis. However, our results indicate that economic freedom predicts more democracy and income predicts less democracy. This evidence suggests that their democracy-income effects are sensitive to the presence of economic freedom in the model.[26]

While fixed-effects estimation methods correct for biases induced by the omission of a complete list of country-specific unobserved heterogeneity variables correlated with the independent variables, parameter estimates are inconsistent if time-varying independent variables correlated with explanatory variables are omitted, violating consequently the strict exogeneity assumption. Moreover, fixed-effects estimates in dynamic specifications are biased, and in short time-period panels inconsistent,[27] due to the correlation between the transformed lagged

26. In our sample and using our functional form that regresses changes in democracy over five-year periods against initial-period democracy and income, and also allowing for time and country fixed effects but not controlling for economic freedom, income enters significantly at a 5% level predicting *less* democracy. These results, which are available upon request, are also at odds with the tenets of the modernization hypothesis.

27. However, these estimates become consistent as country time observations increase. More precisely, the fixed-effect estimator is consistent as T increases assuming both that there is no other source of correlation between lagged democracy and the error term and that remaining regressors are strictly exogenous (see Wooldridge 2002).

dependent variable and the transformed unsystematic error term $\varepsilon_{i,t}$ inherent to the time-demeaned transformation. To overcome inconsistency of the fixed-effect estimator, and following AJRY, we apply the Difference Generalized Method of Moments (GMM) estimator proposed by Manuel Arellano and Stephen Bond (1991). In addition, we also apply the System GMM estimator introduced by Blundell and Bond (1998) to the following dynamic specification:[28]

$$PF_{i,t} = \alpha' \cdot PF_{i,t-1} + \beta \cdot EFW_{i,t-1} + \theta \cdot Y_{i,t-1} + \tau \cdot HK_{i,t-1} + \delta_i + \mu_i + \varepsilon_{it} \quad (2)$$

where the dependent variable is the level of political rights for country i in period t.

Columns (4) through (9) of Table A present Arellano-Bond estimates. Economic freedom enters significantly and negative in column (4), whereas income enters significant at a 10% level and positive, thus with a sign at odds with Lipset's hypothesis. In column (5) controlling for time effects, economic freedom loses significance, the p-value being 0.103, and income again enters significantly at a 5% level but with the 'wrong' sign. In columns (6) and (7) we substitute human capital for income, and only lagged political freedom enters significantly. Horse race results between human capital and income using Arellano-Bond are presented in column (8) not controlling for time effects and in column (9) controlling for time effects. Lagged income enters significantly at a 10% level in column (8) and at a 5% level in column (9). However, in both cases income predicts less democracy. Human capital again does not predict democracy at conventional levels of significance.

Moreover, Sargan tests suggest that none of the models that apply Arellano-Bond methods are correctly specified. These results are qualitatively similar to those obtained by AJRY (2005; 2008; 2009) in that predictors of the modernization hypothesis, education and income, either enter not significantly or, if significantly, show up with associated regression coefficients bearing the wrong sign.

The Arellano-Bond estimator is based on the following moment conditions: $E(PF_{i,t-s}\Delta\varepsilon_{it}) = 0$ for $t \geq 3$ and $s \geq 2$. It is well known, however, that democracy, education and income are highly persistent variables,[29] and therefore instruments in levels are poorly correlated with first differences.[30] This low correlation originates a weak-instrument problem aggravating finite sample biases. To enhance

28. The estimating equations (1) and (2) are equivalent. Specification (1) is obtained subtracting lagged democracy on both sides of (2).

29. See, for example, Glaeser, Ponzetto, and Shleifer (2007) and Bobba and Coviello (2007).

30. To understand the poor correlation between the instrument in levels and subsequent differences when the series is highly persistent, consider a simple autoregressive process of order one (AR(1)), e.g., $PF_{it} = \alpha PF_{i,t-1} + \varepsilon_{it}$. Subtracting $PF_{i,t-1}$ from both sides, to transform this process in differences, yields $\Delta PF_{it} = (\alpha - 1)PF_{i,t-1} + \varepsilon_{it}$. The closer the value of α to 1 (the higher the persistence), the lower the correlation between ΔPF_{it} and $PF_{i,t-1}$, that is, between the difference and the level.

precision of the point estimates, the Blundell-Bond System GMM estimator employs simultaneously the equation in levels and the equation in first differences, conforming to a system of equations which uses lagged differences as internal instruments for the equations in levels and lagged levels as instruments for the equation in differences. Thus, the procedure allows us to exploit additional overidentifying moment restrictions that may contribute to overcome the weak-instrument problem. These additional moment restrictions use internal instruments in differences which are assumed to be orthogonal to the country fixed effect plus the zero mean error term.[31]

In Table A, the Columns (10), (11), and (12) report Blundell-Bond estimates. The parameter estimate associated with income in column (10) is significant at a 5% level, with a p-value of 0.018, and the estimate is negative, suggesting that development predicts democracy. According to column (11) the point estimate of education is also negative and significant at a 5% level with a p-value of 0.018, indicating that education predicts democracy. Both specifications, used in columns (10) and (11), condition on time effects and in neither do the regression coefficients associated with economic freedom enter significantly. Further, according to the Hansen over identification test and second order autocorrelated disturbances in the first differences equations, AR (2), we fail to reject the Hansen test's null hypothesis that the instruments are valid and also the null hypothesis of no second-order autocorrelation.[32]

Column (12) contains results of the horse race between income and education. Neither income nor education enters significantly. In fact, only the coefficient estimate of lagged democracy, the autoregressive variable, enters significantly.

Summing up the results of our horse races using high-frequency panels: parameter estimates associated with education do not enter significantly, and in the cases where income parameter estimates entered significantly they show up with the 'wrong' sign. Similarly non-instructive results are reported by AJRY (2005, 46) in their Table 1. This lack of meaningful results should not be surprising given the aforementioned low residual variability to be explained by income or education. The lowness of the residual variability stems from the inclusion of lagged democracy, economic freedom, time and fixed effects (see footnote 10). This

31. This is not to say that the Blundell-Bond estimation technique cannot be afflicted by weak instruments; see Bazzi and Clemens (2013) for cases in the economic growth literature. Alas, extant econometric methods do not provide standard tests to detect weak instruments in dynamic panel GMM settings.

32. Interestingly, when applying Blundell-Bond methods to specifications that do not control for economic freedom, income does not predict democracy, which underscores the appropriateness of our basic specification that includes economic freedom. These results are available from the authors upon request.

problem is exacerbated by the simultaneous inclusion of both of the democracy predictors, education and income, which are highly positively correlated.

Finally, columns (13), (14), and (15) of Table A evaluate the predictive power of income, human capital, and economic freedom on political rights over a longer time span and using traditional OLS methods. We use a specification where the dependent variable is the change in political freedom over 35 years from 1975 to 2010 and the independent variables are the initial values of political rights, economic freedom, income, and/or human capital.

Economic freedom in 1975 does not predict a change in political rights over the long run controlling for income (column 9) and human capital (column 10) in the year of 1975. By contrast, parameter estimates associated with income and human capital show up as highly significant in columns (9) and (10) respectively. Regression coefficient estimates are precisely estimated and appear with the hypothesized sign. The negative sign of 1975 political rights suggests the presence of mean reversion.

These findings are reassuring because they suggest that our results based on shorter time spans are not driven by sample characteristics. Indeed, the evidence is consistent with prior findings on the long-run effects of income and education on democracy.

Conclusions

Lipset's renowned quotation (1959, 75) suggests a gradual democratization process associated with greater socio-economic development. Thus, the empirical strategy of panel data with fixed effects, which assesses the within-country variation between relative wealth or education and democracy, captures econometrically the spirit of Lipset's hypothesis.

Applying fixed-effects OLS and Arellano-Bond methods to our post-World War II data set, using five-year high-frequency panels and conditioning on a proxy for capitalism, we obtain results qualitatively similar to those of AJRY to the extent that increasing both income and education do not induce greater political development. However, to account for weak instruments and endogeneity bias, we use a System GMM estimator advanced by Blundell and Bond, and we find that income and education predict democracy in five-year panels conditioning on economic freedom. Intuitively, as countries become relatively wealthier and their populations relatively more educated, the likelihood of these countries becoming relatively more democratic increases. We also find using OLS that democracy changes over a 35 year period are predicted by income and human capital in 1975. Thus, using our sample and the frequently used method of OLS, we corroborate

prior evidence that was consistent with the modernization theory.

Finally, given the complex interplay between development, capitalism and democracy, we conjecture that one causation channel goes from institutions, such as economic freedom and human capital, to development, and that another channel leads from development to democracy. Thus economic freedom becomes an indirect catalyst of democracy through its impact on development.[33] This of course is not a fully resolved issue and is part of an ongoing research agenda.[34]

33. James Gwartney (2013) in a personal communication states a similar view: "Finally, there is strong evidence that increases in economic freedom promote subsequent increases in income levels. With time, these higher income levels will also promote democracy. Thus, acting through income, increases in economic freedom will also tend to promote democracy. But, the lags between both (a) increases in economic freedom and higher income levels and (b) increases in income and moves toward democracy will be long and variable. Thus, when analyzed across time periods of even a decade or two, the economic freedom–democracy linkage will be quite weak."

34. For some of the evidence on the link between economic freedom and development see Dawson (1998); Faria and Montesinos-Yufa (2009); Gwartney, Holcombe, and Lawson (2006); Hall, Sobel, and Crowley (2010); Rode and Coll (2012); and Bennett, Faria, Gwartney, and Morales (2013). On the relation between capitalism and democracy see Aixala and Fabro (2009); De Haan and Sturm (2003); Lundstrom (2005); Rode and Gwartney (2012); and Giuliano, Mishra, and Spilimbergo (2013).

TABLE A. Impact of income and human capital on democracy

	Short-run fixed effects			Arellano-Bond						Blundell-Bond			Long-run OLS		
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Initial political rights	−0.692 (0.000)	−0.665 (0.000)	−0.673 (0.000)	0.349 (0.000)	0.327 (0.000)	0.447 (0.000)	0.438 (0.000)	0.413 (0.000)	0.391 (0.000)	0.817 (0.000)	0.765 (0.000)	0.788 (0.000)	−0.735 (0.000)	−0.764 (0.000)	−0.821 (0.000)
Initial economic freedom	−0.199 (0.064)	−0.164 (0.106)	−0.204 (0.060)	−0.203 (0.006)	−0.141 (0.103)	−0.090 (0.258)	−0.082 (0.328)	−0.135 (0.111)	−0.124 (0.152)	0.078 (0.504)	0.006 (0.947)	0.017 (0.867)	−0.058 (0.770)	0.081 (0.621)	0.029 (0.875)
Initial income	0.407 (0.045)		0.418 (0.043)	0.514 (0.055)	0.638 (0.028)			0.570 (0.052)	0.744 (0.012)	−0.176 (0.018)		−0.071 (0.411)	−0.536 (0.001)		−0.110 (0.677)
Initial human capital		0.123 (0.138)	0.115 (0.167)			−0.013 (0.831)	0.058 (0.614)	−0.049 (0.438)	0.118 (0.301)		−0.081 (0.018)	−0.055 (0.159)		−0.374 (0.000)	−0.352 (0.008)
Time effects	yes (0.008)	yes (0.047)	yes (0.028)	no	yes (0.674)	no	yes (0.798)	no	yes (0.677)	yes (0.465)	yes (0.006)	yes (0.176)			
Number of observations	785	758	741	527	527	521	521	504	504	785	758	741	85	94	83
Residual AR(2) test				(0.174)	(0.170)	(0.601)	(0.643)	(0.536)	(0.589)	(0.302)	(0.740)	(0.731)			
Hansen OIR test										(0.117)	(0.235)	(0.979)			
Sargan OIR test				(0.005)	(0.004)	(0.003)	(0.001)	(0.011)	(0.005)						

Dependent variables are: for columns (1) to (3), change in political rights between $t-1$ and t; for columns (4) to (12), level of political rights at t; for columns (13) to (15), change in political rights between 1975 and 2010. Sample periods are: for columns (1) to (12), the eight five-year periods 1970–2010; for columns (13) to (15), the one 35-year period 1975–2010. An "initial" variable is the value of the variable at time $t-1$. A "change" in a variable is its value at time t minus its value at time $t-1$. P-values are in parentheses. Estimated coefficients are above p-values. Fixed effects p-values are calculated using clustered standard error by country. Arellano-Bond and Blundell-Bond p-values are calculated using robust standard errors.

Appendix

Data and code files used in this research can be downloaded from the *Econ Journal Watch* website (**link**).

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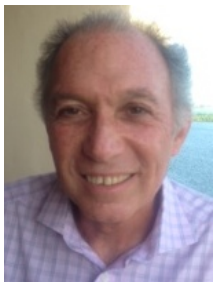
About the Authors



Hugo J. Faria is Visiting Professor of Economics and Finance at Barry University in Miami, Florida. He earned a bachelor's degree in Economics from Universidad Catolica Andres Bello in Venezuela, a master's in Public Policy from the University of Chicago, and a Ph.D. in Business with a concentration in Finance from the University of South Carolina. His email address is hfaria@barry.edu.

Hugo M. Montesinos-Yufa is Professor of the Mathematical Modeling Department at Universidad Simón Bolívar (USB) and Professor of the Center of Finance at Instituto de Estudios Superiores de Administración (IESA), both in Caracas, Venezuela. He has served as Director of the Center for Statistics and Mathematical Software at USB. He holds a Ph.D. in Engineering with concentration in Statistics. His email address is hugomoises@gmail.com.

Daniel Morales is a Research Associate at the Research Center for Leadership and Educational Management of the Barna Business School in Santo Domingo, Dominican Republic. He is also Founder and Director of DatAnalitica. He earned his Ph.D. in Economics (2012) from Universidad Catolica Andres Bello in Caracas, Venezuela. Currently he is engaged as Local Principal Investigator in the J-PAL LAC project "Learning the Value of Education in the Dominican Republic." His email address is D.morales@barna.edu.do.

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Ill-Conceived, Even If Competently Administered: Software Patents, Litigation, and Innovation—A Comment on Graham and Vishnubhakat

Shawn P. Miller[1] and Alexander Tabarrok[2]

LINK TO ABSTRACT

In their article in the *Journal of Economic Perspectives*, Stuart Graham and Saurabh Vishnubhakat (2013) argue that the emergence of the "smart phone wars" and the rash of recent lawsuits over software patents are *not* evidence that the patent system is broken. Graham and Vishnubhakat are both Expert Advisors at the United States Patent and Trademark Office (PTO). Their article is more successful at absolving the PTO of responsibility for low-quality patents than at demonstrating that software patenting has fulfilled the patent system's avowed purpose of promoting the "progress of science and useful arts" (U.S. Constitution, article I, sec. 8).

