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IN MEMORIAM 135

ECONOMICS IN PRACTICE

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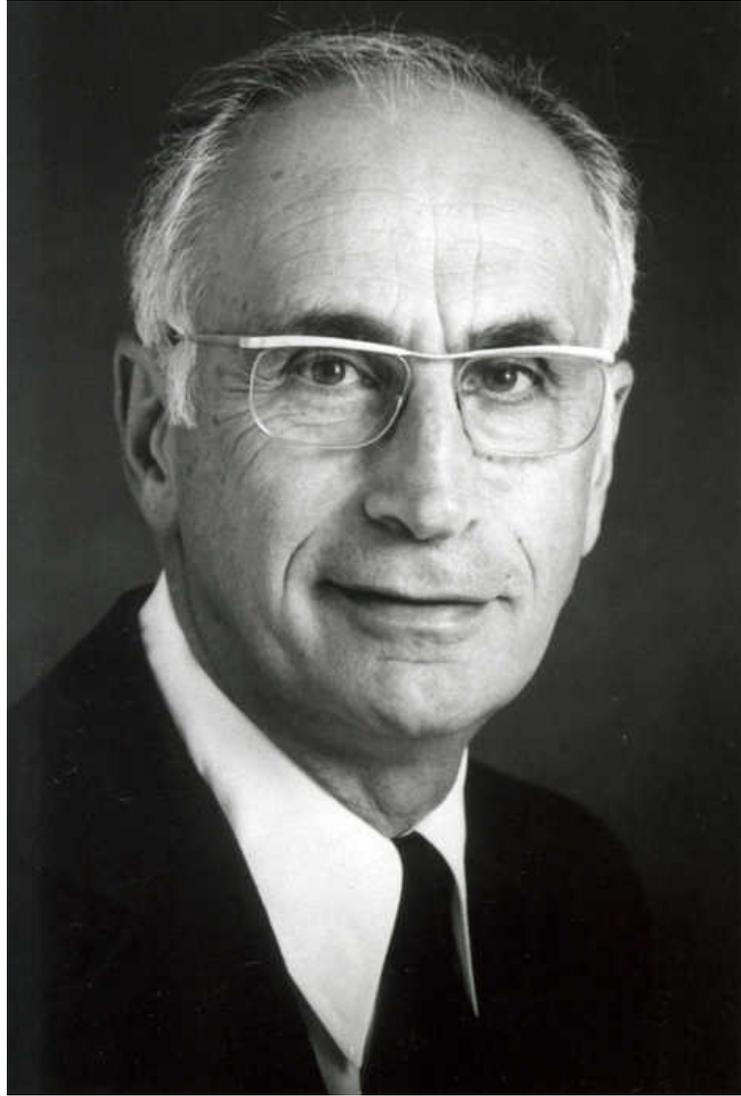
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In Memoriam



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Regression Costs Fall, Mining Ratios Rise, Publication Bias Looms, and Techniques Get Fancier: Reflections on Some Trends in Empirical Macroeconomics

Martin Paldam¹

[LINK TO ABSTRACT](#)

The following deals with an ‘iceberg’ property of empirical economic research: Only a small fraction of the statistical estimates done are actually published, so most of the berg is hidden below the water. That part is the dangerous part. This paper suggests that it is growing, and that this is a problem.

The problem can be analyzed by considering the body of research that attempts to determine the size of some important parameter β . The research output has two dimensions: The visible dimension of the reported results and the invisible dimension of unreported results. The total number of regressions run is given by the product NJ , where $J - 1$ is the average number of unreported estimates behind each of the N reported estimates. The paper speculates about the invisible dimension using economic theory and some general results from the recent wave of meta-studies, which analyze the visible dimension.

The key observation that has prompted this paper is that the cost of running a regression has fallen rapidly: Data is becoming increasingly available, and our computers and econometric packages are becoming faster and more user-friendly. The researcher, therefore, will run more regressions and get more estimates of β . For this simple reason it may be suspected that the number of regressions made per regression published— J , which can be called the *mining ratio*—is rising. I will argue

1. Aarhus University, 8210 Aarhus V, Denmark. I am grateful for comments from Chris Doucouliagos, Erich Gundlach, and Sarah Necker, and for all comments when this paper was presented at the 2012 MAER-Net Colloquium, September, Perth, Western Australia and at the European Public Choice meeting in Zürich in April 2013. Also, I want to thank the referees for their feedback.

that this is likely to lead to a rise in publication bias, which is the excess size and significance of the published estimates, as compared to the true values.

A looming increase in publication bias should be counteracted.² Indeed we see another trend today, namely a rapid rise in the number and sophistication of econometric techniques that do increase regression costs. Meta-studies typically show that little of the variation between empirical results is due to variation in econometric techniques, which suggests that new techniques have a small marginal productivity. It is tempting to see the trend toward econometric sophistication as part of a response to the falling cost of regression, but there are no doubt other reasons to demand that front-line econometrics is used.

This paper discusses problems that are present in all empirical economic research. In micro the dataset is normally an important part of the experiment. The sample is often large, independent, and expensive, and thus replications are independent. Below we concentrate on macro investigations that use smaller publicly available datasets. An experiment is basically one regression, so meta-analysis is done by MRAs, meta-regression analyses. Since the data often overlap between studies, independent replication is difficult.

It is common in such literatures that NJ , the number of regressions done to determine the size of a certain β , exceeds the number of available data observations, which may be 50 annual observations, one for each of only 50 countries.³ That is, the ‘mining collective’ of researchers analyzing β has, in a manner of speaking, exhausted the degrees of freedom many times.⁴

Data mining has long been discussed by applied economists; see, e.g., the essays in Hylleberg and Paldam (1991). D. F. Hendry (2001) has recommended that data be broken into two, where you mine the first to your heart’s content and use the second to validate the best model found; the problem is that you may mine the second part too. E. E. Leamer (1983 and later) has a series of papers on the “con” element in econometrics caused by data mining, arguing for (rather cumbersome) Bayesian methods. In the field of growth regressions a large discussion has taken place, which is summarized in chapter 12 of Barro and Sala-i-Martin (2004).

This paper has some relation to the discussion started by John Ioannidis (2005) dealing with medical research, where meta-analysis originated. That discus-

2. Hopefully journal editors and other gatekeepers can do something, but perhaps they are part of the problem.

3. The reader may think that β is the slope of the IS curve, or that of the LM curve, Phillips curve, consumption function, etc. Or maybe β is the effects of education, democracy, or development aid on economic growth.

4. The smallest of the literatures mentioned in note 3 is the one on aid effectiveness. It uses cross-country data averaged over four to ten years, where about 1,000 observations are available. Here about 1,500 regressions have been published, and if $J \approx 25$, it means that the degrees of freedom have been used more than 30 times.

sion deals with problems that are fairly close to those in microeconomics. Even with independent data samples compiled to estimate a certain effect, one can still vary the explanatory models substantially.

In economics the seminal paper was T. D. Stanley and Stephen Jarrell (1989), along with the collection of papers in Roberts and Stanley (2005). However, the state-of-the-art technique was only developed in Stanley (2008), which greatly helped in pushing the ongoing wave of meta-studies. Chris Doucouliagos and I have discussed some of the problems considered here, but only based on the development aid effectiveness literature (Doucouliagos and Paldam 2009). The present paper is an attempt to show that economic principles provide some insights into the mechanisms underlying the problems of data mining and publication bias, and it speaks to how much these problems can be reduced by fancier econometrics.

The theory of the rising mining ratio

The analysis below considers a literature aimed at estimating some parameter β by regression experiments. The studies of β have two dimensions: A visible dimension, comprising the N -set of regressions that are reported in the literature; and an almost entirely invisible dimension that comprises the J -set of regressions run as part of the research processes behind each of the N published results. For ease of presentation I make two assumptions: (i) Each study reports one estimate of β , reached by a regression. (ii) At any point in time J_t is the same for all researchers, and J_t moves smoothly over time. The mean J over time in the β -literature is \bar{J} . The full body of regressions to determine β is $N\bar{J}$.

In the 150 papers of the aid effectiveness literature, the number of published regressions per paper has gradually increased from about five to about ten during the last 40 years (see Doucouliagos and Paldam 2011). In this regard the aid effectiveness literature appears to be typical. It is important that the individual author has little influence on that number, which is largely determined by editors and referees. Given that the number is (almost) exogenous, it might as well be assumed that it is one, and so assumption (i) is that each paper in the β -literature reports one estimate. This will save one index on the variables below.

The mining ratio J differs from one paper to the next. I assume that if, by a miracle, all J_{it} were revealed for one β -literature, and plotted over t , a well determined moving average (kernel) would appear in these data. The analysis deals with papers having J 's equal to that moving average; in this sense we discuss the J of the typical paper. Assumption (ii) is that J is the same for all papers written at time t and that J_t has a smooth trend over time. This saves another index on the variables below.

The rest of this section of the paper deals with the J -dimension of the β -literature, that is, with the J regressions run as part of the research process behind each published result. The profession agrees that J is private information for the individual researcher. We cannot peer into the private space of the individual researcher, so J is unknown. I have applied introspection and informally polled friends and colleagues, and my guess is that, in 2013, J is well above 25. It should be added that there are search strategies that make it difficult to define what counts as a regression privately undertaken. A simple example is that econometric packages typically provide estimates with a handful of diagnostic tests pointing to the nature of variables that can increase the fit. So perhaps the true increase in J is larger than a simple count will reveal, since packages are doing some of the experimentation for us.

Thus, J is the unobserved private choice of each researcher. Economic intuition suggests that he will go on regressing as long as he feels that $MC(J) < MB(J)$, where $MC(J)$ is the marginal costs and $MB(J)$ is the marginal benefits. The J chosen is hence the solution to:

$$MB_t(J_t) = MC_t(J_t), \text{ where } t \text{ is an index for time that is used to discuss } J = J_t \quad (1)$$

This equation is meant to be specific to the individual, even to the individual investigation, but I economize on notation. The equation expresses the equi-marginal principle so common in economic analysis. As far as I know it has not been clearly stated in any analysis of J .

The total costs of macroeconomic research are high: Macroeconomic data are expensive to compile, econometric packages are costly to write, and human capital of researchers in the field is expensive to produce and acquire. The individual paper carries only a tiny fraction of these costs. Most macro data are free to use. At the operative margin, the human capital of researchers is a sunk cost. Econometric packages are installed on the computers of researchers, who know how to use them!

The costs for the author of the empirical part of a paper arise from the opportunity costs of the following steps in the work: (i) The data set has to be collected from the relevant sources, (ii) it should be organized in the computer in the form needed by the econometric package; then (iii) the necessary commands have to be given to the computer to run the regression; finally, (iv) the results should be studied. The time necessary to implement each of the four steps keeps falling.

When I started in research more than 40 years ago, data had to be copied from tables in books found in libraries. Computers were mainframes, and the data had to be punched onto cards that were entered into the computer by a special

staff. Once read into the computer the cards could be stored on magnetic tape. You had access to the mainframe a couple of times a day only, and then your tape had to be mounted. The early statistical programs were clumsy, poorly documented, and not very user-friendly. But they were a huge advance from the pre-computer technology. Lots of orders were necessary for running a regression, and if you got something just a tiny bit wrong, as frequently happened, half a day was wasted. Results were provided on big sheets of paper with green lines.

Today, data libraries are virtual in the form of webpages hosted on computers around the world. Most of the relevant pages post data in a format that is user-friendly, so that data can be downloaded straight into a program, such as Excel, that all researchers have on their own personal computers. To combine data from different sources and get them into the form necessary for the econometric package still needs some work, but the work typically only takes a couple of days. Personal computers now have about the same brainpower as the typical mainframe 40 years ago. The companies producing econometric programs are impelled by the competition to make large efforts to make the packages user-friendly.

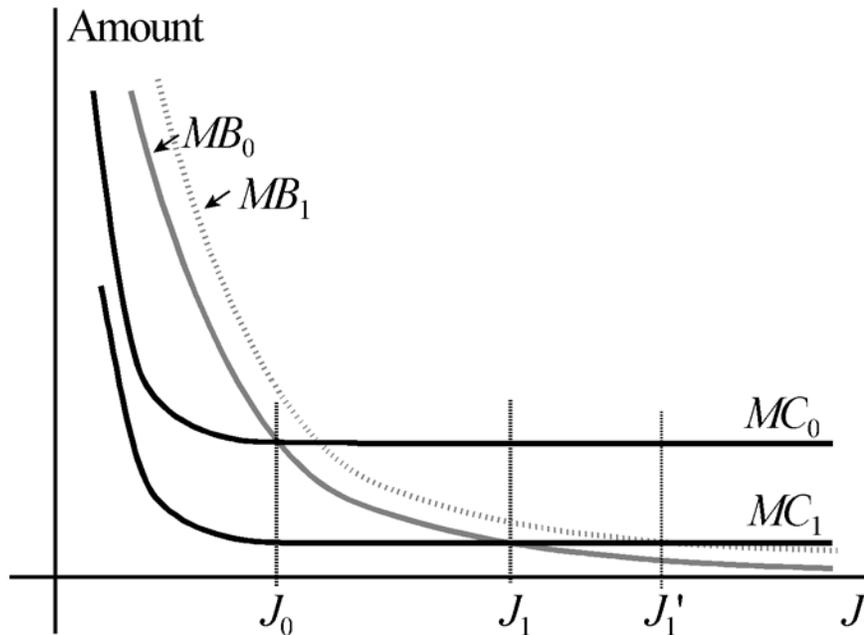
Therefore, it is a strong fact that the *MC* curve is shifting downward over time. In the 40 years the cost of the marginal regression has probably dropped between 100 and 200 times—say 150 times. A fall by 150 times over 40 years is 3.5 times per decade or 13.5% per year. The drop is probably approximately log-linear. Technological jumps have occurred, but regression costs do not fall neatly in steps. Think of the change from mainframes to PCs: productivity gains were small in the beginning because PCs were slow and few programs were available, but the productivity gains have kept growing.

The form of the *MC* curve

The first regression still needs some days of work, and maybe the next few regressions also need new data to be entered into the program, but then it will quickly fall to almost nothing—just a few keystrokes. Also, the output may just be a screen to be looked at for a moment, and then you rush to the next screen till something looks good. Thus, the *MC* curve flattens out. This is drawn in Figure 1.

For $t = 0$ the *MC* curve is MC_0 . For $t = 1$, one decade later, the curve is MC_1 . The two curves are drawn to reflect the crude orders of magnitude mentioned. That is, the shift in the *MC* curve is almost three-quarters of the way down toward the horizontal axis. It was argued that the cost per regression was falling along a log-linear path. As the picture is the same (after a multiplicative transformation of the axes) next decade, this also implies that the *J* increases log-linearly.

Figure 1. The mining ratio (of the average researcher): A function of benefits and costs



Note: The initial situation ($t = 0$) and the end situation ($t = 1$), are indicated with a subscript. They differ by one decade. The curves are drawn as explained in the text.

The reader may wonder, as I do, how long the downward shifts in the $MC(J)$ curve can continue. The next step, already underway, will make it so data available on the Internet are downloadable directly into the main econometric packages. It appears that the Stata package is winning market dominance, and some of the main data providers, including the World Values Survey and the World Development Indicators (World Bank), are already providing data in Stata format. Stata's data handling ability increases, and, of course, computers keep improving. There is space for a continuation of the downward shift in the $MC(J)$ curve.