Graham and Vishnubhakat—henceforth GV—write:

> [W]e examined the US patents involved in some of the high-profile litigation among four major firms in the smart phone industry: Motorola, Microsoft, Apple, and Samsung. … Of the 65 software patents still involved in this litigation, thus far only 21 of them—less than one-third—have received court decisions of the type that provide

1. Stanford Law School, Stanford, CA 94305.
2. George Mason University, Fairfax, VA 22030.

> some indication of their validity or likely validity. Of those, only four patents have had decisions indicating they are invalid or likely invalid. The remaining 17 software patents evaluated so far in these cases have been declared by a court to be valid or likely valid. *This 80 percent favorability ratio is not consistent with the pronouncements that the smart phone wars are being driven by low-quality software patents*. (GV 2013, 73, emphasis added)

Similarly:

> [T]he evidence does not support…low-quality examination by the Patent Office. In fact, data from Patent Office internal quality assurance reviews on nearly 29,000 random examination audits over six years show that, for both software and non-software applications, the overwhelming majority of allowances and final rejections correctly apply the patent laws and examination standards. (GV 2013, 78)

In other words, GV's proof that software patents are not of low quality is that the PTO followed its own regulations and the law. The argument works as a partial defense of the PTO but fails as a defense of software patents. The criticism of the patent system offered by James Bessen and Michael Meurer (2008), Michele Boldrin and David Levine (2008), Dan Burk and Mark Lemley (2011), Tabarrok (2011), Lemley (2012), and others is not that procedures are not being followed. The criticism is that patents are being issued that are far too broad and ill-defined, possibly resulting in a net decrease in innovation. Indeed, GV's argument that PTO procedures and the law are being followed might be taken as a sign that the system cannot be fixed by tinkering with procedures. In our view, what is necessary to make the patent system more supportive of innovation is fundamental change to the legal rules used to define software patent boundaries (for similar judgments, see Burk and Lemley 2011; Boldrin and Levine 2008; Tabarrok 2002; 2011; Lemley 2012; Miller 2012).

Software patent boundaries and functional claiming

The subject matter of a patent is supposed to be a process, a machine, a manufacture, a composition of matter, or a design. Patents are supposed to protect inventions, not ideas. A pharmaceutical patent, for example, protects a specific set of closely related chemical structures, but you cannot patent a particular means of

curing cancer as "any means by which cancer is cured" and thereby exclude every other means of curing cancer.[3] In theory, the same rules apply to software, but in practice the courts have allowed software patents to be much broader and much more abstract than in other areas.

Traditionally, functional claims—claims about the end goal or function of say a machine or process—were allowed in patent claims so long as they were limited by a specific means. Such means-plus-function claiming was essentially a way of defining equivalents and it worked well enough for most physical devices or processes because claims were adequately limited by the specified means. When the courts applied the means-plus-function construction to software, however, they ended up specifying the means as 'a computer' or 'a data processing system,' and this is no limit at all.

Consider U.S. Patent #5,930,474 (Dunworth, Veenstra, and Nagelkirk 1999). The patent's primary claim is simply "A system which associates on-line information with geographic areas." The patent gives this example of what they intend to patent: "[I]f a user is interested in finding an out-of-print book, or a good price on his favorite bottle of wine, but does not want to travel outside of the Los Angeles area to acquire these goods, then the user can simply designate the Los Angeles area as a geographic location for which a topical search is to be performed" (ibid.). In any ordinary reading the patentee has a patent on an abstract idea, thus gaining the right to exclude others from using such an idea. In any other area of patent law, this type of patent would not be allowed. It is allowed for software, however, because software patents such as this one go on to detail the means of implementing such a function. Namely,

> A…system comprising: a computer network wherein a plurality of computers have access to said computer network; and an organizer executing in said computer network, wherein said organizer is configured to receive search requests from any one of said plurality of computers, said organizer comprising: a database of information organized into a hierarchy of geographical areas wherein entries corresponding to each one of said hierarchy of geographical areas is further organized into topics…. (ibid.)

In other words, the means of the patent is the Internet. By merely adding some entirely nugatory terms such as computer, database, and display—nugatory

3. As is usual in the law there are exceptions to everything. Under the Orphan Drug Act, for example, it is possible to get intellectual property protection that excludes all competitors from a field, even those using radically different methods.

because *any* modern method would use these devices—the patentee has turned an unpatentable idea into a patentable, and potentially very profitable, method.

The specification of the means in this patent is a bit like specifying a new business method that would periodically transmit information regarding the qualities, capabilities, and form of specified products and the time, place, and terms at which such products could be exchanged using the means of a plurality of electromagnetic devices connected to a central electromagnetic device via the electromagnetic spectrum—in other words, a patent on radio and television advertising. The Internet is a general-purpose medium, and it should not be considered a specific means that, ipso facto, limits a patent's claims. Not only are software patents often overly broad, but it is often uncertain how wide is the scope of any such patent. The abstract functional language in software claims makes it difficult to relate the words that describe claim boundaries to actual technologies (Bessen and Meurer 2008). In contrast, it is much easier for patentholders and technology users to agree on the scope of a patent that claims a specified chemical compound.

Only towards the end of the article do Graham and Vishnubhakat acknowledge that there are problems with the legal boundaries of software patents. They write that the "disclosure-claim balance … has proven particularly difficult in the software area, where terminology has tended to shift and can be imprecise, and where functional language is frequently used to describe ideas that themselves are inherently functional in nature" (GV 2013, 81). They then assert, however, that disclosure-claim correspondence requirements have been strengthened by recent Federal Circuit decisions and that new PTO guidelines focusing examiners on disclosure clarity and claim-disclosure correspondence will improve the situation.

The Federal Circuit has strengthened requirements for means-plus-function claims.[4] But current precedents make it easy for software patent applicants and holders to avoid these requirements by not characterizing their claims strictly in a means-plus-function format (Lemley 2012). The PTO is not empowered to impose more stringent requirements than those mandated by the courts or Congress. Barring additional legal changes, we may expect uncertain and overly broad software patent boundaries to remain a problem regardless of how much more PTO examiners scrutinize disclosure clarity and claim-disclosure correspondence. GV (2013, 80-81) discuss a number of new "post-grant" procedures created by the America Invents Act of 2011, but all of these procedures expand opportunities for interested parties to challenge the validity of a patent prior to litigating; they do not expand opportunities for interested parties to clarify a patent's boundaries.

4. 35 U.S. Code §112(f). *See* Lemley (2012) for examples of means-plus-function claims and a concise explanation of section 112(f), including why it was originally enacted and how it has been interpreted.

The costs of uncertain software patent boundaries

The evidence that software patents have been a problem since they first proliferated during the 1990s is considerable. Patent litigation is notoriously expensive, and software patents are responsible for a disproportionate share of total litigation costs. Bessen and Meurer (2008) reported that software patents were over twice as likely to be litigated as other patents (based on patents granted between 1983 and 1999 that were asserted in suits filed through 2005). The U.S. Government Accountability Office estimates that nearly half of all patent litigation is for software patents, and, because software is ubiquitous and claims are often broad, the GAO estimates that more than half, 64 percent, of the *defendants* in patent ligation are being sued over claimed software infringements (GAO 2013). Finally, the most litigated patents, defined as those asserted in eight or more separate lawsuits, are much more likely to cover software (Allison, Lemley, and Walker 2009; 2011; Miller 2013b).

Miller (2013b) finds that software patents are weaker, both legally and substantively, than other patents. Between 2000 and 2010 only 20 percent of software patent holders won fully adjudicated lawsuits, compared to 38 percent of non-software patent holders. Also, software patent holders were less likely than holders of other patents to win final judgments that their patents were infringed (31 percent versus 53 percent) and valid (41 versus 57 percent). Most importantly, Miller (2013a) found that software patents are much more likely to be found to lack innovation because their claims were either anticipated or obvious.

So although software patent holders are much more likely to litigate, and to litigate aggressively, they are also more likely to have their patents found not to have been infringed upon and not to have been innovative. The reason for the untoward state of software patents, we believe, is that functional language generates great uncertainty as to how judges will interpret the legal boundaries of software patent claims and, at the same time, the ubiquity of software makes an enforceable claim extremely valuable. As a result, it pays software patent holders and so-called non-practicing entities or 'trolls' to search out and bring weak claims that have value as lottery tickets. Consistent with this theory, litigated software patents are over twice as likely to be the subject of claim construction appeals (Bessen and Meurer 2008), and the Federal Circuit has been 50 percent more likely to find claim construction error when the patent covers software than when the patent covers other fields (45 versus 29 percent) (Miller 2012).

Evidence of software boundary uncertainty in GV's PTO examination statistics?

Graham and Vishnubhakat's evidence is consistent with a narrow conclusion that during the last decade the PTO has given software patent applications the same scrutiny as non-software patents in complying with existing validity standards.[5] The issue, however, is not the PTO but the law—and GV also present interesting evidence that may support our theory that software patents have uncertain boundaries.

GV provide information about the rate at which the PTO's Board of Patent Appeals and Interferences (BPAI) affirmed the patent-application rejections of PTO examiners. From 2003 to 2008, BPAI's affirmance of examiner rejections was much lower for software than non-software patents. Like Miller's (2012) finding that the Federal Circuit has been much more likely to find software patent claim construction error, the lower rate of affirmance shows that when independent experts evaluate the scope of a software patent, they disagree more frequently than when evaluating non-software patents. The recent reversal of that trend seen in Figure 4 of GV's paper (p. 79) may show the impact of *KSR Int'l Co. v. Teleflex, Inc.* (2007), where the Supreme Court gave courts, and (as GV note) by extension PTO examiners, more discretion in invalidating patents on the basis of obviousness. Thus, the courts are slowly moving in the right direction, but in light of the extent of the change that is required, we should not be sanguine about the pace of action by courts. The number of patents has exploded in the last three decades, increasing by a factor of five, and there is very little evidence that the increases in monopoly power that patents have conferred, along with associated uncertainties and costs, have been redeemed by increased innovation.

5. GV's evidence is consistent with but does not prove even the narrow conclusion that the PTO has given software and non-software patent applications the same scrutiny. The administrative appeals from PTO examiner rejections and USPTO Quality Assurance Reviews (GV 2013, 78-79) are largely "in house" quality control reviews and if, as an institution, the PTO is under-scrutinizing software patents, then it would not be surprising to see these reviews result in similar rates of examiner error for software and non-software patent applications. An independent second opinion on patent quality would be more compelling. The GAO (2013) has recently recommended just that in advising the PTO to use information on patent litigation to determine their performance and how they might improve patent quality. Moreover, as we explained earlier, recent studies of litigation outcomes paint a bleak picture of software patent quality.

What explains the smart phone wars?

Graham and Vishnubhakat's analysis of a sample of litigated patents from the "smart phone wars" includes 21 smart phone patents containing software claims that have been subject to validity determinations. Of these 21 patents, GV write that "only four," or 19 percent, have been found invalid or likely invalid, and GV argue that this 81 percent validity rate "compares favorably with other technology areas" and "is not consistent with the pronouncements that the smart phone wars are being driven by low-quality software patents" (GV 2013, 73).

Contra GV, we think that 19 percent of smart phone patents with validity problems is a large percentage and to the extent that numbers are similar in other high-technology fields that only speaks to how widespread is the low-quality patent problem. Moreover, the patents litigated by *practicing* entities (as opposed to "trolls") are not random and are likely to be of higher quality than the average (Marco 2004; Miller 2013a; 2013b). That is, we would expect parties like Microsoft, Apple, and Samsung to be savvy patent holders, expending the high legal fees only when the expected benefit of litigating exceeds the costs (Allison, Lemley, and Walker 2011; Miller 2013b). Such benefit depends on the likelihood that their patents are found to be valid.

The specific lawsuits involved in the smart phone wars do not (so far) appear to be premised on the 'lottery ticket' type of weak claims so often seen in software patent disputes. If this remains true, why has there been so much litigation over these smart phone patents? The explanation, we suspect, is uncertainty—uncertainty not only over patent boundaries, but also over how the courts will interpret licensing commitments made by the patent holders in developing smart phone industry standards. The patent-thicket problem posed a substantial danger to smart phones, so the parties agreed to license patents "essential to the standard on 'fair, reasonable and non-discriminatory'" terms (Contreras 2012). The parties involved in the smart phone wars, however, remain uncertain as to which patents the courts will find "standards-essential" and what licensing terms will be found fair (ibid.). Given the stakes, we are not surprised the number of lawsuits involving these patents has exploded.

Conclusion

GV's evidence related to PTO examination supports the idea that, over the decade 2003 to 2012, examiners have taken the law as given and applied similar levels of scrutiny to software and non-software patent applications. Their evidence is also consistent with the more fundamental argument that the legal standards for

defining software patent boundaries have been weak. We remain convinced that software patents continue to generate greater social costs than other patents.

By calling attention to the apparent legal validity of some of the software patents involved in the smart phone wars, Graham and Vishnubhakat remind critics of software patents that the fundamental issue is not PTO error. But we disagree with GV's conclusion that absence of error is proof of utility. Rather, we join others in arguing that Congress and the courts must rein in the patent system with stricter interpretations of patent boundaries to reduce patents of overbroad and uncertain scope.

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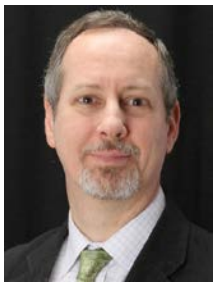
About the Authors

Shawn P. Miller is Olin-Searle Fellow at Stanford Law School. His email address is smiller@law.stanford.edu.



Alex Tabarrok is Professor and Bartley J. Madden Chair in Economics at the Mercatus Center, George Mason University. His email address is Tabarrok@gmu.edu.

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Ragnar Frisch and the Postwar Norwegian Economy

Arild Sæther[1] and Ib E. Eriksen[2]

LINK TO ABSTRACT

In Norwegian academic life the memorial Nobel Prize winner Ragnar Frisch (1895–1973) is still a major figure, and he is universally recognized as a great economist. Here the story will be told how he built up the Oslo School of economic teaching and research, and how the Oslo School influenced economic policy in the small, homogeneous, and relatively culturally insular country of Norway. That influence moved the Norwegian economy toward economic planning. During the postwar decades the Norwegian economy achieved economic growth rates similar to other OECD countries, but with significantly higher investment ratios. The Norwegian economy was getting less 'bang for its buck,' with the result being lower rates of consumption. At the end of the 1970s the lagging economic performance impelled a change.

Much of the present article is a reworking of materials that we have published previously, some with our late colleague Tore Jørgen Hanisch, particularly articles in the *Nordic Journal of Political Economy* (Eriksen, Hanisch, and Sæther 2007; Eriksen and Sæther 2010a). This article hopes to bring the story and its lessons to a wider audience.[3] Translations from Norwegian sources are our own, unless otherwise noted.

1. Agder Academy of Sciences and Letters, 4604 Kristiansand, Norway.
2. School of Business and Law, University of Agder, 4604 Kristiansand, Norway.
3. The present article incorporates some material also used in our ideological profile of Frisch (Sæther and Eriksen 2013) that appeared in the previous issue of *Econ Journal Watch*.

Ragnar Frisch, Nobel Prize winner

Born in 1895, Ragnar Frisch graduated with distinction in 1919 from the Royal Fredericks University of Oslo, with the degree Cand.oecon in political economy. On completion of his studies he was awarded a fellowship from the university to study mathematics, statistics, and economics, and to do so abroad. He spent nearly three years in France, but also visited Germany, Great Britain, and Italy. On his return to the university he continued his scientific activity, believing that research was his calling. In 1925 he became an assistant on a research program in production theory led by professor Petter Thorvald Aarum (1867–1926).

For Frisch the year 1926 was an eventful one (Edvardsen 2001, 9). He defended his doctoral thesis, *Sur les semi-invariants et moments employés dans l'étude des Distributions statistiques*, a work on time series and statistics at the Faculty of Mathematical and Natural Sciences (Frisch 1926a), and he published several academic articles. One was "Sur un problem d'economie pure," which was his first work in economics, and the first in his own quantification program of economic science (Frisch 1926b). It was an attempt to develop an axiomatic foundation of utility, as a quantitative notion to measure statistical variation in the marginal utility of money. Another article claimed that economics should follow the same path towards theoretical and empirical quantification as the natural sciences, especially physics (Frisch 1926c). The same year, Frisch was appointed Assistant Professor in economics and statistics at the University of Oslo.

In 1927 Frisch received a fellowship from the Rockefeller Foundation and went to the United States, where he met the leading mathematical economists of the day. Among them were Irving Fisher (1867–1947), Wesley Clair Mitchell (1879–1948), Henry Schultz (1893–1938), and Allyn Young (1876–1929). Frisch, Fisher, and Charles Roos (1901–1958) began planning the formation of an association that came to be the Econometric Society.

In 1929 Frisch published an article, "Statics and Dynamics in Economic Theory" ("Statikk og dynamikk i den økonomiske teori"), in which he develops dynamics as a new way of analyzing economic phenomena. That year he was promoted to Associate Professor and started to lecture on the theory of production, in which mathematics was used extensively.

On invitation from Irving Fisher he went again to the United States in 1930. He spent several productive months at Yale University and the University of Minnesota. During his stay he produced several papers and gave many lectures, enhancing his reputation as a coming star in economics. He returned to Oslo when the university, with extra funding from the Parliament, created a Chair for him.

During the 1930s Frisch became an ardent protagonist of what he called rational and scientific economics, and he played an active role internationally through his scholarly contributions. His joint efforts to establish the Econometric Society came to fruition in 1931. In 1933 he became the first editor of its journal *Econometrica*, a position he held for more than twenty years. Frisch then published "one of [his] most striking contributions" (Chipman 1998, 95) in *Econometrica*, "Circulation Planning: Proposal for a National Organization of a Commodity and Service Exchange" (Frisch 1934).

During his life Frisch published continuously and was an invited member of a great number of learned societies in different countries and he received several *honoris causa* doctorates. In 1961 he was awarded the Antoio Feltrinelli prize by the Italian society Accademia Nazionale dei Lincei. When the Swedish central bank established its Prize in Economic Sciences in Memory of Alfred Nobel in 1969, the inaugural prize was awarded jointly to Frisch and the Dutch economist Jan Tinbergen (1903–1994) for their development and application of dynamic models for the analysis of economic processes.

Frisch's life work is impressive (Arrow 1960; Johansen 1969; Samuelson 1974; Edvardsen 1970; 2001; Thonstad 2005). He was one of the founders of economics as a purportedly modern rational science, and he made a number of significant advances in the field of statistics and economics. He coined such terms as *econometrics* and *macroeconomics*. In Norwegian academic life he is still a major figure and he is universally recognized as a great economist. He is famous for having written a substantial number of ground-breaking articles on econometrics, time series, linear regression analysis, production theory and business cycles, and for having played an important role in ensuring that mathematical techniques figure prominently in modern economic analysis. During WWII he worked with new methods of constructing national accounts and national budget. He also contributed much to the development of large decision models for government planning.

But, as often is the case for famous people, there is also another story to be told.

The Oslo School of economics

Frisch returned to the University of Oslo and took up his chair in economics and statistics in 1931. The next year he became Director of Research at the newly established Institute of Economics at the university.

As professor Frisch started his grand project of bringing economics as a science "out of the fog."[4] He fought against what he called "fictitious thinking"

4. Frisch used this phrase in a lecture, attended by one of the authors, at the University of Oslo in the early 1960s.

(Bjerve 1995, 24) and claimed that his adversaries, who were many, belonged to what he called "the unenlightened plutocracy"[5] (Frisch 1961b). Frisch held that economics should be a rational and objective science. Economic theory had to be based on mathematical models and quantitative analysis. The new economics should be shaped in a precise mathematical language. Mathematics ensured greater precision and control over assumptions. Only with mathematical models would it be possible to carry out complicated analysis and reasoning. He promoted the agenda with enthusiasm, genius, and force. Preben Munthe (1997; 1999) claims that when Frisch returned from the U.S. in 1931 he had fresh impressions from the economic crises there and his thoughts had a strong American imprint. The market economy with private investors and private initiative was very important, but in a crisis the government should intervene, stabilize the economy, and then withdraw.

Since it was Parliament that provided the funds for his professorship, Frisch felt he was obliged to contribute something in return. In the autumn of 1932 he initiated a private meeting with the prime minister of the center-oriented government, with prominent parliamentarians of the non-socialist parties and also with the leaders of banking, trade and industry. In the meetings he circulated a memorandum (Frisch 1951/1932), not meant for publication; it presented his views on the crisis and remedies to introduce. Monetary policies were the most important tools. When resources were not fully utilized the reason was "lack of circulation money in consumption" (see Munthe 1999, 145). His solution was to increase credit and reduce the income tax. Reduced income taxes would stimulate demand but also create a deficit in the state budget. A key in his program was how this deficit should be financed.

His proposals fell on stony ground and his ideas were considered to be unpractical and far-fetched. Frisch later said that "it was like hitting his head against a wall" and that "their delusions were many" (in Bjerve and Frisch 1971, 5). But their rejection moved him to put his ideas into writings.

Frisch and the economic crisis

In 1933 Frisch wrote three articles in the daily newspaper *Dagbladet* which outlined his economic thinking about the causes of the present economic crisis. The articles were also published in a booklet entitled *Savings and Circulation Regulation* (*Sparing og circulasjonsregulering*, Frisch 1933). Frisch claimed that the economic problems in the 1930s could not be solved with traditional economic policies, such as changing the interest rate. He argued strongly for an active state. "We have to understand that many of the habitual symbols in our monetary and financial system

5. The Norwegian term is "uopplyst pengevelde."

are optical illusions that will lead us on the wrong track. We must emancipate ourselves from these symbols and look in an unprejudiced and sober manner on the underlying factors of the real economy. We must leave behind the old floating sail marks and instead take a bearing on land itself" (Frisch 1933, 35).

His main point was that the crisis stemmed not from a production or poverty problem, but a turnover and organization problem. It was not necessary to make fundamental changes in the economic system. His ideas built on the present system of private ownership. What was needed was a public system that did not directly involve itself with production but instead influenced production indirectly by resolving problems in turnover and organization.

It was the development of what he called the real economy that was of importance and it was superior to the monetary economy. Frisch saw the monetary system as the source of the artificial economic downturn, and believed the monetary system needed reforms (Eriksen, Hanisch, and Sæther 2007). The monetary policies had, in his opinion, been governed by what he described as "monetary fictions." He identified three different "monetary fictions": 1) a skewed distribution of purchasing power between branches and sectors, 2) a skewed distribution of purchasing power between economic classes, and 3) the so-called "encapsulating phenomena" (Frisch 1951/1932). Frisch put greatest emphasis on the last. By "encapsulating phenomena" he meant the fear mentality that developed during a depression. The fear caused businesses, industries, and even nations to behave like the trolls in Ibsen's Peer Gynt—"be thyself enough"—meaning that economic actors kept within themselves and passively reacted to the vicissitudes, reluctant to place new orders before their own sales had increased.

The government should adopt the objective of ensuring that all available resources, both labor and capital, are utilized. The state should redistribute purchasing power and stimulate supply through tax relief. It was essential that any deficit was financed through enhancement in aggregate credit, as opposed to government borrowing that displaced private borrowing and investment. Public works were not part of his plan. His aim was to increase activity in society by stimulating consumption through reduced taxes. When the economy had reached its equilibrium with full employment, the government should "withdraw from the play" (Frisch 1933, 35).

Cooperation between Frisch and Ole Colbjørnsen

His analysis and recommendations in the articles (and booklet) were harshly reviewed by Ole Colbjørnsen (1897–1973), a financial expert, in the daily newspaper *Arbeiderbladet*. This newspaper was the main media outlet for the socialist Labour Party. Colbjørnsen had worked in Soviet service, but had defec-

ted.[6] He lived in London at the end of the 1920s and was fascinated by the ideas of John Maynard Keynes (Munthe 1995; Eriksen and Sæther 2010a, 4ff). Colbjørnsen was crucial in bringing Keynes's ideas to Norway. He attacked Frisch's proposals because they benefited people who were able to pay their taxes.[7] Why not give the support to the unemployed? Or even better: Why not use the money to put people to work? He claimed that Frisch was just scratching the surface of the crisis and that he should instead be debating the basic weaknesses of private capitalism. Colbjørnsen asserted that Frisch was far behind his more progressive Nordic colleagues such as the Swedish professor Bertil Ohlin (1899–1979).[8] Colbjørnsen contended that Frisch's proposals were just another example of a reflation initiated by the government. He was surprised that Frisch did not embrace the program of the Labour Party or realize that private capitalism had failed and that the road forward was a socialist planned economy. Frisch counterpunched, claiming that Colbjørnsen did not put enough emphasize on private initiative. According to Frisch economic intervention had to be organized in a rational way "that one can utilize the tremendous energy source which is implied in the will and initiative of each individual."[9]

Although it started in fury the relationship between Frisch and Colbjørnsen quickly turned cooperative. How this came about is, according to Munthe (1995, 281), not very clear. Perhaps the motivation came in part from the rejection of his ideas by the political leaders he had met with during 1932. A crude theory would be that a yearning to see himself in with a governing set led Frisch to bend his thinking to make himself viable with the one power faction that seemed open to him.[10]

Frisch and Colbjørnsen started a fruitful collaboration and together they participated in the development and making of the Labour Party's crisis plan of 1934. Frisch accepted the proposal for a strong increase in public spending and that it should be partly financed by an increase in taxes, and he no longer praised private initiative and tax relief. Colbjørnsen on the other hand accepted that increased public spending should be partly financed by loans. His major concern was the effect on the real economy. Also in 1933 Colbjørnsen wrote together with Axel Sømme (1899–1991) *A Norwegian 3-Year Plan* (*En norsk 3-årsplan*), in which they

6. Colbjørnsen was a brilliant student and research assistant in the natural sciences during World War I. A scientific career lay open to him. However, he became a Marxist and went into Soviet service, first in Oslo, and later in Moscow, Leningrad and Arkhangelsk. He worked on plans to organize Russian foreign trade. In 1929 he was appointed CEO of a Russian shipping company in London. He defected in 1932 and returned to Norway (Hirsti 2000). During the years 1940–1948 he was financial attaché at the Norwegian Embassy in Washington. He became a free market liberal and a strong supporter of NATO.

7. *Arbeiderbladet*, March 15, 1933.

8. On Ohlin's ideological character, see Berggren (2013).

9. *Arbeiderbladet*, May 15, 1933.

10. See our discussion of Frisch's ideological outlook and its evolution (Sæther and Eriksen 2013).

proposed that the state invest heavily in the economy as a first step to a true socialist economy.[11] Colbjørnsen strongly believed that private investment was not capable of curing unemployment. Only the state could do that, and the state would be more rational than private business people.

After the 1935 parliamentary election, the Labour Party formed a minority government and could have put its crisis plan into effect. To the disappointment of Colbjørnsen, Frisch, and many others, that did not happen (Vogt 1961, 29). The budget was kept in balance and the grants for public works were rather small. The government carried out a contractive fiscal policy. These years could therefore "be seen as the years of neglected possibilities" (ibid., 148). In Norway the business cycle turned in late 1932 (Klovland 1998, 329), and a real upturn started in the second half of the 1930s, partly because of exports to a rearming Germany.

The Labour Party politicians saw both Colbjørnsen and Frisch as theorists and unsuitable as politicians. As a consequence Colbjørnsen moved away from Keynesian policies and advocated, as a Parliament back bencher, industrial socialism, i.e. a system where major industries are owned and controlled by the government. He later became a supporter of a market economy. Frisch withdrew from direct participation in politics and turned his attention to his research and to the content of a new study program in economics at the university.

Eriksen, Hanisch, and Sæther (2007, 6) argued that there had in the 1920s and early 1930s been a general agreement among university economists that a fundamental revision of University of Oslo's two-year program in Political Economy was overdue. Preparations for the new five-year study program in economics were in 1934 led by Professor Ingvar Wedervang. Wedervang wanted to build on the old two-year program, and at the same time introduce new subjects such as business economics, sociology, and economic and social history, and include more use of mathematics. Trond Bergh and Tore Jørgen Hanisch (1984, 146) claim that the new study program approved in 1936 was very much influenced by Frisch. The new program had strong emphasis on the use of mathematics, statistics, and mathematical models and analysis. The reactions against the dominance of Frisch were sharp and the discussion among the university economists about the content and structure continued. As a result, a new committee to revise the study program was appointed the year after it had been launched. However the opposition to Frisch's dominance was divided, and he managed to a large degree to isolate his opponents. In 1937 Wedervang accepted an offer to become the first rector of the newly founded Norwegian School of Economics

11. Sømme had earned a Dr. philos. degree in 1931 and would be professor in economic geography at the Norwegian School of Economics from 1948 to 1969.

in Bergen (NHH). On his departure the dominance of Frisch and his supporters among the university economists was absolute (Bergh and Hanisch 1984, 148).