The form of the MB curve

Seen from the point of view of the individual author, the marginal benefit of one extra regression depends (though not entirely, surely) on the increase in his chance of publication and his expected benefits from publishing. The main factor determining the shifts of the $MB(J)$ curve over time is probably the increasing use of publication information (about citations and impact factors of journals) on the

academic job markets. The increasing use of such information in the job market leads me to suggest that the *MB* curve shifts a bit upward over time. The MB_1 curve in Figure 1 shows the effect of an upward shift in the *MB* curve—it further increases the equilibrium value for *J* from J_1 to J_1' . The MB_1 curve is dotted to indicate that it is difficult to assess the size of the shift. But how much *MB* rises is secondary; the story is mainly about the fall in *MC*.

The solution to equation (1) is the intersection of the two curves. As drawn, the intersection J_0 moves to J_1 , or probably to J_1' . Already, J_1 is twice as large as J_0 , and J_1' is even further from J_0 . I have assumed that the two situations are one decade apart. Hence, the graph suggests that *J* increases 3.5 times each decade. Obviously this estimate is very rough. But I think that it is hard to draw the two curves in a way that is both reasonable and gives only a small rise. I proceed on the assumption that the mining ratio is rapidly rising and that the rise is log-linear.

Findings from meta-analysis

This section switches to the visible dimension of the β -literature, i.e., it turns to the *N*-set of reported estimates, b_i , of β . They have standard errors s_i , *t*-ratios $t_i = b_i/s_i$, and precisions $p_i = 1/s_i$.

Meta-analysis was developed in medicine where it is a standard technique. More than 100,000 meta-studies have been made in medicine. A handful of textbooks have appeared—the main one is Hunter and Schmidt (2004). A medical experiment is costly, and hence it is reported. Also, it typically uses data that are unique to the experiment, but meta-studies still point to important problems (see Ioannidis 2005).

In microeconomics an experiment often involves independent data collection and a set of calculations, which might be a regression, but also they may not be, as is often the case in medicine. It is possible to vary the model used, but the data are to provide an answer to a question. Hence, the model variation is not so large. Still a meta-analysis of the replicability of the result is necessary. The situation in microeconomics is fairly close to the situation in medicine.

In macroeconomics the data are often available, so an experiment is a regression. And the meta-analysis is done by regressions on regression coefficients (MRA). As discussed a regression is so cheap that many of the ones made remain unreported.⁵ Macro data are limited and many studies are therefore done on over-

5. In the analysis of macro models all relations need to include controls so that the *ceteris paribus* assumption holds. If, e.g., the researcher considers 25 potential controls and a tidy model contains fewer than six controls, then there are about 70,000 possible model variants to try.

lapping data, so the meta-technique has had to be adjusted to be equally useful. The adjustment took some time, as mentioned. The first textbook on meta-analysis in economics is Stanley and Doucouliagos (2012), and recently a set of the guidelines has been published; see Stanley et al. (2013). Before 2005 only a handful of meta-studies had been done in economics, but since 2008 the number has increased to about 500. As of 2012, only one study (Doucouliagos and Stanley 2012) has looked across many meta-studies. The meta-studies have coded approximately 20,000 primary papers. About half of the meta-studies are in macroeconomics.

At the basic level, meta-analysis is very robust, so that if two teams make an independent meta-study of the β -literature, they reach virtually the same result.⁶ It consists of four steps: (1) The literature is collected; (2) it is coded; (3) the distribution of the estimates is analyzed by the funnel graph (see Figure 2); and (4) the FAT-PET is estimated, where FAT stands for Funnel Asymmetry Test and PET stands for Precision Estimate Test.

If the estimates are weighted with their precisions, this will move the mean towards β . This is the idea behind the FAT-PET (from Stanley 2008). It is the regression:

$$b_i = \beta_M + \beta_F/p_i + u_i, \text{ where } \beta_M \text{ is the PET, } \beta_F \text{ is the FAT, as explained in the next paragraph, and } u_i \text{ is noise. It is a hyperbola converging to } \beta_M \text{ for } p \rightarrow \infty, \text{ as drawn in Figure 2b.} \quad (2)$$

The FAT is a Funnel Asymmetry Test, so that if $\beta_F \neq 0$, by a standard t-test the funnel is asymmetric. The literature referred to shows that it is a rather powerful test. In Figure 2a, β_F is zero. The PET is the Precision Estimate Test, β_M . It is the PET meta-estimate of β . A standard t-test tells if it differs from zero. In Figure 2b the β_F/p_i term converges to zero and $\beta_M = \beta$. The literature shows that in the face of publication bias, β_M is a fine estimate of β . Hence, the publication bias is estimated as:

$$PB = (\underline{b} - \beta) \approx (\underline{b} - \beta_M). \text{ Note that the publication bias is } \underline{b} \text{ if } \beta = 0. \quad (3)$$

Figure 2 may help the reader to build intuition about the meta-analysis, and Table 1 is meant to provide some help to keep track of the notation. In Figure 2, each funnel presents the distribution of the N estimates b_i as a scatter over their precision p_i (on the vertical axis). In the case analyzed, economic theory predicts that $\beta > 0$, so estimates where $b < 0$ have the ‘wrong’ sign.

6. Both at step 1 and especially at step 2 it is difficult to fully escape mistakes, but experience shows that a small number of stochastic errors matter very little; see Doucouliagos and Paldam (2008), Mekasha and Tarp (2013), and Doucouliagos and Paldam (2013).

Figure 2a is the ideal funnel where all estimates are published. The least precise results scatter most, and as precision increases the results move closer. This gives a lean, symmetrical funnel, so the FAT-PET is vertical. The FAT $\beta_F \approx 0$, and the PET meta-average $\beta_M \approx \underline{b} \approx \beta$.

Figure 2. Funnel diagrams and the FAT-PET estimate

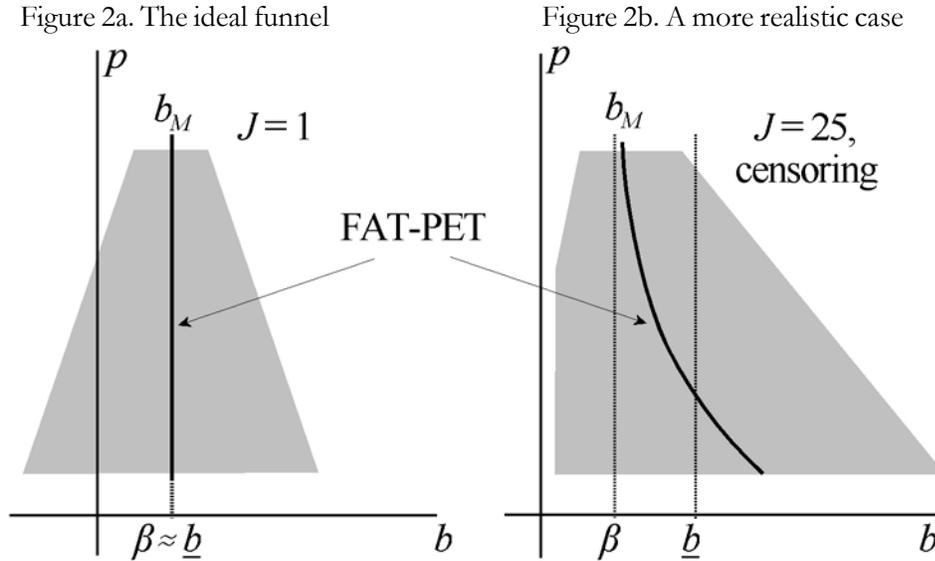


TABLE 1. Summary of meta-analysis terms

Term	Definition considering the β -literature	Explanation
Funnel	(b_i, p_i) scatter plot, where $i = 1, \dots, N$	Distribution of the N -set of estimates
Funnel width	The coefficient of variation of the N -set	Standard deviation divided by mean
Ideal funnel	The model is true and all estimates are published	Funnel is lean and symmetric
Empirical funnel	The reported estimates in the β -literature	Funnel is wide and often asymmetric
Arithmetic mean	Disregard funnel asymmetry, \underline{b} , may be weighted	Best average for ideal funnel, where $\underline{b} \approx \beta_M$
PET meta-average	Adjusts mean for funnel asymmetry, β_M	The best average if publication bias
FAT-PET MRA	$b_i = \beta_M + \beta_F/p_i + u_i$	MRA, Meta Regression Analysis
	FAT, $\beta_F \neq 0$, Funnel Asymmetry Test	FAT-PET is joint estimate of FAT and PET
	PET, $\beta_M \neq 0$, Precision Estimate Test	Assumes the asymmetry is publication bias
Publication bias	$PB = \underline{b} - \beta \approx \underline{b} - \beta_M$, when $\beta \approx \beta_M$	The publication bias is the mean of the selection bias in the individual studies

Figure 2b is a more realistic funnel where each researcher runs 25 regressions and selects one for publication. Here the choice set is wider. The situation permits the researcher to select estimates with the correct sign and high significance. The figure is a sketch of the effect of such selections. Thus, the funnel will be wider,

especially at the base. As it is censored at zero it looks asymmetric. Hence, the FAT-PET is hyperbolic, so the FAT $\beta_F < 0$ and $\underline{b} > \beta$, but the PET meta-average is still $\beta_M \approx \beta$.

The excess width and frequent asymmetry of the N -set

The width of a funnel is measured by the coefficient of variation of the N -set. Most estimates in the typical N -set have t-ratios above 2, so precision is high. Thus, one should expect funnels to be lean. Ideal funnels that occur in simulations, where $J = 1$, are indeed lean—they are also symmetrical. Ideal funnels have another property that will be used below: The distribution of the N -set is normal. We can only be sure that simulated funnels are ideal, and the normality property holds rather well for simulated funnels. In simulation experiments the variation is generated by the data, and most of the variance of empirical funnels is probably from model variation. But by the central limit theorem, the normality assumption still holds for variation generated from many sources. Below it is assumed that ideal funnels are normally distributed.

When empirical funnels are compared with the lean and symmetric ideal funnels, two major findings emerge: empirical funnels always have *excess width* and they are often *asymmetric*. Doucouliagos and Stanley (2012) compare a whole set of meta-studies showing these properties. Especially when the size of β is debated, the width becomes large. In the case where $\beta = 1$, the typical width is between 2 and 5, even when the typical t-ratio reported is above 2.

This paper requires that we guess at the coefficient of variation of the typical J -set. It is easy to give reasons to expect that the N -set varies more. The N -set is calculated from many data sets, while the typical J -set is estimated from one data set. The N -set is often made by researchers from different schools who want to find different results, while the J -set is made by one researcher. But good reasons also exist to expect that the J -set varies more. For instance, many researchers select the reported estimates (the b_p 's) after some averaging. Second, the profession often decides that a certain sign on β is the right one, as discussed in the next section. The likely result is some censoring, reducing the variation of the N -set. All said, it appears reasonable to expect that the two variances are roughly the same size.

The asymmetry indicates a problem that calls for an explanation. In principle, an asymmetry may have many explanations.⁷ From the way the asymmetry looks

7. Callot and Paldam (2011) study how funnels can become asymmetric. It appears that the funnels are robust to most standard econometric problems such as misspecification of the function, non-normal residuals, etc. To generate a significant asymmetry such problems have to be so large that it is unlikely that they are undetected. Also, funnels do become asymmetric due to randomly omitted control variables, but this asymmetry rarely looks like censoring.

one can often explain what is going on. By far the most common explanation is publication bias that looks as in Figure 2b.

From asymmetry to publication bias

Publication bias follows from the iceberg property of empirical research: Only $1/J$ of the results are reported, while the remaining $(J - 1)/J$ estimates remain unpublished and invisible. A simple question is: Is the visible part representative of the whole berg? The answer is obvious: The incentives facing the researcher will surely make him select results that are *better* than the average: That is, they are polished so that they have relatively high t-ratios, and they might be censored to conform to the priors of the author.⁸ The problem is less if many separate authors have different priors.

The profession, however, often develops a dominating body of thinking, or beliefs, taken to be *the* theory in the field. Such thinking produces a main prior which typically suggests a ‘correct’ sign on β . Authors, colleagues, editors and other gatekeepers may all have that prior. Estimates with the ‘wrong’ sign tell everybody that the research is suspect! Funnels often have appearance of having been censored accordingly. If the theory is right (as it surely is!), estimates will converge to the true β and thus have the correct sign as data samples increase. But results from small data samples should have a large variation and contain a good deal of estimates with the wrong sign. It is common to find that such estimates have suspiciously few results with the wrong sign (as in Figure 2b); see Stanley and Doucouliagos (2012, chapter 4). The asymmetry is often visible to the naked eye, and the FAT is a powerful test of asymmetry.

When the funnel has this asymmetry, the mean \bar{b} is biased by the missing values, and the bias is in the direction of the prior, so that \bar{b} is an exaggerated estimate. The PET estimate of the meta-average, β_M , corrects the mean for the publication bias. The publication bias found by formula (3) has been estimated in many meta-studies. It does have a large variance across the bodies of economic literature analyzed (see Doucouliagos and Stanley 2012), but a crude average seems to be about 2. Consequently, publication bias is not a negligible problem. The analysis offered above argues that the publication bias was smaller in the past and will be larger in the future because of the steady rise in J , unless something happens to counteract the trend.

8. Two additional bias-generators are: (i) In sponsored research, sponsor interests produce priors. (ii) An author with prior results in a field has a confirmation prior, so that relation (2) has to be estimated using clustered standard errors, with author clusters. Clustered standard errors are also used when more results are reported in the same paper.

Once a body of literature is coded, the meta-analyst can ask various questions by adding an extra variable in equation (2). Some examples are: Q1: Are results published in better journals different? Here the impact factor of each journal is added. Q2: Are the results in the A-journal different? Here an A-journal dummy is added. Q3: Are results in papers by female researchers different? Here a gender dummy is added. Such questions are common in meta-studies, and the answers for different β -literatures are not always the same. However, the answers to the three questions mentioned seem to generalize. They are: A1: No; A2: Frequently; and A3: No.

One important question to ask is how independent the data are across the studies, so it is important that variables for the data samples used are coded.

The small effect of estimators

One question that has been frequently asked deals with the effect of estimators. The author has seen most of the approximately 250 meta-studies in macroeconomics (and is co-author of 10) and has yet to see one that has found a substantial effect of any particular estimator.

A literature often unfolds as follows: Old studies in the literature use the old M_0 -estimator, and many of the new studies add a new M_1 -estimator, or bring only M_1 -estimates. When the M_1 -estimator is introduced into the literature, the initial paper shows that the results are indeed different, and this is announced as a methodological breakthrough. If the new estimator is presented as a minor honing of the coefficient, it is unlikely to be easy to publish, but journals do like major methodological breakthroughs. Many meta-analysts have learned to view such sales talk with a dose of skepticism.

Let us imagine a typical example: The β -literature has 500 estimates of β . The range of estimates is from -0.55 to $+0.95$. The two main averages are $\underline{b} = 0.40$ and $b_M = 0.20$, so the publication bias is about 2 as it is on Figure 2b. The breakthrough paper showed that while the old M_0 -estimate is 0.30 the new M_1 -estimate is 0.70, which is significantly larger. When coding the subsequent literature you soon find one paper where the M_0 -estimate is 0.50, supplemented with an M_1 -estimate of 0.45. In the next study the M_0 -estimate is -0.25 and the M_1 -estimate is -0.40 . When the M_1 -estimator has been used sufficiently often, the meta-analyst can ask if it matters by adding an M_1 dummy in regression (2). As far as I know the effect of estimators is normally small and often insignificant—it certainly is in the studies I have participated in⁹—and it *always* explains only a minor part of the variation.