Frisch concentrated all his efforts on building what came to be called the Oslo School of economic research and teaching.[12] Through the research that was carried out and the new study program he was soon surrounded by many students and disciples that helped him promote his ideas. In the Institute of Economics building, behind the old University buildings, in the centre of Oslo, Frisch created a genuine environment with himself as a kind of 'house-master.' Here the students studied and lived their social life, discussing the important issues of the time as well as playing table tennis and chess. Frisch was often seen playing chess or talking with his students.

Characteristics of the Oslo School

The Oslo School can, in our opinion (Eriksen and Sæther 2010a; 2010b), be characterized by the introduction of quantitative methods into economic teaching and research, underpinned by extensive use of mathematics and statistics. Such tools were used to build and test economic theories and models. The School concentrated on the development of national accounts, national budgets, and macroeconomic planning models. It was marked by a separation of the monetary and the real economy. It rejected the idea of an interest rate as a price on capital, as well as the relationship between interest and liquidity.

During the second half of the 1930s, Frisch and his disciples became increasingly skeptical of the use of market forces to obtain an efficient allocation of resources and distribution of goods.[13] A free market economy was in their opinion unstable. In such an economy the adaptation would in any case be ex post. Therefore it would entail resource waste. A planned economy would, in sharp contrast, give an ex ante adaptation and therefore a better utilization of resources. The solution was a state macroeconomic planning system and state governance with detailed regulations and selective policies for all branches of industries. The

12. According to Bergh and Hanisch (1984), the term "Oslo School" was probably coined by the economist Ole David Koht-Nordby in his review of the book *Hva krigen kostet Norge* (*What the War Cost Norway*, Aukrust and Bjerve 1945) in the newspaper *Verdens Gang*, September 22, 1945. We do not think that Frisch at any time used the term "Oslo School." According to Søilen (1998, 43) the Oslo School is a term used for the main economic thinking at the Institute of Economics and which set its imprint on the students that studied there.

13. Ariane Dupont-Kieffer (2012) claims that Frisch's decisive shift from modelling for market purposes to modelling for planning purposes occurred with the general equilibrium model he developed in the very long and controversial paper on circulation planning (Frisch 1934).

extent of such controls would depend on the economic situation. There was no place for private investors or entrepreneurs in the system. Economists should make the important investment decisions. Frisch's move away from liberalism is, as reported by Hanisch and Sæther (2003) and Sæther and Eriksen (2013), reflected in correspondence between Frisch and Trygve J. B. Hoff (1895–1982), who was the editor of the Norwegian liberal economic journal *Farmand* (an old Norse word for tradesman). At the end of the thirties and beginning of the 1940s Frisch appears convinced that the market economic system had failed. He explained his position in a letter to Hoff at the end of 1941:

> Personally, I believe that we are entering a period where more developed forms for industrial regulations will come to prominence. They are both unavoidable and, in my opinion, correct as counter-measures against the disproportional conditions that have developed. The grotesque outcomes we had in the depression of the 1930s—conscious destruction of commodities, permanent unemployment and stationary machinery—was, I would argue, mainly caused by certain 'individualistic' features in our economic system.[14]

Hoff, for his part, rebutted this statement quite coarsely.[15]

Frisch and his colleagues showed very little interest in the international debate about the feasibility and efficiency of centrally planned economies and they did not see any reason to make their students aware of this debate (Hanisch and Sæther 2005, 83).[16] Through his strong, domineering personality and his habit of disseminating by privileged mimeos his own ideas and judgments, Frisch provided graduates of the Oslo School with an exceptional confidence in the science of economics. Jens Christopher Andvig (1993, 29) writes: "Their confidence was easy to understand because they knew about the complex, but simple world of the 'real' economy. It was knowledge which was exclusive to them and it had been introduced to them by an intellectual genius."

Frisch became increasingly insistent that economic life be strictly regulated. In a 1947 article in the Norwegian journal *Samtiden* he wrote: "Studies of the

14. National Library of Norway, Manuscripts Collection, Brevsamling 761B, letter dated November 10, 1941.

15. National Library of Norway, Manuscripts Collection, Brevsamling 761A, letter dated November 22, 1941. Hoff's journal, *Farmand*, was closed during the Nazi occupation but reopened after the liberation. Hoff, a liberal of the Hayek stripe and participant in the 1947 founding meeting of the Mont Pelerin Society, fought fiercely but in vain against the Oslo School and the centrally planned economic system that was established in Norway after the war.

16. A survey (Hanisch and Sæther 2005) indicates that no reference to Hoff's dissertation had been given to students in the period 1945–1985.

modern economic machinery have made me completely convinced that if this machinery is left to itself, it will according to its nature have to go through convulsive spasms and periodically spread sorrow and misery to large groups of the population." Furthermore, he wrote that there are people who contend that if we start to regulate economic life we will end up with a society where intellectual life is in chains. He admitted openly that intellectual repression was a terrible danger but he did not believe that it would be the case. "Our only hope is that this will not happen. Therefore we will have to burn our ships behind us and put all our effort into this solution: Regulation of the economic life with intellectual freedom." According to Frisch we did not really have a choice. The modern capitalistic system will go through the most terrible economic convulsions if it is permitted to develop under extreme freedom. And he, without predicting the exact time, claimed that an economic catastrophe would surely come in the United States. Frisch claimed that we want to have "full democratic control" of this system of a regulated economy. But to achieve full democratic control we need to educate people so they can understand the main features of the economic relations. And as a consequence we need to increase the number of students in economics.[17]

Frisch not only promoted his views through his teaching and discussions with his students and staff but also through the organization of the research that was carried out at the Institute. He involved many students, including economists, actuaries, and mathematicians. He had a large staff of colleagues and students around him. The traditional independent research method, with smaller individual projects, was to a large extent replaced by large collective research projects. It was the creation of the Institute of Economics that made this possible.[18]

Trygve Haavelmo and Leif Johansen

In addition to Frisch, two professors played an important role in the development of the Oslo School (Søilen 1998, 43). Trygve Haavelmo (1911–1999) had joined Frisch as a research assistant in 1933. From 1933 he and Frisch worked closely together, interrupted only by the war. In 1938 Haavelmo was visiting professor at the University of Aarhus and in 1939 a research fellow at Harvard University. After the war he spent one year at the Cowles Commission in Chicago, where he interacted with Tjalling Koopmans (1910–1975), Gérard Debreu (1921–2004), Herbert A. Simon (1916–2001), Theodore W. Anderson (1918–) and Lawrence

17. *Samtiden* no. 2, 1947, pp. 27, 28, 38.

18. The Institute was originally established with a grant from the Rockefeller Foundation. Later it was financed by private Norwegian sources until 1945 and had an ambitious program.

Klein (1920–2013), among others. During Haavelmo's time in the U.S., according to Joseph Schumpeter (1954, 1163), he "exerted an influence that would do credit to the lifetime work of a professor."[19] His doctoral thesis *The Probability Approach in Econometrics* (Haavelmo 1944) showed that the results of many of the methods used to that time had been misleading. Returning to Oslo he was appointed professor of economics in 1948, a position he held until his retirement in 1979. With his many important research contributions, his teaching, and his generosity and gentle personality, he had a decisive influence on the development of economics. He was awarded the Nobel Prize in 1989 for his fundamental contributions to econometrics.

Based on his practical econometric studies going back to the 1930s, Haavelmo became, like Frisch, very skeptical of the free enterprise system. As Bergh and Hanisch (1984, 211) have pointed out, Haavelmo did not believe in neoclassical equilibrium theory. He denied that markets were a self-regulating mechanism that could be left alone. His skepticism and analytical mind also led him, however, to question the efficiency of some elements of the planned economy. He inclined toward planning, but with diffidence. This diffidence and skepticism characterized his teaching as late as the 1970s.

The other major figure was Leif Johansen (1930–1982), who at age 18 entered the University of Oslo and later became an assistant to Frisch. From 1951 Frisch and Johansen worked closely together. Unlike Haavelmo, Johansen shared with Frisch not just the planning spirit but an aggressive, forward-looking ambition. When Frisch retired in 1965, Johansen took over his chair. Johansen's doctoral dissertation, *A Multi-Sectoral Study of Economic Growth* (1960) became the foundation for long-term economic planning by the Ministry of Finance.(Hanisch, Søilen, and Ecklund 1999, 167) With Johansen, economic planning became a very strong discipline at the Institute. His lectures (collected in Johansen 1977) became the standard work to be used by students as well as economic planners in Norway. Johansen was a member of the Norwegian Communist Party and defended the Party in radio discussions in parliament elections, but he promoted his ideas within the context of a democratic society. Still, he strongly favored a Soviet-type planned economic system and in writing he fought vigorously against free trade (Johansen 1983, 21-27).

19. Clifford Hildreth (1917–1995) at the University of Minnesota was advisor for Arild Sæther from 1966 to 1968. He was at the Cowles Commission at the same time as Haavelmo. He claimed that Haavelmo had a tremendous influence on the research environment. At lunch and coffee breaks Haavelmo distributed new ideas and research proposals freely to his colleagues.

Frisch was the leading light in Norwegian economics in the period 1935 to 1950 (Bergh and Hanisch 1984, 206). Once Haavelmo and later Johansen had established themselves, they gradually assumed the guiding positions.

Towards a centrally planned economy

When France and Great Britain declared war on Nazi Germany in September 1939, the Norwegian socialist Labour government passed a provisional decree that introduced strong regulation of the Norwegian economy. The Trust Control with its director Wilhelm Thagaard (1890–1970) was given the authority to introduce all regulations necessary to control prices and profits. The decree empowered the government to regulate directly or indirectly production and trade. An administrative system was established across the country to carry out these regulations.

During the five years of Nazi occupation these regulations were further developed by the Quisling government of occupied Norway. Meanwhile, the ousted Norwegian government, in exile in London, planned for a reconstruction of the Norwegian economy after the liberation. Thagaard and Erik Brofoss (1908–1979), who had left occupied Norway and joined the administration of the government in exile, played an important role in this planning. They were both strongly influenced or even indoctrinated by Frisch. On 8 May 1945, the day Nazi Germany capitulated, the London-based exiled Norwegian government issued a provisional Royal decree, called "Lex Thagaard" after the originator, which set out a series of important regulations. The decree not only formalized the state control of production and trade that had existed during the war years, it even extended them. Thus, on the day of liberation from one regime, the returning regime tightened restrictions on economic affairs. According to the decree, the Price Commission would acquire a vital position in the Norwegian economy:

> All activities that fall under the Price Commission are of the greatest importance for the reconstruction and development of trade and industry following the liberation. [….] By setting favourable prices for a trade, [the price regulation] can effectively stimulate an increase in production. By reducing prices below cost for firms that are performing badly, it will force a reduction in production, or "rationalization." In addition, the Price Commission has the authority to control the establishment of new enterprises and to execute direct regulation of production and trade and other commercial affairs […] This [control] happened on a large scale during the occupation, and the activities within these areas will surely be greater in the first period

> after the liberation, when industry and trade have to adjust to new conditions.[20]

Note the remarkable belief in the ability to govern, regulate, and control markets, and an equally strong will to use the necessary planning instruments. Decisions that had, prior to the Nazi occupation, been left to each individual player in the market would now be decided centrally by the government and its bureaucrats.

How could the Government push through such a far-reaching edict, in the form of a Royal decree, which was in defiance of fundamental principles of the rule of law? Many authors (e.g. Bergh 1987, 244ff; Lange 1998, 126ff) have tried to explain Lex Thagaard by pointing to the extraordinary conditions that existed at that time, along with a large liquidity surplus, a shortage of commodities, and the need for reconstruction. The Norwegian socialist and historian Berge Furre explained it in the following way:

> In the special situation after the war, it was necessary to ration goods and regulate prices in order to prevent widespread starvation. But other solutions are imaginable, such as a monetary reform, which eliminated the accumulated purchasing surplus. Both money redemption and a one-time tax were used, but the government chose to focus upon administrative regulation of production and trade. With its strong control mechanisms, the war economy had been effective in getting the most from scarce resources, and the "play of the free market forces" did not tempt the post-war government. It smacked of the thirties and unemployment. (Furre 1999, 211)

Furre's explanation does not fully address the necessity of such extended legislation after the war. Norway already had a system for rationing and price regulation, which had been introduced before the occupation. That system had functioned reasonably well during the five years of occupation. According to Furre, there were also alternatives to the policy of detailed regulations. Belgium, for example, unrolled most of its war regulations by the autumn of 1944. Other countries, such as Sweden and Denmark, undertook a more gradual deregulation. Thus, the extraordinary conditions immediately following the war could not fully explain why the provisional decree was retained almost without modification, and even extended, long after this type of regulation was abolished in other Western countries. Here especially the Oslo School deserves credit as a key factor in how economic policy and performance unfolded in Norway.

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20. Paragraph 2 in the "Provisional decree of May 8th 1945 concerned with price regulation and other regulations of industry and trade."

The new freedom and the planned economy

In 1945 the socialist Labour Party won a majority in Parliament. Many of its members and supporters wanted to turn Norway into a socialist society with a centrally planned economy. Norway should develop a national plan; competition is a nuisance and would not lead to a social optimum. New inventions and technical improvements within one firm should, for example, immediately be shared with all the other firms within its industrial branch. But such sharing could only be implemented in a planned economy. Such attitudes led to an expansion of the Norwegian wartime regulations. The Lex Thagaard and several other decisions made by the government and the parliament in 1945 and 1946 can, according to Espen Søilen (2002, 29), be seen as "a step on the way to a permanent form of a planned economy."

Thagaard as director of the Price Commission and Brofoss as Minister of Finance were central to creating the new policy. They shared the opinion that there was a need to govern the economy. It was Brofoss who played the main role in convincing the Labour Prime Minister Einar Gerhardsen (1897–1987) about the possibilities that followed from the scientific knowledge developed and delivered by Frisch and his disciples. Brofoss implemented Frisch's economic thinking in government. Petter Jacob Bjerve (1913–2004), another strong supporter of Frisch and his ideas, and, as head of the Central Bureau of Statistics, important in the construction of the Norwegian planned economy after the war, wrote in his last years: "The economists march—Brofoss governs" (Bjerve 1989, 182).

When Brofoss presented the national budget for 1947 to Parliament he explained the reason for the new system:

> Man has through scientific and technological progress managed to free himself from the violent forces of nature. They have managed to break the strings that nature has laid over human lives. It would be a step towards increased freedom to be able to free ourselves from the blind submission to the chances in economic life, which for each individual looks like the work of the forces of nature. We have to make ourselves master of the economic forces instead of being ruled by them.[21]

The government should therefore free the nation from the blind economic forces. To do so, he said, "a regulated economy is necessary in a modern society."