9. Doucouliagos and Paldam (2011) study the effect of adjusting the aid effectiveness effect for simultaneity by this technique. Out of 1,000 estimates, 200 are adjusted. The test shows that they do not differ.

Personally, these findings from meta-analyses tally well with my experience in doing primary studies: New estimators are made to *hone* coefficients. It assumes that the researcher has a good estimate already and that it is important to chip away small to moderately sized biases. But the message from the amazing width of funnels found in the typical meta-analysis is that our knowledge is far from the honing stage in most research fields.

An assessment of the publication bias as a function of the mining ratio J

The perspective now switches from the N -set back to the J -set of estimates. They are b_j , where $j = 1, \dots, J$, from which the researcher selects one b_p for publication. This makes b_p one of the b_j 's. If it is assumed that the J -set is normally distributed, analytical results can be derived from any well defined selection rule. If the selection rule is simple, so is the result. If the size of the coefficient of variation, μ , for the J -set, was known, the orders of magnitude could be calculated. Below the guesstimate (from above) of $\mu \approx 2$ is used. When it is assumed that $\beta = 1$, it follows that $\mu = \sigma/\beta = \sigma \approx 2$, where σ is the standard deviation of the J -set.

The distribution of the J -set and the selection bias

The J -set is the set of regressions the researcher thinks contains the true estimate of β . Hence, before any of the estimates are selected the J -set is an ideal funnel, where the best estimate of β is $\beta \approx \underline{b}$. I have argued above that it is reasonable to assume that ideal funnels are normally distributed, and that it appears likely that the researcher selects a b_p that is better than \underline{b} . Hence, there is a selection bias:

$$SB = b_p - \beta \approx b_p - \underline{b}. \text{ If } \beta = 0, \text{ the selection bias is } b_p. \quad (4)$$

The selection bias is the 'micro' version, for the individual estimate, of the 'macro' publication bias of the whole literature. If everybody uses the same selection rule and J is constant, $PB = SB$. In practice PB is the mean of the SB s of the N individual studies.

For ease of presentation the J -set is sorted with b_1 as the smallest and b_J as the largest. From the normality assumption it follows that the J -set has the distribution:

$$b_j \approx \beta + \sigma\Phi((j - 0.5)/J), \text{ for } j = 1, \dots, J, \text{ where } \Phi \text{ is the inverse of the standard normal cumulative distribution function.} \quad (5)$$

This shows that b_j differs from β with $\sigma\Phi((j - 0.5)/J)$, which is the selection bias:

$$SB = \sigma\Phi((j - 0.5)/J), \text{ if the researcher selects } b_j. \quad (6)$$

Imagine that the selected regression is chosen by a well defined *selection rule*. It is possible to think of many selection rules. I shall only discuss three rules that are tractable and known to occur occasionally. This means that some average of the three may approach the actual situation, as discussed at the end of this section.

Three selection rules

Selection Rule 1

Here the researcher wants to make a completely unbiased choice. As already explained this means that he uses the following rule:

$$b_p = \underline{b} = b_{j/2} \approx \beta \text{ so that (6) yields } SB = 0, \text{ and if all researchers use Rule 1} \\ \text{also } PB = 0. \quad (7)$$

With this rule the increase in J only means that the standard deviation of the N -set falls, and it does so by the square root of J . This makes the coefficient of variation of the N -set fall by the same. The N -set funnel is ideal, and as J goes up the funnel should become leaner rather fast. This does not seem to happen, so even if some authors use Rule 1, it cannot be the dominating rule in practice.

Selection Rule 2

The researcher wants to make a good but moderate choice, so he selects b_p at a fractile $F > 0.5$, such as $F = 0.75$. It is better than β , but it might still be defensible. This will happen if the researcher finds reasons to discard half of the regressions and then selects the regression in the middle of the remaining 'good' half. Then the other regressions in the good half can be used to demonstrate the robustness of the selected one.

$$b_p \approx \beta + \sigma\Phi(F) \quad (8) \\ F = 0.75 \text{ yields } b_p \approx \beta + \sigma\Phi(0.75) = \beta + \sigma 0.674, \text{ and thus, (6) gives } SB = \\ \sigma 0.674. \text{ If } \sigma = 2, \text{ the bias is } 1.35, \text{ so that if } \beta = 1, b_p = 2.35.$$

This selection bias is independent of J . If everybody uses this selection rule it also produces a constant publication bias that is independent of J . The falling cost of

regression has no consequences for the publication bias. The increase in J only means that the precision increases. In the language of meta-analysis, the funnel is still symmetric—provided *everybody* uses the same rule. In passing it should be noted that in this ‘sensible’ case, publication bias occurs with a symmetric funnel, and the FAT-PET does not detect and correct the bias.

Selection Rule 3

The researcher selects the largest estimated coefficient as the best regression.

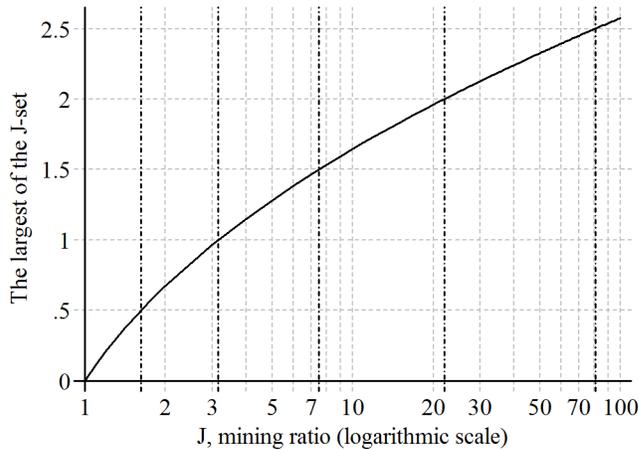
$$b_p = b_j \approx \beta + \sigma\Phi((J - 0.5)/J) = \beta + \sigma\Phi(1 - 0.5/J) \tag{9}$$

Figure 3 shows (9) for $\beta = 0$ and $\sigma = 1$. Here (6) becomes: $SB = \sigma\Phi(J - 0.5/J)$. For $\sigma = 1$ it is also the line drawn on Figure 3.

This expression depends upon J and as will be shown the dependence is quite strong. To select the largest b in the J -set is an extreme choice, but the qualitative results reported still stand as long as the selection depends on J .

Figure 3 gives the normalized path of b_j , for $\beta = 0$ and $\sigma = 1$. All other paths are reached by shifting the vertical axis to start in β and multiplying the curve by σ . To accommodate the conclusion from Figure 1 that the rise in J is log-linear, the J -axis on Figure 3 is logarithmic.

Figure 3. The path of b_j calculated from equation (9) for $\beta = 0$ and $\sigma = 1$



Note: The broken vertical lines are the intersection points for $b_j = 0.5, 1, \dots, 2.5$ reported in column (3) of Table 2.

The b_j curve rises monotonically, but we are only interested in the integer values for J , as reported in column (2) of Table 2. Column (1) gives 7 values for $b_j \cdot J$

is an integer and column (2) gives the closest matching J for each of the values of b_j , while (3) gives the value to three or four significant digits. Our standard assumption is $\sigma = 2$ as shown in column (6) in Table 2, but it is also shown what happens when $\sigma = 1$ and 3.

TABLE 2. The intersection points for b_j

(1)	(2)	(3)	(4)	(5)	(6)	(7)
Calculations from eq. (9) for $\beta = 0$ and $\sigma = 1$				Selection bias from eq. (6)		
b_j	Closest J	Exact J	Ln exact J	$\sigma = 1$	$\sigma = 2$	$\sigma = 3$
0	1	1	0	0	0	0
0.5	2	1.62	0.48	0.5	1	1.5
1	3	3.15	1.15	1	2	3
1.5	7	7.48	2.01	1.5	3	4.5
2	22	21.98	3.09	2	4	6
2.5	81	80.52	4.39	2.5	5	7.5
3	370	370.4	5.91	3	6	9

The assessment that most researchers make at least 25 regressions for each one published means that the selection bias is on the order of 2 to 6. This appears high, as the typical publication bias is only around two, but Rule 3 is extreme.

How bad is the rise in J really?

The β -literature consists of N estimates. The N estimates are likely to use a range of research strategies, corresponding to selection rules such as the ones discussed. Imagine that the researchers chose among the three rules so that the publication bias, PB , is a weighted sum of the three biases:

$$PB = 0w_1 + 1.36w_2 + 4w_3 = 1.36w_2 + 4w_3, \text{ where the } w\text{'s are weights. (10)}$$

If the weights are equal, $w_1 = w_2 = w_3 = 1/3$, the weighted sum (10) is $PB = (1.36 + 4)/3 = 1.8$, which is much like the average publication bias. This allows a crude estimate, for if J rises by 3.5 times in the next decade to about 80, this increases the bias to $(1.36 + 5)/3 = 2.1$. After another decade it is 2.5. These orders are very uncertain, but they do suggest that there is a growing problem.

Is fancier econometrics a device to reduce J ?

I think the research community will agree that the rapid rise in J poses a problem. I have often heard senior members in the research community express

misgivings about the ease with which the coefficients of the size desired can be generated by standard regression techniques, and about the inflated size of t-ratios. We all know that we are in a game that is too easy to play. One reaction is to demand graphical presentation of the data to show that the effect examined is visible to the naked eye; some demand robustness; others turn to theory and demand a rigorous derivation from first principles; and still others demand fancy estimators.

The core insight is that a new empirical result can be trusted only after enough independent replication. This is precisely where meta-studies come in. They become increasingly important—for with a high J it becomes easier to estimate something that looks like a replication on new data. In order to be credible, replications using new data should be made with *precisely* the same model, and be done by another researcher. In Paldam (2012) I discuss a case where a new and very nice theoretical model was empirically confirmed on a data set that proved to be an outlier. The first paper was so convincing that it managed to establish a widespread prior about the right signs.¹⁰ The signs survived seven independent replications, by researchers who managed to find models that looked almost like the first model and generated the right signs, before it broke down after eight replications. The lesson is that, as J rises, the number of independent replications necessary for a result to be credible rises.

However, it is my impression is that the most widespread reaction to the problem is to increase the costs of regressions by demanding that techniques be fancier, so that the costs of regressions rise and J falls. Due to the rapid development of estimators, the old method M_0 soon changes to M_1, M_2, \dots, M_t . Maybe the key reason to demand the latest M_t -estimator be used is not the small improvement it gives to the estimate, but the increase in cost caused by the use of the new estimator. The costs are high if the M_t -estimator is not (yet) included in the standard econometric package available to the researcher. He will then have to make an effort to master the new technique and to get it to run on his PC.

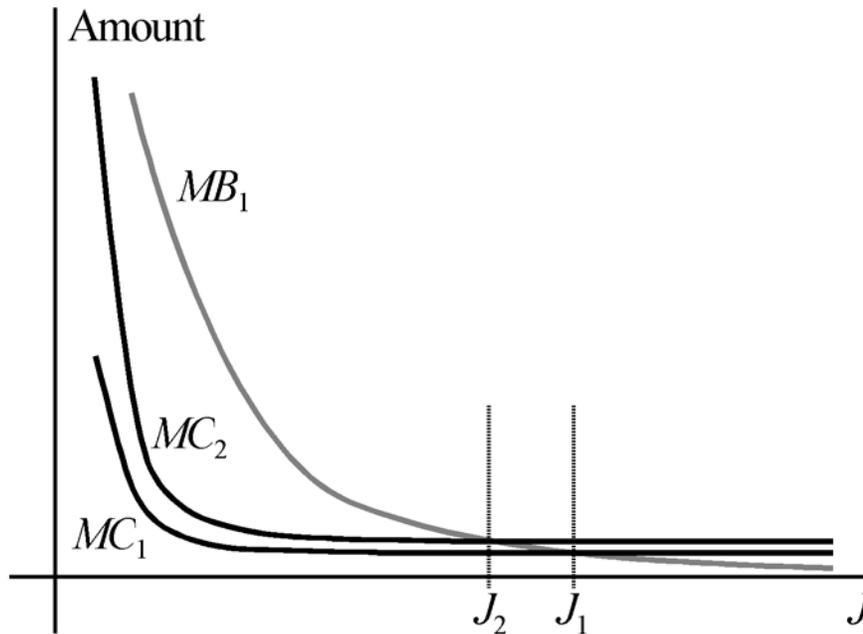
Figure 4 shows what happens when editors and referees demand that a new estimator is used. Here MC_1 is the costs of using the same estimator as before. It is the same as on Figure 1, though the vertical axis is compressed a bit. MC_2 is the costs of using the new estimator. The demand for the new estimates increases the initial costs of running regressions; but once you get the M_t -estimator to run on your PC, the marginal costs soon decrease almost to normal. So the MC_2 curve is above the MC_1 curve, but mostly in the beginning, i.e., J_2 is smaller than J_1 , but not much smaller.

If the new estimator has a honing effect only, the pattern found by the two estimators is much the same, and the researcher will typically do the J experiments

10. It is also possible that the authors of the first paper were referees to the most of the following papers.

with the old estimator and then just re-estimate the selected estimates with the new one. Thus, the researcher may just make a data set ready and seek the assistance of an econometrician who knows the new estimator.

Figure 4. The effect of demanding that a new estimator is used



Note: MC_1 and MB_1 are as on Figure 1. The new estimator changes MC_1 to MC_2 .

The market for econometric packages is a worldwide competitive market where the leadership edge can be lost quite quickly. Hence, if the companies producing these programs discover that there is a demand for the M_r -estimator, it is soon included in the package, or somebody writes an add-on program that you can use to run M_r -estimates in your favorite package. I have recently changed from Stata 10 to Stata 12. The inclusion of new estimators is quite impressive, and StataCorp (located in Texas) supports the community of Stata users by providing an exchange of add-on codes, which allows access to a world of estimators.

My own experiences are that when a new estimator is introduced in a literature it takes perhaps a year before the profession demands that it be used in the relevant papers.¹¹ And then it takes perhaps another half year before it is included in the standard econometric packages. There is a window of opportunity where the

11. It is not enough that a certain estimator is presented in a paper about econometric theory. However, once somebody starts to use it, it quickly becomes the new standard.

M_T -estimator can be used to reduce J , but it is not a large window. Once the M_T -estimator is included, the MC_2 curve falls again.

But there is an effect till the window closes. Thus the development of the M_T -estimator has served a useful purpose. The small (and falling) size of the window is somewhat countered by the increase in the production of new and still fancier estimators. It is necessary for the producers of econometric packages to be behind if publication bias is to be kept at bay.

It is arguable that the theory in the previous section—though it explains something—does not explain enough. It seems to me that the outpouring of new estimators is bigger than necessary to reduce J . An additional theory of why ever-fancier techniques are developed comes from Mike Felgenhauer and Elisabeth Schulte (2011). The key idea is that to increase the credibility of papers, authors have to put something at stake. Well known authors can use their reputation, but young authors have not accumulated enough reputation, so they must do something else: They show that they can jump through high econometric hoops. Perhaps the jumping through the high hoops is a signal of their ability, their value to the research community, and their dedication. Since it takes a lot of effort and time to learn these jumps, they have put all that work at stake.