21. *The Parliament Gazette* 1947, p. 17897.

The Oslo School becomes dominant

When the war ended in 1945, Brofoss worked actively to hire the new Oslo graduates not only in his own ministry. Moreover, he also worked vigorously to get them into central positions in other ministries and directorates. In such positions they became very influential and gained substantial power in carrying out the policies of a planned economy. Bjerve (1989, 190) claimed that Frisch strongly believed that his students could play an important role against "a fictional economy" and that they could prevent the economic disasters like the 1930s. But not everyone looked on with positive eyes. The President of the Parliament C. J. Hambro (1885–1964) felt, according to Rune Slagstad (1998, 278), "anxious about this crowd of young highly intelligent economists that are let loose on a defenseless society."

A nexus of three institutions came to be called "the Iron Triangle." It consisted of the Institute of Economics (at the University of Oslo) with Frisch as its chair, the Bureau of Statistics' research department with Odd Aukrust (1915–2008) as its director, and the planning department in the Ministry of Finance with Eivind Erichsen (1917–2005) as its head. Throughout the Iron Triangle, most of the high-level personnel were researchers trained at the Oslo School. The Iron Triangle played a decisive role in work on national accounts, national budgets, and economic planning during the postwar period. In addition Thagaard's Price Directorate and the Ministry of Industry played an important role.

Tore Thonstad (2005, 239) contends that Frisch, after the liberation in 1945, did not have any great influence on Norwegian postwar economic planning, although Frisch in many articles gave his advice freely. This view can be contested. Indirectly through his Oslo School, the Iron Triangle, and the hundreds of new graduates in the first post war years, Frisch clearly had a considerable influence on the development of economic planning in the first decade after the war. From the beginning of the 1960s he became very critical of the Labour Party government for being unsystematic, so Frisch himself eventually fell out of favor with the socialist politicians, to some extent, but nonetheless the Oslo School movement continued into the end of the 1970s and some would say beyond.

Foiled by free trade, the Oslo School economists need new measures

In 1947 Norway reluctantly accepted Marshall aid with its conditions to join GATT[22] and OEEC. The two organizations worked for free international trade.

22. General Agreement on Tariffs and Trade, later changed into WTO, World Trade Organization.

It is our opinion that membership in GATT and OEEC[23] saved the country from the worst excesses of a regime bent on economic planning, since the regime had to give up some of its detailed regulations on imports and exports. When the gradual free listing of imports started in 1949, Norwegian industries faced increasing competition. The industries had previously been protected by the regulation of imports. Some politicians and planners felt at this point that the foundations of the detailed regulated economy were starting to crumble. Professor Johan Vogt (1900–1991) wrote on this occasion that one of the pillars of the domestic planned economy had been pulled away (see Hanisch, Søilen, and Ecklund 1999, 197).

But the economic planners invented new measures and the detailed regulation of the economy continued into the 1950s. The low-interest policy, which Frisch had strongly recommended, was formally introduced in the spring of 1952 with the white paper "Directives for the Monetary and Credit Policy," followed by the Law on Interest Rates from 1953. Also in 1953, after heated debate, the Parliament made the aforementioned 1945 provisional decree, the Law of Prices and Competition Regulation, permanent law. On this occasion the well known British journal *The Economist* wrote that Norway could not any longer be counted among the countries that had a liberal economic system.[24]

When Norway joined the European Free Trade Association (EFTA) in 1960 it had to give up more of its detailed quantity regulations of international trade. Turning to other levers to bring their wisdom to bear on the commonweal, the economists and regulators now focused on monetary and credit policy. An elaborate system of credit controls were developed to ensure that the government's planned investments were carried out. In June 1965 the Parliament adopted a new credit law, the Law Authorizing the Regulations of Money and Credit.[25] The law was drafted by a committee within which Johansen played a central role. Under the law it became possible for the Ministry of Finance to force private banks to limit their loans to specific industrial sectors. The private banks could be required to hold reserves in the Central Bank and they could also be required to buy government bonds. Private savings could be channelled to state banks. Such powers came under the law's authorization of the Ministry of Finance to determine investment activity levels and resource allocation in the short and long term. Furthermore, investments were regulated through the issuing of building permits, import permits on building materials, and rationing of building materials. The government passed a provisional decree that provided the ministries with a wide set

23. Organization for European Economic Cooperation, later changed to OECD, Organization for Economic Cooperation and Development.
24. *The Economist*, October 3, 1953, p. 37.
25. Ot.prp.no. 28 1964–65, Lov om adgang til regulering av penge- og kredittforholdene.

of measures to control credit markets. The governance system went from rationing of commodities to the rationing of credit. In a culturally homogenous nation of 3.7 million people in 1965, such policy levers were sufficient to sustain a sense of economic planning.

During the 1960s several countries in Western Europe tried to develop models for long-term planning. Søilen (2002, 12) says that Norway was a special case in that both the level of activity subjected to control and the resource allocation were governed at an extremely disaggregated level. Also in Norway the central administration built and utilized models to an exceptional extent.

The role of the planning department in the Ministry of Finance was substantially extended. Reforms to create more efficient economic policies were carried out during the 1950s and '60s. The main purpose was to prepare a long-term state budget. Furthermore, the government's corporate income policy was strengthened. A central macroeconomic planning system, with detailed and selective policies for specific industries, was introduced.

New government in 1965—but the Oslo School still holds sway

In the parliamentary election in the autumn of 1965, the Labour Party lost the majority and a center coalition government took over.[26] Although this government did not use the most extreme tools allowed in the new law on credits, the power of the Oslo School economists in the ministries was so strong that there was no change in the main features of the economic planning of the previous socialist governments.

An OECD study from 1967 was very critical of the monetary and credit policies carried out in some of its member states. It pointed out that "The defects in the long-run [of such policies] lead to waste of economic resources." It suggested that the defects were sufficiently serious to justify re-examination of the methods used for intervening in the financial markets. The study warns in particular against direct fixing of interest rates: "When carried out to the extreme, direct fixing of rates is obviously incompatible with the concept of the market." It reported that cases where the financial authorities dictate the entire structure of monetary and credit policy are rare: "The systems existing in Spain, Greece and Norway probably come closest to this" (OECD 1967). Norway found itself in the company of two countries that were, at the time, dictatorships.

26. The Labour Party lost its absolute majority in the 1961 Parliament election but ruled with the support of a left-wing socialist party. A centre-right government was in power for one month during 1963.

Frisch and free trade

Occasionally during the later years of Frisch's career some Norwegian would argue in favor of a free market economy with free trade, and several times Frisch responded with a rebuke in a newspaper article. One such article, published in the socialist newspaper *Arbeiderbladet* in 1956, was titled "Dangerous Tendencies of Simplification in Economic Policy" ("Farlige forenklingstendenser i den økonomiske politikken"); in it Frisch strongly advocated planning instead of a free market economy.[27] A year later the conservative newspaper *Aftenposten* published a Frisch essay titled "Heretical Opinions on Free Trade" ("Kjetterske meninger om frihandel"):

> If the western democracies persist in basing their economy on the free market they will soon be ousted by the eastern countries who now prepare themselves for an expansion under full utilization using rational and flexible economic planning.[28]

Although Frisch still talked about the necessity of intellectual freedom, he became a great admirer of the Soviet economic planning system. He expressed this view in a letter to Hoff in 1958:

> The depression that the USA (and partly other western countries) suffers from at present is, in my opinion, further proof of the technical inferiority of a free market economy. It is grotesque that the USA is happy if the national product does not sink when one considers that the Soviet Union has a secure and business cycle-free growth of seven or eight percent each year.[29]

In a later letter he revised the numbers to claim that the economic growth in the Soviet Union was about ten percent each year. In 1961 Frisch contended that the majority of western economists did not acknowledge this fact, but in a few years they would be forced to see it.

> The blinkers will fall once and for all at the end of the 1960s (perhaps before). At this time the Soviets will have surpassed the US in industrial production. But then it will be too late for the West to see the truth. (Frisch 1961a)

27. *Arbeiderbladet*, December 10, 1956.
28. *Aftenposten*, March 11, 1959.
29. National Library of Norway, Manuscripts Collection, Brevsamling 761B, letter dated August 11, 1958.

Frisch and his supporters rejected the ability of a free enterprise economy to allocate resources effectively. Frisch compared an economist to a social engineer who has a toolkit consisting of new mathematical planning models, a system of detailed national accounts and detailed regulations and controls. With this toolkit he could beneficially govern the economy. An economist with the right training could assist the politicians in government and parliament to develop the welfare state. Until his death Frisch defended what he called rational economic planning and claimed that it was superior to a market economy (see, e.g., Bjerve and Frisch 1971).

Postwar economic performance

Although the destruction caused by warfare and five years of Nazi occupation was formidable, the effect on production capacity was not as bad as first estimated. Under Nazi rule, Norway's infrastructure (railways, roads, airports, harbors, etc.) was maintained through the use of both the Norwegian labor force and slave labor (prisoners of war from Russia and the former Yugoslavia), and some production facilities were developed. Comparison with other industrialized countries directly involved in the war shows that Norway's loss in production capacity was average.

Already in 1947 Norway reached pre-war levels of production (Aukrust 1965, 62). Initially, Norway's output was oriented towards raw materials and metals. Fish and other unrefined products became the most important export products. Norway was in many ways a half-industrialized country and had, as argued by Anders Skonhoft (1994), an industrial structure characterized by enclaves. The melting furnace works and the shipping located in these enclaves had little interaction with the rest of the economy, and the capital-intensive and energy-intensive industries were as a main rule located in the periphery. Engineering industry, notably shipyards, machinery workshops, and processed products, became an important activity in the 1960s. Engineering-based industries, and in particular shipyards, met serious problems in the 1970s. In the spring of 1966, Brofoss told students of economics at the University Oslo, that "the Norwegian harvesting season was at its end." Norway could no longer depend on agriculture, fisheries, and mining. "We have to start using our heads!"[30]

But then, in the early 1970s, oil and gas was discovered in the North Sea. As a consequence the deindustrialization that took place in the Norwegian economy from the middle of the 1970s was joined by a crowding-out effect caused by the

30. Said in a lecture to economics students at the University of Oslo, spring 1965.

oil and gas sector. Relative to other similarly wealthy countries, Norway remained economically reliant on raw materials.

Initially members of the Oslo School expressed, as earlier mentioned, great skepticism toward the Marshall aid because of the conditions that Norway become a member of OEEC and of GATT, two organizations that promoted free trade and consequently advocated tariff reductions and the abolition of quantitative restrictions on trade. But the economic situation forced Norway to overcome such skepticism of free trade, and in the 1950s and 1960s Norway's participation in international exchange of commodities grew. Norway was a founding member of EFTA, and then in 1972 supported the free trade agreement between EFTA and the European Economic Community, which created the Western European free trade area for industrial products. Although Norway did not join the European Union it is a member of the European Economic Area, and with the exception of agriculture and fishery its economy is totally integrated into the European markets.

The surface: GDP growth rates

How did the planned Norwegian economy actually perform in comparison with its Nordic neighbors and the OECD countries in the 1950s and 1960s?

The yearly economic growth in average GDP per capita, in fixed 1985 international prices, was in Norway from 1950 to 1960 2.6 percent. This is lower than the average of 3.3 percent for all OECD countries. It is on par with Sweden, Denmark and Iceland, but lower than Finland, which had an annual growth of 4.0 percent.

From 1960 to 1973 average yearly economic GDP growth per capita for all OECD countries was 4.0 percent. The growth in Norway during this period was at 3.7 percent. This is again lower than Finland's at 4.3 percent, and on par with Iceland at 3.7 percent, but higher than Denmark at 3.5 percent and Sweden at 3.1 percent.

In the period 1973 through 1988 the average yearly economic GDP growth per capita in Norway was 3.3 percent, which was substantially higher than the OECD average of 1.8 percent. The higher average yearly GDP growth rate from 1973 to 1988 is in our opinion a reflection of Norway's development as an oil and gas nation.

The economic growth in Norway in the 1950s and 1960s cannot therefore, as Skonhoft (1994) and a few others have pointed out, be interpreted as the result of a particularly successful economic policy. Nevertheless, a 'success' interpretation has often been maintained by economic historians such as Einar Lie (2012, 119) and Fritz Hodne (1981, 563), and it has also been reflected in socialist and social democratic memoirs, for example that of Gerhardsen (1972, 155).

Beneath the surface: Investment ratios and consumption

But there is another important factor for any assessment of economic development in Norway during the years preceding the discovery of North Sea oil and gas. From 1950 to 1975, investment ratios in Norway were exceptionally high compared to other OECD countries.[31] The investment ratio is the ratio of total gross investment, private and public, to GDP. This important factor has been overlooked in much of the later literature covering the period—notwithstanding that it was discussed intensively by economists in the 1960s.

The significance of integrating the investment ratio into the assessment can be illustrated by some examples. During the period 1950 through 1959 the yearly average investment ratio was in Norway almost 32 percent. During this time Norway had approximately the same average yearly GDP growth rate as Denmark and Sweden. Denmark and Sweden on the other hand had yearly investment ratios of 17 and 21 percent respectively.

Norway had also in the following decade, 1960 through 1969, an exceptionally high investment ratio of approximately 29 percent, compared to Denmark's at 21 per cent and Sweden's at 23 per cent. During these years the yearly average GDP growth rate was only slightly higher than these countries.

Left-leaning politicians and economists of the Oslo School believed that the policies during the 1950s and 1960s, which were based on the principles of central economic planning and state governance of economic life, would give and had given the best results. High investment ratios in the short run would give high growth rates and increased welfare in the long run. This turned out not to be the case.

The high investment ratios coupled with only average economic growth rates were a source of worry for some economists. Odd Aukrust (1957), then head of research in the Bureau of Census, asked a series of interesting and highly relevant questions: What relation is there between Norway's relatively high level of investment and the growth in GDP? What is the reason we have had weaker growth in GDP in the last five or six years compared to the years just after the war, despite similar investment levels? His most important question was: Is the reason that we do not choose the right investments? Should not Norway, with one of Europe's highest investment rates, be able to show substantially stronger GDP growth rates than is the case? He continues to explain that Norwegian economists and Government officials who took part in international meetings and conferences during the 1950s frequently were asked the following question: Why does Norway

31. Data obtained from Aukrust (1965, ch. 2.3) and Søilen (2010, 103-121).

get so little in return from its very high rate of investment, compared to other European OEEC countries?