Conclusions

This paper deals with the mining ratio J , which is the number of regressions made for each published. By necessity the mining ratio J reflects activities that are private information to the individual authors. But as the costs of regressions keep decreasing, the mining ratio must increase. I have suggested that the incentives on authors cause authors to select the best of their regressions for publication. Hence, there is a selection bias, and with a larger set of regressions to select from the selection bias will rise.

The average selection bias is the publication bias. Meta-analysis normally finds that funnels are much wider than expected from the t-ratios reported. Often they are also asymmetric in ways suggesting publication bias. It follows that publication bias is common in economics. The increase in the bias depends on the rules by which researchers select which regressions to publish.

Some of the excess variation is due to the introduction over time of new estimators. Yet another observation from meta-analysis is that changing estimators explain little of the variation in the typical economic literature. It appears that the efforts made to improve estimators seem to be excessive relative to the benefits, at least in macroeconomics.

All this poses something of a paradox and leads me to suggest that perhaps one reason to welcome ever fancier estimators is the effect they may have in slowing down the alarming rise in the mining ratio.

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Why Is There No Milton Friedman Today? A Symposium Prologue

Daniel B. Klein¹

[LINK TO ABSTRACT](#)

*Sitting on a sofa
On a Sunday afternoon,
Going to the candidates' debate,
Laugh about it,
Shout about it,
When you've got to choose,
Every way you look at this you lose.*

*Where have you gone, Joe DiMaggio?
A nation turns its lonely eyes to you
Ooo ooo ooo.*

*What's that you say, Mrs. Robinson?
'Joltin' Joe has left and gone away'
Hey hey hey, hey hey hey*

—Paul Simon

Something in Simon's song speaks to the subject of our symposium. Sadness was expressed in the invitation to participate, when we prompted the recipient to address, in up to 2,500 words, the question: Why is there no Milton Friedman today? That invitation is reproduced, with a few very minor edits, below. The

1. George Mason University, Fairfax, VA 22030.

reference list at the end of this document contains links to the symposium contributions, written by John Blundell, David Colander, Tyler Cowen, Richard Epstein, James K. Galbraith, J. Daniel Hammond, David R. Henderson, Daniel Houser, Steven Medema, Sam Peltzman, Richard Posner, and Robert Solow. We are very grateful to each. We are also grateful to the Social Change Project and its director Claire Louise Morgan at the Mercatus Center at George Mason University for support for this project.

Milton Friedman was a person of many remarkable features and areas of great accomplishment:

1. He achieved very high standing in the economics profession, perhaps as high as anyone in his day.
2. He was very active in public discourse, and very influential; he wrote several popular books and many pieces for magazines and newspapers. Besides being effective in the medium of the written word, he was effective in front of the camera, in interview or lecture. Many people feel that he was exceptionally appealing, as well as persuasive.
3. He propounded a classical liberal point of view, which maintains a presumption of liberty. He brought this point of view to a wide variety of issues, both macro and micro; he was open to questions on virtually any sort of policy issue. In the United States, he became the leading figure of the free-market, small-government outlook.
4. The professional respect for him was quite general, in the sense that it existed even among many economists who did not particularly agree with his policy views.
5. His great influence extended well beyond the United States. He was renowned internationally, in research and scholarship, and in public discourse over politics and policy.

A continuing wide regard for Friedman is evidenced by a 2010 survey of economics professors. Respondents were asked, in an open-ended format, to name their favorite economists of the twentieth century then deceased. Friedman was a close second to John Maynard Keynes. When the survey respondents are sorted by voting preference, we find that Friedman, besides being far out front among economics professors who vote Republican, was third (behind Keynes and Paul Samuelson) among those who vote Democratic (Klein et al. 2013, 123).

WHY IS THERE NO MILTON FRIEDMAN TODAY?

That 2010 survey of economics professors also asked them to name their favorite living economists. The results support the notion that no one today is close to being a Milton Friedman, in the features listed above (Davis et al. 2011, 137-139).

This symposium asks: Why is there no Milton Friedman today?

The question is important to those of a classical liberal outlook, because it is important that an outlook have leading figures who are highly regarded in the culture generally, and especially within the realms of science, scholarship, and social thought.

One logical answer to the question might be that Milton Friedman was simply that exceptional a human being; he was, as it were, several standard deviations above the mean in many important dimensions, and that happens only once or twice a century.

One way to assess that answer would be by a thought experiment: Suppose Milton Friedman were born during the 1960s or 1970s. Could one imagine that person emerging as a *Milton Friedman* as we understand the concept?

Another logical answer might be that Milton Friedman was, in fact, wrong about a lot of things, and about free-market, small-government liberalism in general, and it took some time to refute his ideas, which now remain justly diminished.

The symposium asks you to speak to the question from any angle that seems appropriate to you. As the question has an historical aspect, explanations may themselves call for explanation: If the age during which Friedman was in his prime was an age that differs from the present age, what is the difference? Also, what explains the difference between the ages?

The main question for this symposium prompts other questions, including: Can a Milton Friedman emerge in the near future?, and, Does it matter whether there are great figures like Milton Friedman? Furthermore, the main question might be considered in a form that presupposes a stronger premise: Why is there no one today anywhere near to being a Milton Friedman? The symposium welcomes your open-ended reflections on these or any questions that you find to be related.

As the question is broad and cultural, and as authors are being given only a rather short time period to write their contributions, the style of the contributions is expected to be informal and speculative, and the contributions themselves to be relatively brief.

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Where Is the Next Rose Director?

John Blundell¹

[LINK TO ABSTRACT](#)

“Behind every great man there is a great woman” has to be close to being the most hackneyed expression in the English language, but in the case of Milton Friedman one has to give really very serious credit to Rose Director, his wife.

They met in graduate school at Chicago when Jacob Viner sat his students alphabetically and there was nobody between Director and Friedman. She was a very young sister of Aaron Director (founder with Ronald Coase of the *Journal of Law and Economics*), but older brother was more of a mentor and father figure.

She co-authored with Milton the international best seller *Free to Choose* (1980) and co-produced the resulting ten-part TV series which showed all over the world (but not in France). It aired just as Margaret Thatcher and Ronald Reagan came to power. It caught a wave, but a wave the Friedmans had helped to generate. To Cato Institute founder Ed Crane, *Free to Choose* “really kick-started” the rebirth of classical liberalism in the U.S. (Irwin 2009).

When Milton turned 90, President Bush invited him and Rose to lunch at the White House and joked openly that the only person he (Milton) had ever lost an argument with was his wife (Bush 2002).² And on the 100th anniversary of Milton’s birth a London paper, *The Daily Telegraph*, ran this headline over my op-ed of appreciation: “Milton Friedman was peerless until he met his wife” (Blundell 2012).

Of the two, Rose was always the feistier and more libertarian policy person. Gary Becker once commented: “It was an extremely close intellectual fellowship, and she was not someone who got credit for things she didn’t do. They discussed ideas constantly. Her feelings about the importance of private markets, opposition

1. Institute of Economic Affairs, London SW1P 3LB, UK.

2. Milton at first refused the White House invite. Ed Crane had to call him, and Cato kindly took care of all the details to ship them from one coast to the other.

to big government, were even stronger than his. Her lasting influence will be as a collaborator, but she was a major contributor to the collaboration, and that's a significant legacy" (Weber 2009).

And Rose never felt in awe of Milton. When asked if she ever felt overshadowed she answered: "No. I've always felt that I'm responsible for at least half of what he's gotten... I feel that I have much of the responsibility for his success" (Robinson 1999).

While Milton is best known as a monetary theorist (his license plate was "MV PT"), and as proponent of the all-volunteer army through his work for President Nixon, school vouchers, and market ideas generally, he commented that his 1957 book *A Theory of the Consumption Function* was "my best purely scientific contribution" (Friedman and Friedman 1999, 222).³ The work was a challenge to Keynes. It was developed in conversations with Rose, who had worked on household consumption with Dorothy Brady and Margaret Reid. Indeed in the preface Milton wrote that the book was: "in essential respects a joint product of the group, each member of which not only participated in its development but read and criticized the manuscript in its various stages" (Friedman 1957, ix).

Milton's first major foray into popular writing was *Capitalism and Freedom* (1962). Rose took his notes from a series of lectures at Wabash College, Indiana, sponsored by the Volker Fund, and as Milton wrote in the preface: "She pieced together the scraps of the various lectures, coalesced different versions, translated lectures into something approaching written English, and has throughout been the driving force in getting the book finished" (Friedman 1962, xvi). It sold 500,000 copies, was translated 18 times, and through samizdat copies helped tear down the wall (Doherty 2007, 301), along with works by Hayek and Mises, as Rose was later to note. It gave them both a lot of pride and satisfaction.

After retiring from Chicago, moving to 1750 Taylor Street in San Francisco, and winning the 1976 Nobel Prize in Economics, Milton was approached to make a TV series by WQLN. He was not that keen, preferring reading to TV and having a Hayekian preference for reaching intellectuals, not masses. But Rose had other ideas and she prevailed as a project planned to last 18 months now dominated their lives for a full four years.

WQLN, however, was a PBS station. Neither Friedman wanted tax dollars involved, so they wrote out a list of wealthy folk they knew, and over \$10 million (in today's money) was quickly raised. Through Ralph Harris (Lord Harris of High Cross), whom I succeeded as Director General of London's Institute of Economic Affairs, WQLN found a private production company in England headed by Tony

3. Milton also called the central tenet of the book—that current consumption is not driven by current income but expected lifetime income—"embarrassingly obvious" (Friedman and Friedman 1999, 225).

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Jay (Sir Antony Jay), who knew public choice economics.⁴ He was soon to make *Yes, Minister* and *Yes, Prime Minister*, reputedly Hayek's and Thatcher's favorite TV programs. Daughter-in-law Esca Hayek has fond memories of F. A. Hayek laughing out loud at the exploits of the bureaucracy.

As I wrote in *Ladies for Liberty*, "Rose was deeply involved, steeped in the ideological and logistical aspects of the whole project as well as the psychology of building a team that could be open and honest yet constructive as they struggled to bring complex ideas to the television screen. It was, after all, to be presented by somebody who, while an old hand at televised interviews and debates, had to date never experienced the unnerving sensation of looking directly into a camera" (Blundell 2011, 180).

Free to Choose was billed as "A Personal Statement." Eben Wilson, one of the production team, told me: "In fact the views were those of Milton adjusted by Rose and edited by both. When Milton spoke to camera what you heard was the result of a fascinating brainstorming session involving both of them. Logic, language and ideas were battered into shape on location and without a script" (Blundell 2011, 181). A quiet cough, a murmur, a raised eyebrow by Rose was all it took to stop filming. Milton and Rose would huddle and thrash through the economics. And if they agreed to disagree he would deliver his lines terribly, "so Rose always won" (182).

Of *Free to Choose*, Milton said: "Her title as associate producer was far more than a formality. She played an indispensable role: she participated in every planning session and every editing session; she was on every shoot and involved in every discussion about the content of my statements to the camera; she was the best critic of my performance, and perhaps more important, the only one willing to be blunt in criticizing me, and the most helpful in setting me on the right track" (Ebenstein 2007, 204).

It was Rose who then took the ten TV programs and turned them into the eponymous book. It was the #3 best-selling nonfiction book of 1980 according to *Publishers Weekly*; it sold 400,000 in hardcover and over one million in paperback with 17 translations and who knows how many samizdat copies (Cassidy 2009, 80). In Japan alone a staggering 200,000 hardbacks flew out of the doors.

The TV series became a phenomenon beating out *Masterpiece Theatre*. Can we today conceive of any economist getting higher ratings than *Downton Abbey*? Market penetration was so high that when Queen Elizabeth II met Milton at a party she hosted on her boat in San Francisco harbor she proclaimed "I know you."

4. Jay had read Buchanan and Tullock and Niskanen through the IEA publication program (Blundell and Robinson 1999).

Philip [her husband the Duke of Edinburgh] is always watching you on the telly” (Friedman and Friedman 1999, 569).

The Friedmans tried to follow up on *Free to Choose* with a low-budget program and book, *Tyranny of the Status Quo* (1984), but it was no match and fizzled out. It was probably a mistake.

So my question actually is not “Where is the next Milton Friedman?” but rather “Where is the next Rose Director?”

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Why Are There No Milton Friedmans Today?

David Colander¹

[LINK TO ABSTRACT](#)

The question of why are there no Milton Friedmans today can be approached in a number of different ways. For example, it might be approached as a question about economists' role as public intellectuals: Are there fewer economist public intellectuals today than previously, and if so why? Alternatively, it might be approached as a question about Friedman's support of free markets: Are there fewer economists who support free markets today, and, if so, why? I will provide brief answers for each.

Are there fewer economist public intellectuals today than previously, and if so, why?

There are fewer public intellectual economists today, and the reason why is that they are filtered out of the profession. If Milton Friedman at age 21 were somehow time transported to today and was thinking about going to a top graduate school in economics, he either would not apply, or would be rejected if he did apply. The reality is that, every year, thirty or so future potential Milton Friedmans think of applying to graduate school in economics. Twenty-eight decide against it; two decide to apply and are rejected. So the answer to the question of why there are no Milton Friedmans today is that there are Milton Friedmans; they just aren't going into economics. The skills he had are not the skills that are most valued by

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the profession today, and the profession is much more efficient at selecting for the skills it values than it was previously.

Sherwood Rosen once told me that I had discouraged more people from becoming an economist than any single person alive. The reason was my work on the nature of graduate economic education, such as the article “The Making of an Economist” (Colander and Klamer 1987). In that article, Arjo Klamer and I described economic training in the late 1970s and early 1980s. This work made clear to prospective students that graduate economic education was quite different from undergraduate economic education; it was much more mathematical, much less focused on policy and institutions, and much more focused on preparing students to write academic articles for other economists.

The reason why that knowledge discouraged students from becoming economists is that most students do not want to be efficient writers of journal articles. That is an unusual taste that must be inculcated into students through years of problem sets and brainwashing. Before our work on the profession, there was a mismatch between what incoming students thought they would be getting in graduate school in economics—general discussions about policy that built on their undergraduate training—and what they actually got—a boot camp in mathematics and techniques. The result was a mismatch of interests—the training students got did not match their interests. The result was a discontentment among many of the then-young economists such as myself. We still became economists, but we were never fully indoctrinated into the academic economics rituals.

The diffusion of information about the nature of graduate school has reduced that mismatch. Today, just about anyone applying to a top graduate school knows what it will be, and the top graduate schools know the skills they want, making the selection process much more efficient. Those who become economists are much more likely to want to be efficient writers of journal articles. They are much better trained at writing those articles than were earlier cohorts. The flip side is that they are much worse trained to be public intellectuals, which is why there are fewer economist public intellectuals such as Milton Friedman today than there were earlier.

Are there fewer economists who support free markets today, and if so, why?

Let me now turn to the second sub-question: Are there fewer free market supporters than there were, and if there are, why? The answer to this question seems to differ among economists. When I am with rightish economists I detect

WHY ARE THERE NO MILTON FRIEDMANS TODAY?

a belief that there are fewer free market supporters than there used to be. But when I am with leftish economists, I detect the opposite belief; they think that there are more free market supporters. I suspect that neither is right—the views of economists about free markets are diverse, have always been diverse, and are likely no more or less diverse than they were in the past. This is as it should be. An economist's view on markets often reflects her morality and ideology, and her pragmatic assessments of institutions and politics, much more than any conclusions of economic theory. Good economic theory does not lead one to support or oppose free markets as a matter of policy; instead, as J. M. Keynes (1922) pointed out, economic theory provides one with “a technique of thinking, which helps its possessor to draw correct conclusions.”