Another economist at the Institute of Economics, Johan Vogt (1961, 122-124), also asked why, in light of Norway's high investment ratio, economic growth was so low. He even made a computation showing that if we had the same relation between investment ratios and economic growth as the other countries in Western Europe we should have had 6.2 percent yearly growth in GDP.[32] The actual growth rate was only 2.6 percent! Year after year Norwegians sacrificed better living (consumption) to pay extra for only average growth rates. As possible reasons for the remarkably low "bang" for the investment buck, Vogt points to the high capital-labor ratios in major Norwegian industries such as electro-technical and electro-chemical and shipping. Even Frisch, who at the beginning of the 1960s had been very skeptical of the economic planning carried out by the Labour Party, claimed, in the last available letter from him to Hoff in 1964, that Norway had not gained as much as it should from the country's large investments. He blamed "the incompetence of the government" and its support of "a thoroughly naive and unimaginative form of economic planning."[33] A more rational form of planning was needed.

In 1965 the Central Bureau of Statistics published an investigation of more than 400 pages, with Odd Aukrust as the editor, entitled *The Norwegian Postwar Economy*. In the study the fact that Norway had very high investment ratios in comparison with other OECD countries is registered. No satisfactory explanation is given, but the study points to particular features of the industrial-economic structure in Norway that require extremely high investment, those being the importance of shipping and hydroelectric power, a high level of government saving, and high total saving. In a footnote, however, it is admitted that other countries with which Norway compares itself may also have had industrial structures requiring high investment (Aukrust 1965, 141). The Bureau arrived at the conclusion that "the main explanation for the high level of investment in postwar Norway must be sought for in the strong preference that investment was given in economic policy" (ibid., 142). It was understood, of course, that investment decisions, both public and private, were mainly shaped by the government. Leif Johansen (1966, 14) noted that the high investment ratio was a result of government policy, but said he was not sure whether that was an interesting answer. Neither he nor the Bureau asked if the low return on investment could have something to do with the way investment projects were chosen.

32. Vogt's computation is based on data from Kristensen et al. (1960).
33. National Library of Norway, Manuscripts Collection, Brevsamling 761B, letter dated August 24, 1964.

The cost of high investment ratios

Many historians and economic historians who have given an account of the Norwegian postwar history (e.g., Bull 1979; Lange 1998; Furre 1999; Alnes 2000; Nielsen 2011; Lie 2012) have not compared economic growth in Norway with other countries and have overlooked the fact that high investment ratios and relatively low growth rates come with a cost. One exception is Hodne (1981, 585), who said: "Norway has accordingly sacrificed more than its neighbours to achieve only an average growth rate." But he did not do a further investigation. The price for the high investment ratios in Norway in the 1950s and 1960s was a lower rate of growth in private and public consumption. To our knowledge, no one has used Vogt's counterfactual method—"suppose we had a normal bang for the investment buck"—to calculate in analogous fashion the depression in consumption, in percentage terms. Still, OECD statistics show that Norway was far behind in education (high school and college/university level), health services, and infrastructure. During these years thousands of students from Norway studied at universities in Denmark, Sweden, Germany, Switzerland, the United Kingdom, and the United States because they would be denied entrance at home. The great expansion of the Norwegian system of higher education took place from 1969 and onwards.

We can personally testify that for Norwegians who traveled to Denmark, Sweden, and other Western European countries between 1950 and 1970 the differences in standard of living and private consumption were striking. In the years prior to the North Sea discoveries and development, Norwegian living standards were simply markedly inferior.

In our opinion, neither the prominent Norwegian economists belonging to the Oslo School nor other economic historians have provided a satisfactory answer to the rather disturbing questions that Aukrust raised in his article from 1957. Perhaps the answer lies in the inefficiency of the detailed, regulated, planned economy, and the system's lack of ability to choose the right investments and to correct its errors. The performance of the Norwegian economy by the end of the 1960s and in the beginning of the 1970s was such that even ardent supporters of the Oslo School started to have doubts about the system.

The need for change

At the end of the 1970s an increasing number of economists and politicians agreed that economic policy in general and the industrial policies in particular had to be changed. The result of the selective industrial support policies that had been carried out in the 1970s had alarmed responsible politicians and bureaucrats.

During the 1970s special interest groups had lobbied with remarkable success. The ministries in general, and in particular the ministries for manufacturing industries, agriculture, fishery, and trade, listened to the lobbyists. The result was that industrial productivity and international terms of trade had drastically weakened. As a result, and for the first time since the war, governing failure was discussed (Søilen 2002, 181).

In the second half of the 1970s there was a consensus favoring the revitalization of the stock market and the ending of the selective support policies and credit rationing. Instead of detailed governing of industries the state should limit its use of instruments to framework conditions. Industrial policy should as far as possible be neutral, and any direct industrial engagement by the state should be adjusted to market demand.

In 1977 the Ministry of Finance signalled that the situation was serious, that there was an urgent need for a radical change in economic policy. The trade deficit was expected to be more than 10 percent of GDP and external debt would probably reach 50 percent of GDP. No other OECD country had until then been in a similar situation. A very tight labor market and strong demand had led to a substantial growth in both prices and wages with the consequence that the competitiveness of the manufacturing industries had been drastically weakened. The Bureau of Statistics stated in its *Economic Survey* from 1981 that there was a "dramatic gap" between growth in the country's real income and that in domestic consumption from 1974 to 1977. Changing the economic policy turned out to be very difficult, however. Søilen (2002, 183-184) discusses the reasons for the difficulties in turning the economic policy around. The Labour Party did well in the parliamentary election of 1977. The Labour Party wished to reduce selective support policies, but it was unable to gain the support needed to do so. The Minister of Finance, Per Kleppe (1923–), had limited support from his own government, and several government proposals were turned down by the Parliament. Strong vested interests favored the different support measures. Branches of industry, which received support, wished to keep these measures. Branches that did not receive any support lobbied members of Parliament and government officials, claiming that they also deserved such support and therefore should be included in such programs. Politically, it was a difficult situation. Manufacturing companies were encountering low profitability, and the abolition of the selective support policies left them exposed to competition. The system of subsidies, cheap government loans, and other selective support measures for particular branches of industry had been developed and implemented by bureaucrats; by changing or abolishing the policies, the bureaucrats would lose their power base. This may explain their resistance to change. Brofoss, one of the fathers of

the selective industrial policy, raised the issue in a letter in 1979, shortly before his death:

> The same officials, who have had as their duty to issue loans, and, I must add, have made wrong decisions, are the same persons who are going to propose remission. This can be interpreted as the means to cover up what I will call wrong decisions. (Brofoss, quoted in Søilen 2002, 183 n. 326)

Some bureaucrats in the Ministry of Finance, who were responsible for the practical work with the state budget, had warned about such effects of the support measures, but their arguments did not carry weight with the top management of the Ministry, who for years had indulged a system in defiance of economic reality.

In spite of the serious economic situation the government was not able to turn the economic policy around. Søilen (2002, 186) argues that economists at the Ministry of Finance were unable to level self-criticism. Just as Frisch had blasted the government for not prosecuting economic planning competently, the blame was pinned on irresponsible politicians and organizations. According to Søilen, the civil servants at the ministry should have taken their part of the blame:

> The ambitious economic policy that was carried out was built on ideal assumptions about the possibilities of governance, not only in relation to organizations and the private industries, but also within the state administration. … The governance failure was built into the control system that was built up in the 1950s and 1960s. Only once this was apprehended would it be possible to carry out reforms to the Norwegian economic policy. (Søilen 2002, 186)

Bergen: A new selfhood for economists develops

In 1959, Karl H. Borch (1919–1986) was recruited as a University fellow to the Norwegian School of Economics in Bergen (on Borch, see Aase 2004). In 1963 he was appointed to a new chair in insurance. He stood out as an eminent researcher and became a spirited leader for the younger researchers. He had built a strong international network and he urged his students to pursue doctoral studies abroad and particularly in North America.

Borch built new competencies and achieved international recognition. The developments at Bergen occurred as the influence of the Oslo School slowly waned. By the beginning of the 1980s, economic planning according to Frisch, Haavelmo, and Johansen was no longer the alpha and omega of Norwegian economics. Instead, more emphasis was placed on market economics and the functioning of competitive markets under uncertainty. Jan Mossin (1936–1987) and Agnar Sandmo (1938–) were two of Borch's students who became influential economists in the 1980s. Mossin was part of a group of international researchers who contributed to the development of the modern theory for financial markets, the Capital Asset Pricing Model. Sandmo's research, which to a large extent focused on the theory of taxation, is based on the assumption that we live in a world where we must deal with uncertainty, and where there are limited opportunities for action. Sandmo states that markets and social institutions do not function in an ideal way and we must accept compromises and second-best solutions. This work had a marked influence on Norwegian monetary and fiscal policies and also laid the basis for increased independence of the Central Bank. This line of research was also pursued by Finn E. Kydland (1943–), who, in 2004, together with Edward C. Prescott (1940–) was awarded the Nobel Prize for their contribution to dynamic macroeconomics, notably the time consistency of economic policy and the driving forces behind business cycles.

The Norwegian School of Economics in Bergen did not fire direct criticism at the Oslo School, but rather it showed Norwegian economists a somewhat different way of being an economist, a second option for professional selfhood. During decades following 1960, the Bergen-style economist was, relative to the Oslo School economist, more oriented to the business community and problems of the business world, more favorable to market forces, less enthralled to power in the center of government, and more oriented to international research, particularly in English-language discourse. By a process of rivalrous competition, the Bergen-style economists checked the prestige and cultural power of the Oslo School economists.

The dissolution of Frisch's grand vision

In 1978 and 1979 the government set up two committees to answer important questions about how to turn the Norwegian economy around. In 1979 the Committee for Industrial Growth, also called the Lied Committee after its chair Finn Lied (1916–), which had been appointed by the Ministry of Finance, produced its white paper on the structural problems and growth problems in Norwegian manufacturing industries (NOU 1979). The historian Harald Espeli (1992, 191)

claims that the proposals from this committee represent "the official Norwegian version of the ideological reorientation to the market which characterized the OECD area."

The Committee concluded that political governance failures were just as serious as market failures and that a decentralized market economic system would be better able to tackle the challenges than a centralized economic planning system. The role of the state should be limited to an economic policy that created favorable framework conditions, and declared that the selective industrial policies should be wound up. Also, the committee stated that the government should work to strengthen international free trade; Norwegian industry should be exposed to competition and should participate in new markets.

A decentralized market economy

The proposals were controversial and it was more than a year before the white paper led to any government response. Finally, in a government report (St.meld. nr. 54, 1980–81), which was based on the white paper and its responses, there was a general agreement that the economic system in Norway should build on a decentralized market economy. At the same time the market economy should be regulated through a framework of general laws, taxes and levies. There should be no selective support measures. The report suggested that within such a system each corporation, by attending to its profit, would also advance social welfare.

The 1980–81 report, which was written by the Ministry of Industry, broke with the strategies that for more than 30 years had formed the basis for the work in the ministry (Søilen 2002, 189). It also broke with the fundamental principles of the Oslo School.

The credit rationing system in Norway had changed gradually throughout the 1970s. In other Western countries efficient financial markets were considered important to obtain economic efficiency. Leading economists at the Oslo School denied that financial markets were proper markets and that the interest rate was a price on capital. But the effects of credit market control and low interest rates on resource allocation had raised problems.

Liberalized credit markets

In 1980 the Committee on Interest Rates, which also had been appointed by the Ministry of Finance, presented its white paper (NOU 1980). At this time the committee, maybe not so surprisingly, had no representatives from the Institute of Economics.

The unanimous recommendation from the Committee was to abolish the policy of low interest rates and to allow freer interest-rate formation and consequently more competitive credit markets. The reasons were to allow for a better resource allocation and to resolve problems in administering interest rates.

Thus the Committee implicitly asserted the same arguments against the control system that critics of the system had used when the system was introduced in the 1950s—critics such as Trygve J. B. Hoff and Johannes Andenæs (1912–2003), who warned against the widespread use of enabling acts.[34] The white paper did not formulate criticism of the credit market control system explicitly, however. The revision of economic policy was said to be necessary as a consequence of structural changes in the economy.

The white paper stated that the main objective of financial markets was to implement profitable corporate investments. The best way to achieve that was to allow for competitive financial markets. The commission's first recommendation was to end the control of the bond markets, and in particular to end control of issuing bearer bonds. The interest rates in the bond markets would accordingly be an indicator of the market situation in the credit markets, and would serve as basis for the control of the interest rates in the remaining markets. The government followed up in the spring of 1980 by changing the control of the bond markets.

During the following years the control system of financial markets was in part formally terminated and to some extent undermined by market forces. Leading Norwegian banks envisaged a growing market, and they expanded nationwide by mergers and acquisitions. Norwegian banks also anticipated a future with Norway as a capital exporter due to rising petroleum production, and they established branches at international financial centres.

Consequently, during the 1990s the elaborate system of detailed economic planning and control came to an end.

The legacy of Ragnar Frisch

The implementation of the recommendations from these two committees ensured that the central economic planning system from the mid-1940s to the mid-1970s was totally abolished. Now in Norway, as in most other OECD countries, economic activities are based in large measure on a decentralized market economy with largely free international trade. In Norway, private investors and

34. Hoff, as editor of the liberal journal *Farmand*, fought vigorously against the 1953 Law of Prices and Competition Regulation. The views of Andenæs, a professor of law, are documented in Tafjord (1994, ch. 3.2.1).

entrepreneurs are now chiefly responsible for major investment decisions, and interest rates are governed chiefly by market forces.

Olav Bjerkholt (2005, 25-26 n. 51), in a memorandum from the Oslo Institute of Economics, points out that Frisch, Haavelmo, and Johansen, who all had socialist convictions, reigned supreme at the Institute during the 1960s. They were strong proponents of economic planning. Furthermore he claims that the Institute had a different environment than most departments in Western Europe at the time. Somewhat bitterly he argues that nothing is left of the old tradition. Their books have not been on the reading lists for decades. Even the name of the Institute has been changed from Sosialøkonmisk Institutt to Økonomisk Institutt.

Perhaps a legacy of Ragnar Frisch is the extensive use of mathematics, statistics, quantitative methods, and econometrics in economic teaching and research. The use of national accounts and national budgets remain relevant today. Macroeconomic models continue to be used to forecast and to analyze alternative policy options. But the mathematical trend is not something distinctive to Frisch and the Oslo School; economics training worldwide has grown more mathematical.