Thinking about economic theory as something that does not directly relate to one's view of policy was part of the nuanced view of markets inherent in the classical liberal tradition that the pre-Friedman Chicago tradition of Frank Knight and Henry Simons held dear. Economists might, and generally did, hold strong views on policy issues, but those views were separated from their views of economic theory. Policy views were not derived from economic theory.

That classical liberal tradition was coming under fire in the 1940s as economists started framing policy analysis within a Walrasian model in which the role of the state was to maximize a social welfare function. This Walrasian neoclassical approach integrated policy into theory in a way that had not existed before; it made it seem as if economic theory called for state intervention. Within this new Walrasian neoclassical approach to policy, which I call the “economics of control” approach, the existence of externalities was seen as a theoretical reason for government interventions into markets.

This Walrasian neoclassical policy approach was a significant movement away from the classical liberal approach to policy for two reasons. The first was that it blended scientific theory and policy methodologically and did not keep the strict separation that classical liberal economists maintained. The second was that it was much more open to government intervention, as it did not focus on—or even disregarded—the moral and practical reasons that led most classical liberals to support markets as a matter of policy regardless of their ideology. As I discuss in a paper with Craig Freedman (2011), the classical liberal response was to argue that the neoclassical method was wrong; economic theory does not lead to policy conclusions.

That response was not doing well within the competition for ideas within the economics profession at the time. It was portrayed as unscientific and old-fashioned. Milton Friedman and George Stigler developed an alternative response. Using a variation of Ronald Coase's argument, they argued that positive economic theory, supplemented by correct empirical analysis, led to the scientific conclusion

that the government should not enter into the market. Like those who were using the Walrasian neoclassical approach, they blended theory and policy together and thereby abandoned the classical liberal methodological tradition. (This argument is developed more fully in Colander and Freedman 2011.)

This integration of theory and policy meant that debates about policy became intertwined with debates about economic theory. The practical and moral reasons that classical liberals supported free markets as a matter of policy were given far less focus, and positions on economic policy issues became blurred. Instead of debating the deeper philosophical, practical, and moral arguments for and against markets, the debates became structured around technical models. These debates were largely removed from the real issues in debate, so the separation made it seem to both sides as if the other side put greater weight on certain theoretical arguments than they actually had. Thus, whereas one could say that most classical liberals of the earlier era supported markets on practical and moral grounds, even as they recognized the advantages and disadvantages of markets on theoretical grounds, one cannot say what the views of most modern economists are.

My sense is that there has been little change in the percentage of economists who support markets over the last fifty years. But that is only a guess; given the blending of theory and policy, it is hard to tell what they support, which is why this second question, “Are there fewer economists who support free markets today?” is answered so differently by different economists.

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Why Is There No New Milton Friedman Today?

Tyler Cowen¹

[LINK TO ABSTRACT](#)

If I approach this question from a more general angle of cultural history, I find the diminution of superstars in particular areas not very surprising. As early as the 18th century, David Hume (1742, 135-137) and other writers in the Scottish tradition suggested that, in a given field, the presence of superstars eventually would diminish (Cowen 1998, 75-76). New creators would do tweaks at the margin, but once the fundamental contributions have been made superstars decline in their relative luster.

In the world of popular music I find that no creators in the last twenty-five years have attained the iconic status of the Beatles, the Rolling Stones, Bob Dylan, or Michael Jackson. At the same time, it is quite plausible to believe there are as many or more good songs on the radio today as back then. American artists seem to have peaked in enduring iconic value with Andy Warhol and Jasper Johns and Roy Lichtenstein, mostly dating from the 1960s.

In technical economics, I see a peak with Paul Samuelson and Kenneth Arrow and some of the core developments in game theory. Since then there are fewer iconic figures being generated in this area of research, even though there are plenty of accomplished papers being published.

The claim is not that progress stops, but rather its most visible and most iconic manifestations in particular individuals seem to have peak periods followed by declines in any such manifestation. In essence the low-hanging fruit for the production of fame becomes much harder to find. Probably there will never be

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another geometer as fundamental as Euclid and no economist to rival the reputation of Adam Smith as a founder of the discipline.

Newer areas still seem to be generating iconic superstars up through the current day. Bill Gates, Steve Jobs, and Mark Zuckerberg stand good chances of going down as world-historic innovators and perhaps thirty years from now we will be asking why there is no new Steve Jobs.

Now let's apply this to Milton Friedman. The world made significant advances in liberty in the 1980s and 1990s. Friedman's career covered that period and indeed he was an important figure for many pro-liberty movements. At the very least he was articulating the vision of liberty which many people were fighting for, even if they were not influenced by him in every case. For 'freedom fighting,' at that time, and moving toward markets, he attained iconic status.

Moving to another sphere, I think of the academic world of economics as having made quite significant advances in theory in a period something like 1920 to 1980, and then slowing down. Friedman's academic career developed over those years except for the very early segment. So he was very close to peak icon-producing times on the academic side as well. Friedman circa 1953 could write a fundamental essay like "The Methodology of Positive Economics," or restate the quantity theory of money (1956), in a way that today is no longer possible to do with such far-reaching impact.

By writing that, I don't mean to take anything away from Friedman's truly impressive achievements at the personal, research, and outreach levels. But he was also a person who came along at the right moment doing the right things. If Friedman had been born in 1985 and was starting his career now, or even born in 1965 and starting his career twenty years ago, he would do very very well as an economist but he also would find it much harder to reach the same kind of iconic status.

In addition to overlapping with the fall of communism, Friedman's peak academic influence coincided with a period when the world moved to floating exchange rates, which he advocated and even helped design, and coincided with when the United States experienced stagflation, which he had predicted. That is again wisdom on his part, but had economic policy been better in the first place Friedman would not have become nearly as iconic, so there is a role for random forces as well.

In some respects, if there is a Milton Friedman of today, it is Paul Krugman, who both has a Nobel Prize and has a very large popular audience and considerable skills as a communicator. Of course Friedman's contributions as an economist were far more fundamental. Arguably Friedman deserves three or four distinct Nobel Prizes, while no one would say the same about Krugman, even though most of his serious critics readily would grant he deserves the one.

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What about the differences in political orientation? The great policy battles of Friedman's day were defeating communism and planning, moving away from naive Keynesianism, privatizing, and overcoming an excessive belief in regulation. And today what goals are *perceived* (correctly or not) as comparably important? Improving income inequality, fixing health care, and reining in the banks. The cynic might toss in 'fighting austerity and returning to naive Keynesianism.' It should be no surprise that today's closest equivalent to Milton Friedman—in terms of being an iconic, popular, Nobel Prize-winning economist—should come from the left rather than from a conservative or a libertarian viewpoint.

In short, I don't expect we will be seeing another Milton Friedman anytime soon.

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Why There Is No Milton Friedman Today

Richard A. Epstein¹

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I think that the Prologue to this symposium is right to suggest that Milton Friedman holds a distinctive place in the economic history of the 20th century—one that is not likely to be replicated in the 21st century. The Prologue reports on a survey (Davis et al. 2011) that ranks Friedman second after John Maynard Keynes in terms of his influence. I think that this misstates the relative influence of the two men. The quoted survey polled economics professors, who are quite likely to give greater weight to the macroeconomic issues that fall in Keynes's bailiwick. Friedman's influence is likely even greater among the general public and academics in other fields, who might find it difficult to grasp macroeconomic issues yet find it much easier to relate to the mid-level issues of regulation to which Friedman, with his classical liberal orientation, devoted far more attention.

Concentrate, therefore, on the broader academic and public audience, and it seems that Friedman, especially in the United States, had far greater reach and influence than did Keynes. Friedman (1912-2006) lived over 30 years longer than did Keynes (1883-1946), and he died 60 years later. Longer duration and more recent participation give his work greater public salience today. Indeed, for much of his long life, he was able to bring his works into popular view. *Free to Choose*, his popular TV series ([link](#)), began airing in 1980, when he was 68 years old. It has the clear title that instantly locates him on the general economic spectrum; distracting philosophical ripostes about whether people should be free to choose to murder other people just don't work in popular markets where folks understand the implicit side constraints on individual freedom of action.

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That same forceful presence was equally apparent in his highly popular *Newsweek* columns written between 1966 and 1984, when he was between 54 and 72 years old. The media at that time had greater penetration than it did in the 1930s when Keynes was at his professional peak, so Friedman had greater exposure, which he put to maximum use. Even today, it is possible to see his real intellectual power in, for example, his scathing critique of gasoline price controls (Friedman 1974). Friedman also took aim at other large targets. I remember his powerful comparison of New Hampshire versus Vermont, arguing that lower levels of regulation allowed one to outperform the other (Friedman 1976).

The question posed by this symposium is perhaps directed more to the question of why there is no Friedman today, but part of that question asks why there are no Keyneses, no Hayeks, no Wittgensteins, no Russells, no Eliots, or other giants who once strode the earth. Putting the question that way makes it clear that a large part of the explanation does not depend on the peculiar genius of Milton Friedman or indeed any of the great scholars who were dominant when I came of age in the 1950s and 1960s.

In part that decline is explained by the same market forces that led to the decline of *Life* and, ironically, *Newsweek*, which no longer runs a print edition. The large all-purpose magazine—or scholar—is one that thrives in an environment in which there is not a huge base of technical knowledge that one must master in order to participate in a particular field. Once the level of sophistication goes up in any field, specialization starts to exert its influence. Niche players claim greater expertise in particular areas, and they start to push the all-purpose stars to the side. That tendency is accentuated in economics as high-powered mathematics and large data sets gain prominence. They make it ever more difficult for any person to be expert in more than one or two subfields of inquiry. The demands of the profession influence the kinds of students who enter into it, so that free-spirited intellectuals like Milton Friedman are less likely to be drawn to the field than they were 100 or even 50 years ago.

These trends change the shape of the overall landscape, which no longer contains one single large mountain that is surrounded by foothills. Instead, it becomes a mountain range with many peaks, none of which reach the greatest heights. That has happened in virtually all fields of learning. Specialized knowledge moves to the fore. With the rise of sophisticated mathematics and heavy empirical work, it is hard to find some newly minted generalist whose solid grasp of first principles allows him to move as easily as Milton Friedman did from price controls to education to monetary theory and anything else. Ironically, it is only lawyers (and perhaps journalists) who have that kind of range, and even we lawyers lack one key feature that Friedman and others had. We are not the inventors of these great theories. We are only their relatively clear modern expositors.

There is yet another reason for Friedman's greatness that reflects more on him and on the set of problems that he faced. Friedman came of age in the 1930s, which provided him with the best possible laboratory to see how government could pervert the operation of competitive markets by introducing regulation that entrenched cartels that would not have survived without government backing. It is not my purpose here to explain in detail how that process took place (see Epstein 2006), but it is at least useful to indicate some of the major changes in that direction. The exemption of labor unions and agricultural cooperatives from the sanctions of the antitrust law under the Clayton Act of 1914 is one of the early moves.² The constitutional approvals of rent control (*Block v. Hirsh* 1921) and zoning (*Euclid v. Ambler* 1926) were followed by the passage of the Agricultural Adjustment Acts of the 1930s,³ the National Labor Relations Act of 1935⁴ and the Fair Labor Standards Act,⁵ which covered minimum wages and overtime. The Motor Carrier Act of 1935⁶ introduced an extensive level of cartelization on public highways, and the passage of the Federal Communications Act of 1934⁷ strengthened the same tendency in the allocation of broadcast frequencies, where administrative approaches pushed aside any use of a bidding system.

Milton Friedman and his band of fellow economists had little or no interest in the constitutional challenges that these statutes posed to what I call the Classical Liberal Constitution, which had dominated before the rise of the Progressive period (see Epstein forthcoming). But these new innovations presented a huge opportunity to explain in vivid terms how government intervention scored the rare double of raising administrative costs while lowering overall market efficiency.⁸ Friedman was the master in the public space in dealing with these issues, and it was that work that generated his public fame, not the more technical work on

2. Clayton Act, ch. 323, § 6, 38 Stat. 730, 731 (1914) (codified at 15 U.S.C. § 17) ("Nothing contained in the antitrust laws shall be construed to forbid the existence and operation of labor, agricultural, or horticultural organizations...nor shall such organizations, or the members thereof, be held or construed to be illegal combinations or conspiracies in restraint of trade, under the antitrust laws.")

3. Agricultural Adjustment Act, ch. 25, 48 Stat. 31 (1933) (held unconstitutional in *U.S. v. Butler* (1936)); Agricultural Adjustment Act, ch. 30, 52 Stat. 31 (1938) (codified as amended in scattered sections of 7 U.S.C.).

4. National Labor Relations Act, ch. 372, 49 Stat. 449 (1935) (codified as amended at 29 U.S.C. § 151–169).

5. Fair Labor Standards Act, ch. 676, 52 Stat. 1060 (1938) (codified as amended at 29 U.S.C. 201–219).

6. Motor Carrier Act, ch. 498, 49 Stat. 543 (1935) (codified as amended in scattered sections of 49 U.S.C.).

7. Communications Act, ch. 652, 48 Stat. 1064 (1934) (codified as amended in scattered sections of 47 U.S.C.).

8. Later on Friedman did extend his work to educational vouchers as an imaginative way in which public funding could coexist with competitive markets that allowed parents to displace school boards in choosing their children's education (Friedman 1962). That conviction led him and Rose Director Friedman to create the Friedman Foundation for Educational Choice, which is still active today.

the consumption function or the history of monetary policy, which secured his reputation among the professional economists.

In the current setting, it is very hard for any economist to top Friedman for his force and elegance in slaying the modern incarnations and updates of the 1930s legislation. The overall growth in the size of the profession will also make it far more difficult for any person to separate himself from the pack. To compound matters, the issues that have come to the fore on the professional side, dealing with such matters as communications interconnection, two-sided markets in credit cards, or optimal extraction regimes for common pool resources, have a very different resonance because they require setting out the proper rules for dealing with complex markets where it is not possible to identify some definitive competitive equilibrium of the type to which Friedman naturally gravitated. Of course, those issues predate the rise of Friedman because they hit the courts with great intensity in the period between 1890 and 1945 in connection with the regulation of network industries like railroads and natural monopolies like electric and gas companies, where it is only possible to choose among second-best solutions. But as best I can tell, Friedman never wrote about those issues.

In a sense he was fortunate because these areas of current study all involve complex tradeoffs in the difficult effort to find which of a number of second-best solutions should be adopted in practice. The moment that one speaks about second-best, the incredible public clarity that Friedman demonstrated in his own popular writings is no longer achievable, even in his own work that came to recognize the difficulties with any state-run currency. We have, therefore, a situation today where the state of public regulation and the state of the academic profession are not aligned in ways that are likely to encourage the emergence of a new titan. Exemplars like Friedman and Keynes are not likely to appear in the near future, and we are in a sense all the poorer for that loss.

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Mistah Friedman? He Dead.

James K. Galbraith¹

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I knew Milton Friedman slightly. We met on three occasions. Once was at my parents' home in Cambridge in the late 1970s, to my surprise since I didn't suspect that my father and he were on speaking terms. Once was by happenstance at Alta in 1983; we skied together for three days. And once in 1990 he invited me to debate, along with David Brooks, for the re-release of the first hour of *Free to Choose*.

I think there were two Milton Friedmans. One was an economist in the Age of Keynes, but in that special enclave, Chicago, which rejected every part of the New Deal including the Wagner Act, Social Security, public works and jobs programs, and also the early postwar resolve that "maximum employment, production and purchasing power" should be our national goal. In those years, too, Swedish social democracy and the British move to national health care were models, the next big thing in many American eyes. In protected isolation and non-conformity, Chicago rejected them all.

At that moment, Friedman's genius lay in his line of attack. He did not reject Keynesian models outright; rather he probed for vulnerability to clever modification. Though he had published essays tying economics to logical positivism, hypothesis testing and evidence had nothing to do with his method here, which was that of the pure thought experiment. Two episodes stand out in memory: those of the permanent income hypothesis and the effect of adaptive expectations on the Phillips curve.

The permanent income hypothesis asserted that one-time windfalls would be treated as transient and saved. Hence there would be no multiplier effect. The point to notice is that this argument accepts Keynes's assault on the virtue of saving and the structure of the consumption function. It merely denies a causal link between transient income and the marginal propensity to consume. The Keynesian theory is

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now warped so as to yield an anti-Keynesian result, and Friedman thus established a powerful case against stimulus programs, especially tax rebates and other one-time cash transfers.

The Samuelson-Solow Phillips curve view that one could have a permanent reduction of unemployment for a modest increase in the inflation rate was based (roughly) on demand-pull wage bargaining. Again, Friedman accepted all of this. He merely argued that the theory was inadequate, for it assumed money illusion. Adaptive expectations were a rough-and-ready approach to modeling foresight and shedding money illusion. Adaptive expectations generate an upward shift in the short-run Phillips curve. Then, merely adding an equilibrium assumption gave you the long-run Phillips curve and an open road to the natural rate of unemployment and to the triumph of monetarism.

In both instances, Friedman's appeal was to tenets of economic rationality and equilibrium that his supposedly Keynesian opponents—the neo-Keynesians—had already embraced. A few close followers of Keynes, including Nicholas Kaldor, Joan Robinson, and Robert Eisner, had spotted the trap and evaded it—and they had suffered a fair amount of professional ostracism as a result. My father had by that time moved on to other issues and was not vulnerable. But the neo-Keynesian mainstream, the MIT crowd, had nowhere to hide. They were caught out and outsmarted; Friedman had them cold.

The second Milton Friedman is the one seen in his columns, on the lecture podium and on *Free to Choose*, the television series and the book. The spirit of that Friedman is of the dime-store free-marketeer, with the occasional *frisson* of unconventional thought, such as his position on drugs, and the occasional crowd-pleaser, such as his case against the draft.

In this role, Friedman's appeal came from simple arguments and from the optimism with which he made them. He recast the dour and unpalatable pose of the old Austrians, from von Mises to von Hayek, who shared (despite numerous differences) an autocratic and unforgiving demeanor, and a distinct coldness toward the losers under capitalism. Friedman lived and wrote in an optimistic age for optimistic readers. He urged that in a free-market economy, most people would win out in the end anyway. So why not *be* optimistic? In Friedman's vision, the losers largely disappeared.

In *Free to Choose* historical and institutional subtlety weren't merely lost. They were crushed. The entire program consists of one categorical assertion after another, with barely perceptible caveats—"if you compare like with like..."—thrown in. As I pointed out in my half-hour on camera with Friedman in 1990, his first program, mainly filmed in Hong Kong—that bastion of liberty, then still a British colony—drew no distinction between the government of the United States

and that of the People's Republic of China. He couldn't deny this; it was evident to anyone who'd watched the show.

We may surmise that Friedman's affinity for first principles were behind his support for the Chilean dictator Augusto Pinochet, a man who granted freedom—and life itself—only to those who dared not oppose him. Here was a grisly contradiction between “economic freedom” and the real thing. My impression is that Friedman did his best to ignore Pinochet's crimes, and then made up excuses when he had to. This is perhaps harsh. But it's a more generous view than the alternative, which is to believe that he thought the socialists, communists, poets and musicians in the National Stadium got what they had coming.

No economist from today's Chicago School will ever replicate the great achievement of the first Milton Friedman, as I have described him. Today's Chicagoans have no dominant tradition to oppose. They *are* the dominant tradition. Thanks to Friedman, they have been so for over thirty years. And as such, they've fallen prey to the same philosophical rigidity, to the same indifference to fact, and in many ways to the same complacency and arrogance that mainstream neo-Keynesians displayed a generation back. The Great Financial Crisis provides sufficient evidence of this; the work of Philip Mirowski (forthcoming) will soon demonstrate it in detail. If the Chicagoans were vulnerable—as the neo-Keynesians were—they too would lose out in the next generation of academic jobs. They won't; in these matters the economics profession has moved beyond shame.

The second Milton Friedman, on the other hand, has many epigones. It's not hard work. The cable networks are full of sloppy sloganeers, with their jeremiads against the state, except when the heavy hand is directed at protesters against free markets. Friedman substantially created this media niche and his successors often know their debt. The same is true of his followers in government, including Ben Bernanke at the Federal Reserve, who paid tribute directly to Friedman on his 90th birthday. Bernanke has applied Friedman's anti-depression medicine—printing money—with real tenacity and sadly little effect.

Occasionally reality shakes the faith of economists in their doctrines. To be fair, that was true of Friedman himself on some things; late in life he even repudiated the money growth rule, for which in some quarters he was most famous. We shall see whether Ben Bernanke, in due course, expresses similar reservations about quantitative easing.

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The Uniqueness of Milton Friedman

J. Daniel Hammond¹

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That there is no Milton Friedman today is not a mystery; the mystery is how Milton Friedman could have been. Looking back over the course of our lives, the most significant events, such as meeting and marrying our mates, the birth of our children, the particulars of our careers, seem to have defied the odds of their happening. At the beginning of our lives chances of these particular events occurring were minuscule. Yet they became facts of our biographies. So it is with Milton Friedman's life, though perhaps more so than for the rest of us.

The facts of Milton Friedman's life make him unique among twentieth-century public figures. Consider the odds that his life would take the trajectory it took. A young man receives the best education to be had in mathematics and statistical theory, yet turns away from his fellow economists' enthusiastic embrace of mathematics and econometrics, becoming the leading critic of the econometric modeling program at the Cowles Commission.

Friedman was a follower of Alfred Marshall in the methodological sense of regarding the purpose of economics as solving "concrete problems" (Friedman 1955, 904). Thus he was critical of formal theorizing absent regard for real-world institutions and market data, or more generally, "observed and related facts" (Friedman 1946, 617). For example, he wrote of Oskar Lange's *Price Flexibility and Employment*, which he regarded as high-quality analysis of its type, "The theory provides formal models of imaginary worlds, not generalizations about the real world" (Friedman 1946, 618). His criticism of Abba Lerner's *The Economics of Control* was much the same. He praised Lerner for his skill as a theorist and for his interest

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in social welfare, but concluded that these were employed “in a vacuum and have not been combined with a realistic appraisal of the administrative problems of economic institutions or of their social and political implications” (Friedman 1947, 416).

In college at Rutgers from 1928 to 1932 Friedman’s interest in economics lagged behind his interest in mathematics and statistics. He entered Rutgers planning to major in math, but changed plans and majored in economics. Yet he took almost as many math courses as economics courses. In fact he was awarded the Joseph P. Bradley Memorial Prize in Mathematics. Friedman’s first professional goal was to put his mathematics to work as an actuary. During his senior year arose the prospect of pursuing a Ph.D. His choice in 1932 between studying applied mathematics at Brown University or economics at the University of Chicago came down to a matter of money. In the depths of the Great Depression, Friedman opted for economics at Chicago because the fellowship offer from Chicago was better than that from Brown. Even so, the choice of economics at Chicago rather than math at Brown was not entirely what it would appear to be, for in his first year at Chicago Friedman took more courses in math and analytical statistics than in economics.

When he joined the economics faculty at Chicago in 1946, Friedman’s work experience to date was as much or more with statistical analysis as with economics. His experience included work on sampling techniques at the National Resources Committee and war-related projects with the Statistical Research Group at Columbia. Yet Friedman was skeptical of the value of much of the work that was being done by econometricians in the 1940s and 1950s. He once ran a horse race between a state-of-the-art econometric model of the U.S. economy and a naive model that predicted no change in the values of the endogenous variables. The naive model won the race (Friedman 1951).

As a graduate student at Columbia (1933-34) and staff member at the National Bureau of Economic Research (1937-46) Friedman was trained and mentored by Wesley C. Mitchell, one of the leading American Progressives. The odds were small that the pupil would follow his mentor’s career path, yet become the leading critic of the programs and presumptions that were central to the Progressive cause. Like Mitchell, Friedman became an economist at the National Bureau of Economic Research, and a business cycle analyst. Friedman’s first job out of graduate school was with a New Deal agency, the National Resources Committee. Despite this background, he became an opponent of much of the Progressive and New Deal legacy, concluding that on balance the Federal Reserve was a destabilizing force in the American economy and that commercial regulations and social welfare programs did not serve the ends they were supposedly designed for.

Next consider the odds that a thirty-five-year-old man who has been occupied with the practical problem of making a living and intellectual matters of technical statistics and business cycles, having little interest in politics or political philosophy, would become the great persuader for the cause of classical liberalism. This was the course of Friedman's life. His first foray into public policy debates on the side of free markets was a critique of rent controls, written with George Stigler when they shared an office at the University of Minnesota in 1945-46. A year later, Friedman met Friedrich Hayek at Mont Pèlerin, and experienced his first immersion in classical liberal ideas. By the early 1950s Friedman had become the leading American spokesman for free markets (see Burgin 2012, ch. 5).

There are other improbable dimensions to the life of Milton Friedman. At a time when the scientific prestige of economists was nearing its apex, as the winner of the American Economic Association's John Bates Clark medal for 1951, and as someone who believed firmly that economics is a science, Friedman declined the invitation from his teacher and surrogate father, Arthur F. Burns, to join President Eisenhower's Council of Economic Advisers.

Or consider how likely it is that a middle-age man who is brilliant, who is extremely busy, and by now is famous, would respond with personal letters to readers who write to him about his columns in a newsweekly. Yet, this is what Milton Friedman did while writing for *Newsweek* (1966-84). There are hundreds of letters to ordinary folk in the Milton Friedman Papers at the Hoover Institution.

And finally, consider a man who, because of his gifts of persuasion and his advocacy of causes not in favor with the academic and intellectual establishment, is treated as a public enemy. This man, though reviled by many and mocked even by friends, refused to attribute any motivation other than good will to his critics. He repaid calumny with smiles and simply renewed his efforts to persuade. This was Milton Friedman's way (see on this Hammond 2011).

Milton Friedman should not have been. But he was! He was a true original. Will there be others? No doubt there will be or are economists who are similar in having lives with improbable twists and turns, lives filled with ironies. But I believe there is a reason that we do not have someone substantially like Milton Friedman today. The reason is the movement of history over the past six decades that has left economics and economic policymaking markedly different from the late 1940s and 1950s, when Friedman emerged on stage.

John Maynard Keynes's career marked the beginning, and Friedman's the end, of a brief historical period in which economists, their patrons in government, and the educated public believed that economists were producing knowledge by means of which the business cycle, poverty, and 'market failures' such as monopoly and externalities could and would be brought under social control. Keynes (1923, 80) famously wrote that "in the long run we are all dead." Keynes and Friedman

are both dead now, but the rest of us are living through a long run in which such optimism for economic science and the use of science in our political system looks quite naive. In the extended tail of the Great Recession, the Federal Reserve promises to keep interest rates near zero for as far as the eye can see. They have added most everything to their portfolio but the city of Detroit. Still, there is no ‘normal’ recovery. And the suggestion that Congress will make scientific use of the Federal budget for countercyclical policy belongs in a late-night television monologue.

The conduct of economic debate has changed, too. Reading Friedman’s *Newsweek* columns now, one is struck by their gracious and non-partisan tone, which is often lacking in contemporary public discourse.

So no, there is not another Milton Friedman out there today, and there is not likely to be another any time soon.

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Why Milton Friedman Was Rare

David R. Henderson¹

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I don't know whether there will ever be another Milton Friedman, but I do know what made Friedman so rare: a remarkable combination of characteristics. While a similar combination *could* exist in some future person, the probability is very low.

Friedman's scholarly qualities

Milton Friedman was a brilliant man and a first-rate economic researcher. He showed signs of this as early as age 20 in his master's thesis, "An Empirical Study of the Relationship Between Railroad Stock Prices and Railroad Earnings" (1933). The thesis anticipated the efficient-markets literature that got going in a big way three decades later. His two most important intellectual contributions were *A Theory of the Consumption Function* (Friedman 1957) and *A Monetary History of the United States, 1867-1960* (Friedman and Schwartz 1963). The former book presented one of the first major challenges to the Keynesian consumption function, according to which current consumption is based on current income. If, instead, current consumption is based on what Friedman called "permanent income," then Keynesian fiscal policy via the multiplier will be less potent than otherwise. In the latter book, Friedman and his co-author Anna Schwartz showed that the failure of the Federal Reserve to act effectively as lender of last resort during the banking crises of the early 1930s contributed mightily to the Great Depression. That finding, too, was a body blow to the 1960s' Keynesian consensus, which viewed central

1. Naval Postgraduate School, Monterey, CA 93943. Milton Friedman was not the only economist with a wife who improved his writing; my wife, Rena Henderson, helped with this piece. Thanks also to Robert Murphy for his helpful comments.

banks during the Depression as impotently pushing on a string in a liquidity trap. That latter book, plus some of his later work, helped persuade the economics profession that, as Friedman (1970, 24) put it, “inflation is always and everywhere a monetary phenomenon.”

If those and a few dozen academic articles, chiefly on monetary policy, had been Friedman’s only contributions, he would still have had a successful career at the University of Chicago and would still likely have won the Nobel Prize. But we would not be having this symposium.

Who *is* this guy?

What were Friedman’s other characteristics that mattered? I’ll explain by telling my own tale of discovering Milton Friedman.

In the summer of 1968, when I was 17 and had just finished reading almost all of Ayn Rand’s works, fiction and non-fiction, I happened to pick up an issue of *Newsweek*. In a column titled “The Public Be Damned,” accompanied by a photo of a smiling, bald-headed economist, Friedman argued that the attitude expressed in that title, far from being businessmen’s attitude toward the public, is actually the attitude of the U.S. Post Office. I loved the column and started working through the old *Newsweeks* in the University of Winnipeg library, finding quickly that Friedman wrote in the magazine every three weeks. I liked virtually every piece of his I read, although, truth be told, I wondered why he kept giving advice to the Federal Reserve Board on how to centrally plan the money supply, rather than advocating getting rid of the Fed.

In any case, I was hooked. “Who *is* this guy?” I wondered. I was stunned that someone with his views could be on the faculty of a major American university. So I immediately got something out of Friedman that I hadn’t gotten from reading people like Henry Hazlitt: the idea that one could have those views and still make it in academia.