The Danish economist Niels Kærgård (2000) discusses whether the teachings of the Oslo School were unique to Norway or common to Scandinavia as a whole. He claims that many of the ideas of the School were generally accepted by Scandinavian economists of that period, but that there were clear differences between Danish and Norwegian economic policies. This difference was due to political reasons. The Labour Party in Norway held for many years a majority while their Danish sister party did not. Kærgård concludes: "If one should draw a policy conclusion from the study of the Oslo School, it has to be, that no truth lasts forever, and that one should be careful to put one's trust in the moods of one's time. … The previous generation of economists saw only the breakdown of the liberal society in the 1930s and during WW2, maybe we today see only the collapse of the planned economies in 1989" (Kærgård 2000, 347).

But our attitude is not that Norway suffered under irresponsible economists and has now come right. Elsewhere we have shown that, prior to Frisch's Oslo School, economic theory and teaching in Norway gave an essential place to entrepreneurs, but to this day the entrepreneur remains absent from economics education in Norway (Eriksen and Sæther 2010b). This unfortunate situation is by no means unique to Norway (see Johansson 2004), and it is representative of more general problems in much of mainstream professional economics. The story of Frisch and Norway is not entirely particular to Norway. One wonders, for example, whether a parallel story could be told about Jan Tinbergen and the Netherlands.

Norway has overcome the worst excesses of the tide that rose and partially receded in the twentieth century, but there still remains much scope for improvement.

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About the Authors

Arild Sæther is professor emeritus in economics, Agder Academy of Sciences and Letters, Kristiansand, Norway. He has held administrative and academic positions at the University of Agder in Kristiansand since 1970. He has for two and a half years been professor of European economic integration at the European Institute of Public Administration, Maastricht, the Netherlands; for three years, he has been Director of EuroFaculty Tartu-Riga-Vilnius. His teaching and research areas include micro and macroeconomics, welfare economics, European economic integration, international economics, regional economics, economic history and the history of economic thought. His email address is arild@saether.no.

Ib E. Eriksen is professor in economics at the University of Agder, Kristiansand, Norway. He has also been a research scholar and assistant professor at the Institute for Shipping Research at the Norwegian School of Economics, Bergen. He has been a management consultant. His teaching background is in international economics, shipping economics and finance. His research fields are in shipping economics and financial economics. In recent years he has worked on projects related to the history of economic thought. His email address is ib.e.eriksen@uia.no.

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The Increasingly Libertarian Milton Friedman: An Ideological Profile

Lanny Ebenstein[1]

LINK TO ABSTRACT

That Milton Friedman (1912–2006) grew more consistently, even stridently, libertarian over the course of the last 50 years of his long life has been noticed by several writers. Among these is Brian Doherty (2012), who published a book review whose title I also use for the present article, simply because it says it best.[2] The present article is written as something of a follow-up to Dan Hammond's recent ideological profile of Friedman (Hammond 2013), which I find highly admirable as far as it goes, but which leaves off how Friedman continued to grow more libertarian during the last several decades of his life.

The "first Chicago school" and Milton Friedman to the late 1940s

Although there are no hard-and-fast definitions, classical liberalism favors free trade among nations and a presumption of liberty in domestic issues. It advocates limited and efficient government, and low taxes. It was and has generally remained anti-imperialist, anti-interventionist, and socially tolerant. Such was the larger view of Jacob Viner, Frank Knight, Henry Simons, and other economists at the University of Chicago from the middle 1920s to middle 1940s. It was apparent

1. University of California, Santa Barbara, CA 93106.
2. Doherty (2013) also speaks of Friedman's "later, more libertarian years."

in this period that the federal government increasingly involved itself in the economy, and during the Great Depression the Chicago economists were almost unanimous in calling for stimulative monetary and fiscal policies and relief programs (Davis 1971). However, with a few exceptions such as antitrust, the Knight-Viner-Simons approach opposed federal meddling or regulation in the process of private economic activity itself, e.g., interference with the price and profit system. At the same time, particularly in the person of Simons, high progressive income and estate taxes were advocated. Also, most Chicago economists had serious concerns about monopoly.

Paul Samuelson well described these public policy views of Milton Friedman's Chicago predecessors, calling their perspective the "first Chicago school":

> It advocated use of the market, but recommended redistributive taxes and transfers to mitigate the worst inequalities of the *laissez faire* system. It pragmatically favored macroeconomic policies in the areas of credit and fiscal policies to attenuate the amplitude of cyclical fluctuations. It endorsed antitrust policies. (Samuelson 1991, 538).

Friedman, who did graduate work in economics at Chicago during the early 1930s, initially shared this moderate classical liberal view. In addition, he adopted a Keynesian outlook of the fiscal source of most inflation. In an interview, he remarked that he could not definitely remember for whom he voted in 1936, when he could first vote, but he thought it was Roosevelt (Ebenstein 2007, 34). He supported much of the New Deal. In 1938 Friedman's future brother-in-law Aaron Director wrote his sister Rose kiddingly, on her engagement to Milton, about his "very strong New Deal leanings" (Friedman and Friedman 1998, 81). Friedman would maintain this perspective through the later 1940s.

The degree to which Henry Simons advocated redistributionist income and estate taxes should be emphasized. Simons maintained that a free-exchange society "involves and permits progressive mitigation of inequality; indeed, it affords the largest possibilities of substantial equality" (Simons 1948, 5). For Simons, government was the appropriate vehicle to redistribute income and wealth, and it should undertake its redistributive activities through taxation rather than regulation: "What is important, for libertarians, is that we preserve the basic processes of free exchange and that egalitarian measures be superimposed on those processes, effecting redistribution afterward and not in the immediate course of production and commercial transactions" (ibid., 6).

Early in his career, Friedman supported government measures to redistribute wealth and income. Although he did not make economic equality his paramount

or overriding goal, he believed that greater equality was an appropriate goal for government to seek. In a 1946 telegram to Leonard Read—part of a now-famous dispute over whether to include a phrase, in a pamphlet by Friedman and George Stigler opposing rent control, to the effect that they also supported greater equality—Friedman wrote:

> Consider it essential to retain phrase quote like us unquote from sentence quote for those who would like even more equality unquote.
>
> If this phrase were omitted we would almost certainly be interpreted as opposed to more equality. …
>
> I believe it essential to make it clear wherein we are criticizing means and wherein ends. Failure of liberals to emphasize their objectives seems to me one of [the] reasons they are so often labelled reactionaries. (Friedman 2006b/1946)

Friedman expressed similar egalitarian sentiments on other occasions in the late 1940s and early 1950s, including in a 1948 article in the *American Economic Review*, "A Monetary and Fiscal Framework for Economic Stability," which he included in *Essays in Positive Economics* (Friedman 1953). In the article he held that the long run, basic objectives "shared, I am sure, by most economists, are political freedom, economic efficiency, and substantial equality of economic power. These objectives are not, of course, entirely consistent, and some compromise among them may be required" (Friedman 1948, 246). He noted, specifically of the goal of equality, that "While a truly free market in a 'competitive order' would yield far less inequality than currently exists, I should hope that the community would desire to reduce inequality even further" (ibid.). Among the policies he recommended was a "progressive tax system which places primary reliance on the personal income tax" (ibid., 248).

Through the middle 1940s, Friedman was Keynesian in his approach to the causes of and cures for inflation. In testimony before the House Ways and Means Committee in 1942, he talked extensively about inflation without mentioning "money" or "monetary policy" (Friedman 1942). The method he then recommended to prevent inflation was increased income taxation. Such comments show the distance he traveled over the course of his career:

> Inflation can be avoided only by reducing consumer spending to an approximate equality with the value at present prices of the goods and services that will be available for purchase. Increased taxes help to bring this about by reducing the amount consumers have available to spend. … Taxation is not, however, the only method being employed

> to combat inflation. Price control and rationing, control of consumers' credit, reduction in governmental spending, and war bond campaigns are the most important other methods that are now being employed. But just as it does not seem feasible to prevent inflation by taxation alone, so these other methods cannot be relied upon in the absence of additional taxes. (Friedman 1942)

Fiscal policy clearly drove inflation in Friedman's earlier views.

In 1953 Friedman inserted a footnote to a 1942 article of his that had omitted discussion of monetary factors while considering inflation, remarking that his omission was a "serious error which is not excused but may perhaps be explained by the prevailing Keynesian temper of the time" (Friedman 1953, 253). Many years later while composing his memoirs, Friedman was surprised to reread his wartime Congressional testimony: "I had completely forgotten how thoroughly Keynesian I then was" (Friedman and Friedman 1998, 113).

New views on money and monopoly

Central in Friedman's transition to more libertarian views was his own work in the late 1940s and the 1950s on monetary history and theory, and the work of Aaron Director and others that challenged rationales for antitrust intervention and public-utility regulation. Both of these principally empirical endeavors prompted Friedman to oppose some government activities he had earlier supported.

Friedman's work in monetary theory began in 1948 as a three-year study for the National Bureau of Economic Research in which he proposed to investigate the "role of monetary and banking phenomena in producing cyclical fluctuations, intensifying or mitigating their severity, or determining their character" (quoted in Hammond 1996, 1). In a 1949 memorandum summarizing his first nine months of work, he said that his "general conclusions differ widely from the assumptions about the behavior of the circulating medium implicit in most qualitative discussions of the role of money in cyclical fluctuations. If valid they have important implications for the possible role of monetary factors in generating cyclical fluctuations and for the possible effectiveness of policies directed at promoting stability by controlling the volume of circulating medium" (quoted in Hammond 1996, 63). Friedman the monetarist was beginning to emerge.

Through the work of Director and others in law and economics, Friedman—along with several other Chicago economists—became convinced that imperfect competition models hardly ever provided an adequate framework for guiding government intervention aiming to make the economy more competitive (Kitch

1983).[3] Friedman wrote in 1999 that his views in antitrust had "changed greatly over time. When I started in [economics] … I was a great supporter of antitrust laws; I thought enforcing them was one of the few desirable things that the government could do." By way of contrast, he came to believe that antitrust regulation was taken over by those it was intended to regulate, and "I have gradually come to the conclusion that antitrust laws do far more harm than good" (Friedman 1999).

Angus Burgin writes that Director's "relentless logic and urbane sensibility exerted a powerful hold over generations of students and earned him broad influence among the faculty" (Burgin 2012, 171). Melvin Reder, in researching the history of economics at Chicago, was "struck by the many strong expressions of intellectual indebtedness…to Aaron Director" (Reder 1982, 7). Director's influence on Friedman and others at Chicago merits further exploration and investigation.

From Mont Pelerin Society in 1947 to *Capitalism and Freedom* in 1962

Particularly as a leading student of Knight, Viner, and Simons, Friedman became increasingly identified as the champion and team-leader of a new or renewed approach in economics—one that emphasizes individual liberty and its corollary of free markets. There is less role for government in regulating the economy to ensure competition, and there is less role for discretionary government spending and taxation in response to changes in economic activity. Friedman increasingly saw government efforts to manage the economy as nonproductive or counterproductive, and based on errant theoretical and empirical claims. Friedman shared many goals of left-liberals, but disagreed with them on means to achieve these goals (Burgin 2012, 188).[4]

Important in Friedman's transition from moderate liberal to more libertarian views was his reading of and friendship with Friedrich Hayek. Friedman read *The*

3. Such an attitude was much different than the earlier Chicago view. In 1960, Jacob Viner, by then at Princeton, wrote that "monopoly is so prevalent in the markets of the western world today that discussion of the merits of the free competitive market as if that were what we were living with or were at all likely to have the good fortune to live with in the future seem to me academic in the only pejorative sense of that adjective." Viner advocated "state-suppression or state-regulation of monopoly practices," which he described as "an instance of deliberate departure from laissez faire" (Viner 1960, 66, 67).

4. Sometime around 1966, Friedman gave a lecture titled "Economic Policy: Intentions vs. Results" at the University of Chicago Alumni Club of Omaha. Friedman said there that when he reflected on the "dispute and difference" he had with modern liberals, the conclusion he always came to was that modern liberals' problem was "not that their hearts are soft, but that their heads are" (Friedman n.d., 18).

Road to Serfdom soon after it was published in the United States in September 1944. He first really got to know Hayek personally at the initial conference of the Mont Pelerin Society in April 1947 in Switzerland. Starting in the fall of 1950, Hayek joined the University of Chicago, on the Committee on Social Thought, where he remained until 1962. Friedman regularly attended his seminar. Friedman wrote in 1976 with respect to Hayek's influence:

> Over the years, I have again and again asked fellow believers in a free society how they managed to escape the contagion of their collectivist intellectual environment. No name has been mentioned more often as the source of enlightenment and understanding than Friedrich Hayek's. I cannot say that for myself, since I was influenced in this direction by my teachers at the University of Chicago before I had come to know Hayek or his work. But I, like the others, owe him a great debt … his powerful mind, his moral courage, his lucid and always principled exposition have helped to broaden and deepen my understanding of the meaning and the requisites of a free society. (Friedman 1976, xxi)

Other sources of influence on Friedman included conferences sponsored by the Volker Fund in the 1950s and students in the early 1960s who were involved with the *New Individualist Review*, a libertarian, student-run journal at Chicago.

But Friedman was not as libertarian in the early 1960s as he later became. In *Capitalism and Freedom* in 1962, Friedman argued that there could potentially be a wide area of government services and activity. To be sure, Friedman supported less government rather than more, private rather than public provision of welfare and other social services, and government activity at the local and state levels rather than the national level. In the area of schooling, he introduced and promoted educational vouchers. Yet he wrote that "the role of the state can never be spelled out once and for all in terms of specific functions" (Friedman 1962, 4), going on to add:

> The paternalistic ground for governmental activity is in many ways the most troublesome to a liberal … Yet there is no use pretending that problems are simpler than in fact they are. There is no avoiding the need for some measure of paternalism…. There is no formula that can tell us where to stop. We must rely on our fallible judgment and, having reached a judgment, on our ability to persuade our fellow men that it is a correct judgment, or their ability to persuade us to modify our views. (Friedman 1962, 34)

Friedman offered few principled reasons to oppose welfare-state activities of government in 1962—his argument was essentially empirical and pragmatic. Though he included "present social security programs" on a list of fourteen government activities that could not "validly be justified" (ibid., 35), the list was more concerned with the regulation of economic activity by government than with direct transfers or social services. Friedman argued that neighborhood effects justify "governmental action to alleviate poverty; to set…a floor under the standard of life of every person in the community. There remain the questions, how much and how. I see no way of deciding 'how much' except in terms of the amount of taxes we—by which I mean the great bulk of us—are willing to impose on ourselves for the purpose" (ibid., 191). He did not discuss Aid to Families with Dependent Children in *Capitalism and Freedom*, and he recommended a negative income tax.