I then took *Capitalism and Freedom* out of the library and found myself wanting to photocopy so many pages—at the then-high 1968 price of 10 cents per page—that I plunked down \$5.50 and ordered my own copy. I was not disappointed. The most shocking lines were in the book’s first paragraph:

In a much quoted passage in his inaugural address, President Kennedy said, “Ask not what your country can do for you—ask what you can do for your country.” It is a striking sign of our times that the controversy about this passage centered on its origin and not its content. Neither half of the statement expresses a relation between the citizen and his

government that is worthy of the ideals of free men in a free society. The paternalistic “what your country can do for you” implies that the government is the patron, the citizen the ward, a view that is at odds with the free man’s belief in his own responsibility for his own destiny. The organismic, “what you can do for your country” implies that the government is the master or the deity, the citizen, the servant or the votary. To the free man, the country is the collection of individuals who compose it, not something over and above them. He is proud of a common heritage and loyal to common traditions. But he regards government as a means, an instrumentality, neither a grantor of favors and gifts, nor a master or god to be blindly worshipped and served. He recognizes no national goal except as it is the consensus of the goals that the citizens severally serve. He recognizes no national purpose except as it is the consensus of the purposes for which the citizens severally strive. (Friedman 1962, 1-2)

Wow! I mean, wow! Here was a man, I thought, willing to take unpopular stands and go against the dominant views of the times. Remember that this was published in 1962, at the height of the “Camelot” era, when Kennedy and his pretty wife Jackie were idolized. Again, it was hard to believe that a person like Friedman could exist at a major university.

Milton Friedman not only existed—and thrived—at the University of Chicago, but also had a huge impact in the bigger world beyond academe. Why did he do so well and have so much influence in that bigger world? I came to see that it had to do with his rare combination of abilities and personal characteristics. I’ve mentioned his brilliance. The three other characteristics that I think mattered most were: his integrity, his passion, and his warmth. I don’t know if they should be in that order—they were all important, and related.

Integrity, passion, and warmth

Consider, first, his integrity. I don’t mean integrity in just the narrow academic sense, that is, being the kind of person who would not, for example, make up data. I mean it in a much broader way: the way he treated and talked about pretty much everyone. I remember sitting with him and his wife, Rose Friedman, at the opening dinner of the Austrian Economics Conference in South Royalton, Vermont in June 1974. I knew that their summer home in Vermont was near that of John Kenneth Galbraith, one of Milton’s main rivals for the public’s ear on economics and a Keynesian who, around that time, had actually described himself

as a socialist. I asked Milton and Rose a question that suggested—I can't remember my words—what a loon Galbraith was. Milton and Rose would have none of it. They didn't put me down; they realized they were dealing with a 23-year-old who didn't know a lot about the world. Instead, they just said that Galbraith was a fine person and that their differences with him were not over values, but over how they thought the world worked.

That conversation left a strong impression on me. I changed, almost on the spot. I wanted to be like Milton. I loved that generosity of character and tried to emulate it.

Notice that I started discussing his integrity and ended up talking about his (and Rose's) generosity of character. I think those two were tightly linked. But while I'm on the issue of integrity, I want to point out one other aspect of Milton's integrity: his willingness to take unpopular stands because he believed that they were right. Whether supporting Barry Goldwater for president in 1964, when the overwhelming majority of intellectuals were against Goldwater, or coming out against the military draft in the mid-1960s, when the draft still had strong bipartisan support, Friedman was his own man.

Milton Friedman was also a passionate man. His passion showed in his teaching, his writing, and his speaking. A friend of mine, Christopher Jehn, who did his doctoral work in economics at the University of Chicago in the late 1960s, put it well. He told me that in Friedman's class, he always knew that Friedman cared deeply about making sure that every student understood. This was true about Friedman's writing also, although his writing would probably not have been as good if he hadn't had a first-rate editor, Rose Friedman, checking his work. To get a feel for Milton's passion in speaking, you can't do better than to watch the segment of the television talk show *Donahue* in which he challenges Phil Donahue's implicit view that only free-market economies run on greed ([link](#)). His passion, without anger, and his warm outreach to the audience are something to behold.

That brings me to his warmth. Friedman had what I regard as the two main characteristics that lead to warmth: he was totally comfortable in his own skin, and he genuinely liked people. At age 19, a few weeks after graduating from the University of Winnipeg, I flew down to Chicago and went to his office at the University of Chicago. Friedman invited me in warmly and took about ten minutes of his time to convey two main messages to me. The first was that there's more to intellectual life and development than Ayn Rand. The second—and these were his exact words—was, "Make politics an avocation rather than a vocation." Then he gently escorted me to the door. But he gave a 19-year-old kid ten minutes.

To say that Friedman was warm, though, is not to say that he couldn't get angry. He got very angry at me an hour or two before that 1974 South Royalton dinner, when we had a disagreement about how quickly to end Social Security. He

advocated phasing it out over decades; I advocated ending it immediately because it was unjust to workers. But anger can be clean, and his generally was. Indeed, one of the cleanest bits of anger I've ever seen was Friedman's look when, at the moment he was to receive the Nobel Prize, someone disrupted the proceedings ([link to video](#)). Friedman had a beautiful, intense look of anger on his face, kind of like the look that Kiefer Sutherland's character has after Tom Cruise's character mauls him on the stand in "A Few Good Men." But the anger lasted a few seconds and then he recovered, and, unlike Sutherland's character, laughed at the situation and enjoyed the ceremony.

I shouldn't let the story end, though, without mentioning another aspect of Friedman's passion: his willingness to take action. It was not just that he wrote about why the U.S. government should end the draft. It was also that he spoke on it in many forums, testified on it, and personally lobbied Congressmen on it. He once wrote that the draft "is the only issue on which I have engaged in any personal lobbying with members of the House and Senate (as contrasted with testifying before relevant congressional committees on subjects in my field of competence)" (Friedman 1972, 118).

Is another Milton Friedman possible? Yes. But if you want me to bet that there will be another one soon, you'll have to give me good odds.

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Why There Is No New Milton Friedman

Daniel Houser¹

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Milton Friedman was focal in the movement that emphasizes the importance of liberty, free markets, and entrepreneurial spirit in creating broad-based prosperity and peace. No one today commands the attention he did. It is interesting to speculate why. Part of the answer, surely, is that the time and place were right for Friedman to employ his exceptional abilities. Time, place and person combined to create the focal Milton Friedman, and he is one-of-a-kind.

It is unlikely that another single person will fill the role he played. The landscape of economic and political conversation now takes the form of a cloud of online real-time exchange in ideas. The democracy of ideas is powerfully attractive at a human level. All have voice and opportunity to enter the academic and broader public conversation. One result, arguably, is more progress on fundamental ideas as well as more efficient discovery of new paths forward.

For a single person to be focal seems to require, in part, that the person command sustained attention—like the speaker at a plenary session of a conference. But we now spread ourselves between innumerable concurrent sessions, as it were, and we sample those like an invisible man who passes undetected in and out of the parlor. And then there is our own impulse to refute, extend, and clarify—in real time—points just made. The result is evolving webs of discourse, absent hierarchies. Insights emerge from a process distinguished by an absence of formalized leadership.

Milton Friedman was extraordinary. Were the 50-year-old Friedman today to begin to participate in today's conversation, yes, we would read, listen, and react.

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But then we would move on to the next person who wrote something that day. A young Friedman would be an important participant in today's conversation, but I suspect his level of leadership in the conversation would not—could not—parallel what he achieved during the actual period of his career.

A focal figure serves as a common reference point. Perhaps there is no new Milton Friedman because classical liberals already have a serviceable amalgam of reference points, from many personages—Adam Smith, Friedrich Hayek, James Buchanan, and others. Classical liberals perceive themselves as contributors, in smaller or larger ways, to such a viewpoint. Maybe the new structure of things—or lack of structure—does not lend itself to the emergence of a new figure of central importance. But the new situation invites many to contribute to understanding the central issue of whether people should be free to choose.

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On Why There Is No Milton Friedman Today: *Sui Generis, Sui Temporis*

Steven G. Medema¹

[LINK TO ABSTRACT](#)

The question of why there is no Milton Friedman today admits to no simple answer—unless it be that the answer is that there *is* a Milton Friedman today, and his name is Paul Krugman. But this would be selling Friedman short, for even Krugman, with all of his accomplishments on so many fronts, does not hold a sway comparable to Friedman's. And, of course, the organizers of this symposium are, shall we say, less than interested in a Milton Friedman that quacks like a Paul Krugman.

Milton Friedman was one of the towering figures of the twentieth century. Agree with his economics, policy positions and worldview, or not—and this essay takes no position on that issue, emphasizing issues of uniqueness rather than correctness²—his influence on the field of economics, on economic and social policy, and on popular attitudes toward a wide range of topical issues is perhaps rivaled only by John Maynard Keynes in that century. And the reason why there is no Milton Friedman today is, at a surface level, very obvious: he was *sui generis*. But to leave it at that begs the question of what it was that made Friedman unique. The answer, I assert, lies at the intersection of Friedman's professional and personal characteristics, and the context in which he lived and worked.

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2. My own view is that Friedman was right on some things and wrong on others, but that is not my concern in the present essay.

Sui Generis: Being Milton Friedman

Friedman was anything but a one-trick pony, and it is clear in retrospect that the whole was greater than the sum of the individual parts. Nonetheless, each of the particular component parts played a significant role in making the whole possible.

Friedman as price theorist

Friedman's price theory side tends to take second (at best) place in the typical discussions of his academic career, but it was both the foundation of much of his economics and the launching point for his analysis of the market system and of vast realms of social policy. His price theory was not of the recent sort, where the model of the ruthlessly rational agent is invoked to explain all manner of activities within and far beyond the marketplace. Rather, it was grounded in demand and supply analysis as a simple, concrete engine for revealing what he perceived as fundamental truths about the operation of markets and, in particular, the effects of policies that impact their functioning.

If one examines the notes and problems from Friedman's graduate price theory course at Chicago, a course that he taught from 1946 to 1962 and again in the mid-1970s, one is struck by the simplicity of the analysis, but also by its power (see Friedman 1962b and 1976). The conclusions that flow from the analysis are eminently sensible, and it is easy to move from these logical theoretical results to a particular set of policy conclusions, Lionel Robbins' (1932) dictum notwithstanding. Friedman did not hesitate to make such moves and, more generally, evidenced far more concern about applying price theory than about underlying formalisms that were—and continue to be—of so much concern to others in the field.

Friedman as monetarist

Friedman's monetarism is the aspect of his economics that is best known in academic circles, and, perhaps more than any other aspect of his professional work, reveals his absolute passion for and commitment to ideas in which he truly believed. Monetarism was the driver behind Friedman's interpretation of, e.g., the Great Depression, his views of Keynes and Keynesian economics (which are not the same thing and also had nothing to do with getting us out of the Depression), and his negative attitude toward the New Classical economics. His macroeconomics—grounded in the basic elegance of the quantity theory of money and,

on the normative front, constant money-supply growth—was as simple as his price theory, and both are illustrative of the power of simplicity. Monetarism, in Friedman’s hands, yielded explanations and prescriptions easily articulated to and understood by policymaker and citizen alike—in sharp contrast to, say, the fiscal multiplier concept and its implication that taking a dollar from a private citizen and giving it to the government to spend creates more dollars.

Friedman as empirical economist

Friedman’s extensive training in statistics and his attendant empirical bent, evidenced in venues ranging from *Income from Independent Professional Practice* (Friedman and Kuznets 1954) and *A Monetary History of the United States* (Friedman and Schwartz 1963) to the examination problems in his price theory course that were ripped from the headlines of the day, allowed him to combine theory and empirical work in a particularly rich way. Agree with him or not—and there were certainly those in the profession who questioned his empirical work—he was able to powerfully marry theory and empirical analysis to produce results and messages that challenged certain received views.

Friedman as methodologist

Milton Friedman penned what is unquestionably one of the two most important pieces of writing in economic methodology during the twentieth century.³ His “The Methodology of Positive Economics” (1953), served to define the profession’s view of what it is that economists do—that the ultimate test of economic theory is its predictive power and not the realism of its underlying assumptions. Whether economists actually honor the predictive side of this prescription is another story, and one can even reasonably question whether, at times, Friedman’s own work lived up to his methodological stance.

But Friedman’s stance, combined with the simple theoretical constructs that he employed and the empirical analysis that he marshaled to buttress his theoretical views, made for a nice, tidy package. And the profession’s adoption of his methodological credo played into his hands, making his theory and empirical work something with which the profession had to contend. Having granted his methodological position, the onus was on those segments of the profession that disagreed with him to show how and why his analysis was misguided.

3. The other, and I would argue most influential, was Robbins’ *Essay on the Nature and Significance of Economic Science* (1932).

Friedman as policy promulgator and advisor

Unlike his close friend, George Stigler, Friedman did not shy from active involvement in the policy realm, a reflection both of his personality and temperament and of his passion for the ideas he held dear. Friedman wanted to be, as Keynes (1936, 383) put it, that “defunct economist” to whom policymakers were slave—not for personal aggrandizement, but because of the ideas and ideals in which he believed. Friedman was Stigler’s “economist as preacher” and did not hold to Stigler’s oft-dismissive views of those who made policy (Stigler 1982). Friedman was called to preach the gospel, and preach he did—in writings, speeches, formal advisory work, and responses to requests for policy advice from officials and candidates from around the world (certain of which instigated no small amount of controversy).

Friedman as public intellectual

Friedman also differed from his friend Stigler in his views of the general public. Where Stigler saw people thinking, acting, and voting their self-interest, Friedman believed that a message about the larger social good could get through and that narrow or unenlightened perceptions of self-interest could be reformed in ways that caused people to see that things which might at first glance appear to be contrary to their interests were, through the larger benefits they provided, in their self-interest after all.⁴ Here, too, he harnessed—in vocabulary intelligible to the average lay person—the power of his price-theoretic system, often combined with his belief in the virtues of freedom and individual liberty, in outlets ranging from books (*Capitalism and Freedom*) to magazines (the *Newsweek* columns that ran for 18 years) to television (*Free to Choose*).⁵

Friedman is far from the only economist public intellectual of his time, of course, but it is arguable that his impact far outstripped his competitors. His academic *bona fides* provided credibility, of course, but his populism helped to set him apart from academics such as John Kenneth Galbraith, whose public writings had a pervasive ivory-tower aura—an aura that all-too-often comes off as suggesting that the writer knows what is best for the reader. Of course, the writings of public intellectuals treading in realms of policy are preaching almost by definition, given their inevitable normative thrust. But Friedman’s particular style made it seem that

4. In this, at least, Friedman and opponents such as John Kenneth Galbraith were very much on the same page, even if they were often diametrically opposed in their views of the policy direction implied.

5. The economist’s role as public intellectual is an under-appreciated and under-analyzed aspect of the history of economics. For a discussion of this issue, see Mata and Medema (2013).

he was in conversation with the reader rather than talking at or down to him, and so drew in the reader in a way that the style of others often did not.

Friedman as communicator

Woven through all of this, of course, were Friedman's legendary skills in oratory and debate, the force of his personality, and his elegant writing style—essential elements of success for teacher and preacher. These skills, combined with the simplicity of his theoretical system, make it easy for the critic to label his many successes a triumph of form over substance. But that undersells the power of simple models and the inherent complexity of social science. The abstract formalism of Paul Samuelson's *Foundations* and of Kenneth Arrow and Gerard Debreu's general equilibrium system is attractive to those who feel that the complexity of economic reality necessitates the use of complicated models such as these. The heterodox who preach the necessity of greater realism argue that simplicity masks much that is important in economic reality. And each of these positions has a measure of truth associated with them. But Friedman's simple models had something the others lacked—ease of operationalization and communication. This, of course, does not make Friedman's approach superior to others. But it does help us to explain the success that leads to the question under consideration in this symposium.