Friedman in the 1960s and 1970s

Friedman appears, in part, to have been radicalized during the late 1960s and 1970s. With respect to the draft, though he included peacetime conscription on his list of activities in *Capitalism and Freedom* that government should not undertake (Friedman 1962, 36), it was by no means a focus of his commentary. Later in the 1960s he made opposition to the draft a major part of his policy reform agenda and served on the presidential commission that recommended ending the draft. Friedman's leading role in opposing the draft is well described by David Henderson (2005).

Friedman seems to have supported military containment of the Soviet Union during the 1960s. In *Capitalism and Freedom*, he wrote: "The preservation and expansion of freedom are today threatened…[by] the evil men in the Kremlin who promise to bury us," adding that "the threat from the Kremlin requires us to devote a sizable fraction of our resources to our military defense" (Friedman 1962, 201). He said in a 1961 letter to Arthur Seldon, referring to the crisis in Laos: "I am very much distressed at the moment by the situation in foreign affairs. Britain and the U.S. seem to be prepared to sell yet another country down the road" (Friedman 1961). Richard Flacks, a principal author of Students for a Democratic Society's 1962 Port Huron Statement and who participated in a December 1966 conference in Chicago with Friedman and others on the draft, remembers Friedman as strongly anti-Communist in personal conversation at that time.[5]

5. Discussion comment made by Flacks after "Writing the History of Neoliberalism," a talk by Angus Burgin at the Center for the Study of Work, Labor, and Democracy, University of California, Santa Barbara, January 17, 2014.

After the Cold War, Friedman became somewhat less supportive of American involvement abroad. He remarked in a 1995 interview, "I'm anti-interventionist," and "I'm sure we spend more money on armaments than we need to." He also said, "I suspect he [Ronald Reagan] would have gotten much more done if it hadn't been for the Cold War and the problem of Nicaragua and El Salvador" (Friedman 1995). Friedman had mixed feelings about the Gulf War, but thought the United States should not have invaded Iraq in 2003 (Ruger 2011, 140-143; Friedman 1995).

Friedman's views of Richard Nixon changed over his life. In his memoirs and elsewhere later in life, Friedman was very critical of Nixon. In 1995, for example, he remarked of Nixon that he was a "terrible president" (Friedman 1995). In his 1998 memoirs, he said: "I was a strong supporter of Nixon in 1968, less so in 1972 … In retrospect, I must confess that I question whether the support was justified" (Friedman and Friedman 1998, 387), emphasizing the wage and price controls Nixon implemented in 1971, coinciding with elimination of the convertibility of dollars into gold by foreign governments (and the introduction of flexible international currency exchange rates, something which Friedman had long promoted).

At the time, Friedman's comments on Nixon were considerably more circumspect. In his celebrated February 1973 interview in *Playboy*, when asked whether he had changed his mind on wage and price controls, Friedman said:

> I haven't—and neither has Nixon. I'm still opposed to wage and price controls, and so is he. … I regret that he imposed them; yet in doing so, I think he behaved the only way a responsible leader of a democracy could. He resisted controls for nearly three years when there was strong pressure for their introduction. He tried to make the case against controls, to educate the people about the causes of inflation…. But he failed and finally gave in to the popular demand…. As a leader, that was a proper thing for him to do… I think Nixon acted properly. The real problem is educating the public, and there he was unsuccessful. (Friedman 1983/1973, 9-10)

In the area of gun control, Friedman moved in a more libertarian direction. In *Capitalism and Freedom*, he wrote: "The police are often concerned with acts of violence. After the event, it is desirable to find out who had access to firearms. Before the event, it is desirable to prevent firearms from getting into the hands of people who are likely to use them for criminal purposes. It may assist in the pursuit of this aim to register stores selling firearms" (Friedman 1962, 145). In contrast, Friedman in 1998 provided the following endorsement for John Lott's anti-gun control book *More Guns, Less Crime*: "This sophisticated analysis yields a

well-established conclusion that supports the wisdom of the Second Amendment to the United States Constitution rather than of those who would limit the right of law-abiding citizens to own and carry guns" (quoted in Lott 1998, back cover).

San Francisco years, 1976 to 2006

After Friedman left the University of Chicago in 1976 and moved with his wife, Rose, to San Francisco, with a position at the Hoover Institution at Stanford University, he changed from being primarily a professional academic economist to primarily a public policy advocate. He had been a leading public intellectual since the mid-1960s, and now this became almost his exclusive occupation. *Free to Choose* (1980), *Tyranny of the Status Quo* (1984), and *Two Lucky People: Memoirs* (1998), were all coauthored with Rose and intended for a popular audience.

The influence of Rose Friedman has to be counted among the factors that moved Milton's work in a more libertarian and popular direction (Blundell 2013). After they moved to San Francisco, she became his main colleague in a way she had not been previously. Her focus, with one or two exceptions, was not the technical economic theory that interested him for most of his career in Chicago. She was always more interested in real-life applications of theory. John Blundell notes: "Of the two, Rose was always the feistier and more libertarian policy person" (ibid., 162).

Milton Friedman's 'retirement' in San Francisco, from 1976 to his death in 2006, was as long as his time on the faculty at the University of Chicago from 1946 to 1976. Perhaps the two issues with which he became most identified in this last phase of his life were school vouchers and drug legalization. He remarked in 1995, concerning whether he had retired from economics, that he had not retired from the profession but from the sort of work he previously did:

> There's been a tremendous advance in specialization in economics, particularly in the econometrics area. I was just looking at recent working papers published by the Federal Reserve Bank of Chicago. These are clearly built on work of mine…[b]ut there's been a new development in econometrics that I haven't kept up with. The techniques they've adopted…are all different from ours. I'm not an expert in them anymore; I really couldn't deal with this material on the level on which they are dealing with it, although I can understand the thrust of what they're doing.
>
> I'm not making any pretense of trying to do any more basic, fundamental economics work. I believe that almost all important

> contributions of a scientist are made in the first 10 years after he enters the discipline. … The 1940s-'60s was when I did my most important economic work…. (Friedman 1995)

It was considerably less intellectually challenging for Friedman to champion public policy positions than to work and debate in the realm of academic economics. Moreover, his work capacity and cognitive acuity when in his seventies, eighties, and nineties could not have been what they were earlier.

Milton and Rose started the Friedman Foundation for Educational Choice in 1996, and he became a passionate advocate for school vouchers. He believed that the introduction of school vouchers would be the key to renewal and increased opportunities, especially for poor people, in the United States. He wrote in 2005 that vouchers are a "means, not an end." They would be a way of "making incremental progress in schooling. … Vouchers would be a sizable step in the right direction" (Friedman 2005a).

Friedman's views on education well reflect the broader evolution of his thought. Though he had supported vouchers in *Capitalism and Freedom*, he also wrote there: "The school system, with all its defects and problems, with all the possibility of improvement through bringing into more effective play the forces of the market, has widened the opportunities available to American youth and contributed to the extension of freedom" (Friedman 1962, 199). He also wrote: "Government intervention into education can be rationalized on two grounds. The first is the existence of substantial 'neighborhood effects' … The second is the paternalistic concern for children" (ibid., 85-86). And he noted: "I am by no means sure that the [voucher] arrangements I now propose would…have been desirable a century ago" (ibid., 96).

But in his San Francisco years, Friedman came to favor the complete separation of school and state (Friedman 2005a). "A monopoly is a monopoly is a monopoly," he thundered at a conference on educational choice in 1992. "A socialist institution is a socialist institution is a socialist institution, and the school system in the United States next to the military is by far and away the most socialized industry in the country" (Friedman 1994, 94). In their memoirs, the Friedmans wrote: "While a case can be made for both compulsory schooling and financing, it is by no means a conclusive case. Indeed, we have since [publication of *Capitalism and Freedom*] been persuaded by the empirical evidence … that neither is justified" (1998, 628; see also 1980, 151-152). In late correspondence, Milton remarked on higher education: "I am much more dubious than I was when I wrote *Capitalism and Freedom* that there is any justification at all for government subsidy of higher education" (quoted in Vedder 2004, 127). He said late in life that his views on education had "become more extreme" (Friedman 1995).

Friedman also in his later years became a leading advocate of legalizing marijuana and other drugs, an issue he did not raise in *Capitalism and Freedom*. He became, in old age, expansive about the advantages of drug legalization. If drugs were legal, Friedman predicted: "I see America with half the number of prisons, half the number of prisoners, ten thousand fewer homicides a year, inner cities in which there's a chance for these poor people to live without being afraid for their lives, citizens who might be respectable who are now addicts not being subject to becoming criminals in order to get their drug, being able to get drugs for which they're sure of the quality" (Friedman and Szasz 1992, 65). He presented little evidence for these assertions.

In old age Friedman seemed to enjoy making his statements strongly anti-government. He had become more a celebrity and symbol than active research economist. Whereas formerly Friedman took pains to provide detailed empirical, often statistical, support for his positions, his mode of discourse now shifted more to invoking and applying the broad verities or maxims of classical liberal thought. Such a mode can easily lead to broad and radical pronouncements. When Friedman was in Washington in the 1940s working for the Treasury Department, he made careful recommendations as to tax policy; in later years he adopted the position that he was for any tax cut at any time for any reason (see, e.g., Friedman 2000). Before having put forward his views on monetary policy, he had engaged in decades of significant academic work, but now he expressed strong views on fiscal policy—that it was more important to cut government spending than to cut deficits (Friedman and Friedman 1998, 354)—even though he had done little work on the long-term effects of large government budget deficits. Asked in correspondence in 1993 how he would improve health in America, Friedman bluntly replied: "No more licensing of doctors. No more regulation of drugs. Not of any kind. Period" (quoted in Pearson and Shaw 1993, 39).

Friedman also became more extreme in the area of social welfare. He wrote in a letter in 2005: "If people are born into a world in which there are very few welfare supports, in which the culture is one that requires people to be responsible for themselves, there will be many fewer such people [who are unable to take care of themselves] than if they are born into a society in which it is taken for granted that the government will come in and help them out." He decried current welfare and social programs, which have the "negative effect of creating a different kind of culture and a different kind of human being" (Friedman 2005b).

Even in the area of monetary theory and policy, Friedman at times late in his career expressed the view that some of his earlier thinking may have been inaccurate. He remarked in 2003 to Simon London, a columnist for the *Financial Times*, causing some controversy: "The use of quantity of money as a target [for central banks] has not been a success. … I'm not sure I would as of today push

it as hard as I once did" (Friedman 2003). Earlier in his career, at least through the 1960s, he had been strong and consistent that the control of money is an important duty of government. Later in his career, he became more receptive to competitive currencies and the abolition of Federal Reserve discretionary power through freezing the quantity of high-powered money (Friedman 1984a). He said on a number of occasions late in life that he'd "like to abolish the Fed" (Friedman 1995).

With respect to religion, Friedman did not believe in God but could not be sure God did not exist, so he considered himself an agnostic rather than an atheist (D. Friedman 2004). He was pro-choice on the issue of abortion, and he supported stem-cell research at a time when embryonic stem cells were used in research and therapy, but he felt abortion and stem-cell research should not be funded by government (Friedman 1984b, 22; Ebenstein 2007, 228). He said in 2005 that "I do not believe there should be any discrimination against gays," but did not express an opinion about gay marriage (Friedman 2005b).

Friedman remarked in old age that he would ideally "like to be a zero-government libertarian," but he did not think that it was a "feasible social structure," defining a zero-government libertarian as "an anarchist" (Friedman 1995). He had written in *Capitalism and Freedom*:

> A government which maintained law and order, defined property rights, served as a means whereby we could modify property rights and other rules of the economic game, adjudicated disputes…, enforced contracts, promoted competition, provided a monetary framework, engaged in activities to counter technical monopolies and to overcome neighborhood effects widely regarded as sufficiently important to justify government intervention…—such a government would clearly have important functions to perform. The consistent liberal is not an anarchist. (Friedman 1962, 34)

Friedman clearly represents an instance of ideological migration.

Conclusion

Milton Friedman's views evolved over the course of his career, from a rather moderate liberal position in the 1930s and early 1940s to a definite classical liberal position in the 1950s and then increasingly to a robust libertarian view. This journey has been noted by others. According to William Ruger in his recent intellectual biography of Friedman:

> Friedman grew more radical as he got older. In the 1940s and early 1950s…Friedman's rhetoric was much less positive about *laissez-faire* and much friendlier toward state action…. At that time, Friedman was also more vocal about the importance of equality of economic power and the role of the state in reducing inequality. As time went on…Friedman saw greater and greater problems with government action. … [He] became more radical in education policy (more favorable to complete privatization), social welfare policy (questioning whether even a negative income tax was justified in principle), and monetary policy (more friendly to free banking/competitive currencies). By the 1990s, he was arguing that government had become "a self-generating monstrosity." (Ruger 2011, 96)

Burgin writes strongly and convincingly in *The Great Persuasion: Reinventing Free Markets since the Depression* (2012) that Friedman became more sharply libertarian than his predecessors at Chicago and elsewhere—including Hayek—and that it was specifically Friedman's influence that underlay much of the rhetoric of Barry Goldwater and Ronald Reagan. Burgin (2012, 198) notes that "as Friedman matured as an economist, he gradually began shifting his focus away from his technical work and toward his pursuits as a popular proselytizer."

Friedman acknowledged that his later work and opinions were not of the same order as his earlier work: "I don't regard what I've done in the field of monetary policy as on the same level as what I've done about trying to get rid of the draft or legalizing drugs" (Friedman 1995). He often said that he wished to be remembered for his scientific work. "My vocation has been professional economics," he wrote in closing the preface to his memoirs in 1998. "My avocation has been public policy" (Friedman and Friedman 1998, xii). In an interview shortly before he died, Friedman (2006a) remarked: "I really had two lives. One was as a scientist—as an economist—and one was as a public intellectual." He wrote in a 2001 letter: "My contribution to the libertarian cause has not come on the level of values…but rather by empirical demonstration, …by advancing the science of economics and showing the relevance of those advances to the policy of economics."

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About the Author

Lanny Ebenstein teaches the history of economic and political thought at the University of California, Santa Barbara. He is the author of the first biographies of Friedrich Hayek and Milton Friedman and many other works, and he is the editor of a collection of Friedman's writings called *The Indispensable Milton Friedman*. Dr. Ebenstein's work has appeared in a wide range of publications from *Philosophy* to *Nature* and has been translated into a number of foreign languages, including Chinese, Japanese, Spanish, Italian, and Polish. He received his Ph.D. from the London School of Economics and Political Science. He is currently at work on a history of economic thought at the University of Chicago. His email address is lannyebenstein@aol.com.

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