***Sui Temporis*: Becoming 'Milton Friedman'**

Milton and Rose Friedman titled their biography *Two Lucky People*. Luck, of course, is often a matter of circumstance, and the circumstances in which the Friedmans lived and worked played no small part in Milton's success. Simply put, he had, if you will pardon the pun, the 'right' set of ideas at the right time. Growth in the scope of state action and the confidence in planning that followed the planning successes of the Second World War were very much the order of the day. The spectre of socialism or extensive state planning served as a foil for *Capitalism and Freedom* (1962a), and a U.S. regulatory state offered the backdrop for *Free to Choose* (1980). At a time when a Galbraith was attempting to move us farther down these roads,⁶ there was a clear path for Friedman to capture the imaginations not just of those on the right, but of a swath of those in the middle.

6. Galbraith, of course, was highly influential in his own right, though his impact on the economics profession was not nearly so great as that of Friedman.

In like manner, one must wonder how much attention would have been paid to Friedman's monetarist ideas, either on the academic front or in the policy realm, if not for their timing. Would his work on this front in the 1960s have been nearly so influential had it been developed fifteen years earlier, when Keynesian approaches were in their ascendancy, rather than immediately prior to the collapse of Keynesianism that resulted from the events of the 1970s? Or, if these ideas and other aspects of his worldview had not resonated philosophically with the mood that led to the elections of Ronald Reagan and Margaret Thatcher?

The foregoing suggests that there *will* be another Friedman—that all that is necessary is for someone to be that right *will* person at the right time. But context also matters here in at least one other very important way, one that suggests pessimism for the possibility of future Friedmans. When Milton Friedman came of age, the economics profession was in many ways very different than it is today. Generalists are, by and large, a thing of the past in a profession characterized by increasingly narrow specialization. The idea of entering economics because one wishes to make the world a better place—a defining feature of the profession for so long and a necessary ingredient in becoming a Milton Friedman or a John Maynard Keynes—has given way to the idea of entering economics because it is a useful profession for doing applied mathematics and statistics—and now, thanks to Steve Levitt, it's sexy as well.⁷ Writing for the general public and attempting to persuade policy makers is no longer seen as serious work in an age where impact factors and journal rankings are the coin of the realm and the average NBER Working Paper receives far more professional attention than a book published by a major university press. In short, it would be incredibly difficult for a young economist today, even a Milton Friedman clone (or left-wing version thereof), to take the path that Friedman chose at any university with the stature needed to earn an audience on the public stage.

Conclusion

Those waiting for the next Milton Friedman are liable to be every bit as disappointed as the Democrats waiting for the next Jack Kennedy and the Republicans waiting for the next Reagan. Each of these groups will, I expect, be waiting for a very long time. Modern historians have tried to move us away from the 'great man' view of history (where by "great" I mean incredibly influential), but there is a reason why such histories have had staying power. Ideas have power, but it is only

7. Ironically, Friedman's 'ripped from the headlines' examination questions of the 1950s, which dealt with subjects such as the allocation of taxi cab medallions in New York City, were considered every bit as 'freakish' then as *Freakonomics* (Levitt and Dubner 2005) is today.

in the hands of the right personality and the proper context that those ideas become transformative. Milton Friedman was one such personality, and he was fortunate enough to live and work during a period in social and professional history in which he could become what he came to be.

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Why Is There No Milton Friedman Today?

Sam Peltzman¹

[LINK TO ABSTRACT](#)

The question has a broad public aspect and a narrower professional one. Friedman was simultaneously an accomplished economist and an influential public intellectual. This fact provides part of an answer to the question in the title, because the combination of talents is quite rare. That they emerged simultaneously—*Capitalism and Freedom* and *A Monetary History of the United States* appeared within a year of each other—is singular. Friedman's chief contemporary rival as an economist's voice in the public square was probably John Kenneth Galbraith, but he could claim nothing like Friedman's professional stature. Paul Samuelson shared with Friedman the public forum of a regular *Newsweek* column, and Samuelson might have emerged as Friedman's multi-talented rival. But that did not happen.

The opportunities for economists today to communicate with a wider audience are greater than in the 1960s and '70s, when Friedman made his first impact on public discourse. There are more media outlets and more time and space to fill and greater potential rewards than in Friedman's time. Accordingly, there are probably more economists today seeking a non-professional audience than in Friedman's time. However, the process is sequential rather than simultaneous. My sense of the typical pattern is that an economist achieves some professional distinction first and only thereafter enters the public arena.

Why was Friedman able to bring off a successful entry onto the public stage while at the height of his academic career? I can only speculate, but I see two reasons. First, it helped that Friedman didn't answer big questions much differently for the two audiences. In this regard, I recall my first encounter with him vividly.

1. University of Chicago, Chicago, IL 60637.

Along with perhaps 50 other wet-behind-the-ears new Chicago graduate students I trooped into my first class—Milton Friedman’s Price Theory course. All of us eagerly awaited our inauguration into the deeper technical mysteries of our chosen profession. Instead we got an extended paraphrase of an essay entitled “I, Pencil,” in which a humble pencil tells us of the herculean coordination problem required to get itself produced and distributed and of the virtues of markets in solving that problem.² The technical level of the course did not subsequently rise very far above this. Friedman believed that the best economics consisted of applying some uncomplicated and basic insights to important real-world problems. This is also about what you get in *Capitalism and Freedom*. Think of the contrast with Samuelson, whose accomplishment after all includes a monumental systematization of economics that widens rather than narrows the distance between professional and lay discourse. For Samuelson a discussion of the issues of the day would require setting aside much of his best economics. He was able to do some of this for college freshmen in a best-selling textbook, but never really was able to move on to a larger audience. For Friedman this move came much more easily.

The second ingredient in Friedman’s success was his skill as a polemicist, in the best sense of that word. Friedman loved the cut and thrust of public as well as professional argument. He emerged in both arenas as an outsider. He only achieved broader professional respect and iconic public status after the Keynesian consensus collapsed in the 1970s. Until then he could take on the world virtually alone, and his debating skills made him the perfect counterpoint to the conventional wisdom of the day, which was interventionist to one degree or another. All-against-one makes for a good show, and Friedman liked the odds. Again there is a contrast with Samuelson, who was also naturally combative. He would have been (and was) a voice of the conventional wisdom in any public discussion of that era. But arguing stridently for the status quo lacks a certain appeal. The public stage would be less fun for a Samuelson than for an outsider like Friedman.

In short, one answer to the question in the title is that Friedman was an individual of rare talent who came on the scene in a unique period. But there is a contemporary subtext: why are we unlikely to see anyone remotely like him emerge from our profession anytime soon? That is, someone who reaches out successfully during, not after, his or her professional ascent. For the sake of argument, I accept the premise of the question. I think the preceding history contains the seeds of the answer I would give. It suggests that a Friedman-like individual would have the following attributes:

2. The original essay was by Leonard Read (1958) of the Foundation for Economic Education and appeared in the Foundation’s publication *The Freeman*. Friedman did not, as best I can recall, acknowledge this parentage. Instead he just played the story before us in the form of a Socratic dialogue.

WHY IS THERE NO MILTON FRIEDMAN TODAY?

- A contrarian belief in the efficacy of markets and skepticism about the actual, as opposed to the promised, effects of government intervention
- A deep commitment to those beliefs that energizes a need to communicate them widely
- A thick skin, indeed a joy in combat
- An active professional engagement with the kind of real-world problems where conventional belief is skeptical about market solutions
- A mode of engagement that produces results easily communicated to a wide audience

To list these attributes is not quite the same as stating an impossibility theorem, but it gives a sense of what we are up against. To begin with the first of these, the range of belief within economics has narrowed, partly because of Friedman's efforts. The modal economist today is probably more interventionist than the editors of *EJW* but less than the modal economist of Friedman's era. Market solutions that would have been dismissed as lunacy then are given a respectable hearing or are part of the consensus today (think flexible exchange rates or unregulated railroad rates). There is just less room today for a good fight among economists. And this reflects a narrower range of controversy outside economics as well. The alternative of the planned economy—a seriously considered one in Friedman's day—is gone. The notion that Big Problems require government solutions is no longer reflexive and often encounters active resistance. Consider in this regard what has happened in the aftermath of the financial crisis of 2008. The chattering class pronounced with excited joy that Capitalism is now Dead, but the political center hardly moved, and in some countries even moved right—to fiscal rectitude, labor market reform, etc. Hardly any left party that moved away from socialism in Friedman's heyday has moved back since. What is a committed free-market economist spoiling for a good fight to do when the other side is not so far away?

But even if there were a significant socialist faction to fight with, where are the committed free marketeers today? There are to be sure numbers of them within our gates, perhaps more than in Friedman's time. But they lack something that Friedman had in the Chicago department of his time, a critical mass at a top-five department. Instead what you have among our best and brightest, including at Chicago of today, is a much cooler customer. This one tends to be less committed to any politico-economic system. The animating spirit is more the engineer solving specific problems than the philosopher seeking a unified world view. The questions asked tend to be smaller than, say, the connection between capitalism and freedom. On the other hand, the skill with which the question is answered tends to be greater than in times past. At some point today's leading economists may want

to communicate their results to a wider audience. But this is an afterthought, in the sense that what is valued within the profession—the skill in obtaining the result—is not what the outside audience is interested in. I have discussed the pluses and minuses of this generational shift in economists’ practice elsewhere (Peltzman 2011). Suffice it to say here that it is not the kind of shift conducive to producing another Milton Friedman anytime soon.

It is worth noting that the shift to more specialized and ingrown modes of analysis is hardly unique to economics. It appeared in other fields even earlier than in economics. Consider, as just one example, classical music. I claim no great expertise here. However, even if your interest is only casual you might notice how the last 50 or so years have differed from previous eras. Try to list composers working mainly in the recent era who made a significant contribution to the musical canon. Any reasonable list will have fewer names—perhaps none—compared to a similar list for the preceding 50 or so years. If this were *Music Journal Watch* we could easily imagine a symposium on “Why Is There No Stravinsky or Bartok or Prokofiev Today?,” to mention just three names from the previous era already recognized as significant contributors by 1950. As with economics, part of the answer may lie with more academic and self-referential modes of work among the best contemporary composers.

It is hard for me to see a reversal of the kind of trends I have described, whether in economics or music or a host of other fields where the engineer has replaced the philosopher. Perhaps an economic calamity will shake things up in economics. But we had one in 2008, and very little changed within the profession. There was a period of befuddlement, including some self-examination of how useful the skills we had come to value really were. In the end, though, economists went back to their tinkering and were largely irrelevant to the political response to the crisis. You might think some fundamental questioning of our monetary arrangements might have emerged as part of the discussion among our leading macroeconomists. This did not happen. Maybe the next crisis will produce a Friedman-like figure who questions whether we should have government meddling with money at all.

There is an ironic aspect to my answer to the symposium’s question. It is that Friedman’s success makes it harder for someone to follow in his footsteps. There is no serious socialist faction left within economics. There is no serious political/economic alternative to some form of capitalist organization in any major economy. Those who wish there were some such alternative cannot articulate one beyond ad hoc redistributionism. One reason we had a stillborn debate about monetary arrangements is that the Fed chairman learned from Friedman not to permit a credit freeze to turn into a monetary implosion. Even the emergence of the apolitical engineer as the exemplar of the modern economist owes much

to Friedman's "The Methodology of Positive Economics" (1953), wherein he instructs us to become scientists—get our model straight, derive implications and then test them. The next Friedman is hemmed in on all sides—by lack of great controversy within and outside the profession, career concerns centered on technical proficiency rather than asking large questions, and no great pressure from inside or outside for doing anything much different. But one wonders still: is this only the calm before the storm?

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Why Is There No Milton Friedman Today?

Richard A. Posner¹

[LINK TO ABSTRACT](#)

The short answer to the question posed by *Econ Journal Watch* is that Milton Friedman combined, with enormous success both within the economics profession and in the society at large, three distinct roles. He was an analyst, using conventional analytical methods, of technical issues in macroeconomics, such as business cycle economics (with emphasis on the role of money), inflation and deflation, international exchange rates, and the consumption function. He was an advocate on economic grounds of specific public policies, such as an end to conscription, a negative income tax (of which the Earned Income Tax Credit is a variant, adding a work requirement surprisingly absent from Friedman's proposal), and school vouchers. And he was a public intellectual/guru/philosopher, advocating in books, magazines, op-ed pages, and electronic media a politically controversial return to libertarian (equivalently, free-market, laissez-faire, classical-liberal) economic policy.

Friedman also made an influential contribution to economic methodology in arguing that an economic theory should be tested by its predictive accuracy alone, without regard to the realism or unrealism of its assumptions (Friedman 1953). The argument is questionable because, owing to the difficulty of assessing such consequences (reliable experiments concerning economic behavior are very difficult to conduct), economists have tended to rely heavily—certainly Friedman did—on assumptions, often assumptions based on priors rooted in ideology, personality, or other subjective factors. In Friedman's case, the dominant prior was

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a version of classical libertarianism based on a faith, akin to a religious faith, rather than on robust empirical findings.

With the growing attention that economics has received in the wake of the 2008 financial crash and the ensuing global economic crisis (from which the world, including the United States, has, five years later, not yet recovered), and with the rampant growth of the electronic media, an increasing number of economists attempt with more or less success to combine Friedman's three roles, doing technical economics, advocating economic policy changes, and opining on issues of economic policy in the media. But no living economist has the scope, or is having the political impact, of Friedman—or seems likely to. In part this is because economics has become a more technical, specialized, mathematized field since Friedman's day, increasingly attracting to the field persons with mathematical skills and interests rather than with the broad policy and political interests of a Milton Friedman. In part it is because few if any economists have Friedman's rhetorical skills or political passion. (Oddly, Friedrich Hayek, who had none of Friedman's rhetorical skills, was a classical libertarian with influence in Europe comparable to Friedman's in the United States.) A related point is that writing well is no longer valued in economics as it once was. This is a natural concomitant of increased mathematization of a field.

But I think there's a more interesting reason why we are not likely to see another Friedman, at least if to be "another Friedman" an economist must be a zealot for libertarian economics; for had Friedman lacked that—the faith I mentioned and the determination and ability to propagate it—he would be remembered only as one of a number of very important economists contemporary or nearly contemporary with him, such as Kenneth Arrow, Gary Becker, Ronald Coase, John Hicks, Herbert Simon, Paul Samuelson, Robert Solow, George Stigler, and James Tobin, rather than being frequently bracketed with John Maynard Keynes—generally regarded as the most influential twentieth-century economist—and thus put ahead of the other distinguished economists whom I've listed.

The more interesting reason is that Friedman's mature work, which can be said to have begun with his book *Capitalism and Freedom* (1962), largely overlapped economic and associated political developments that provided support for Friedman's economic philosophy. His timing was perfect! The combination of slow economic growth and high inflation in the 1970s, coupled with growing evidence of excessive regulation of the transportation, telecommunications, broadcasting, electrical power, and other major industries, gave rise both to a deregulation movement at the end of that decade and to increased receptivity to Friedman's emphasis on the economic virtues of free markets. His economic doctrines were eagerly embraced by both President Reagan and Prime Minister Thatcher, and by the governments of the newly free nations of central and eastern

Europe when Soviet control of the region suddenly collapsed at the end of the 1980s. That collapse, moreover, seemed an empirical vindication of free-market economics.

Yet Friedman's influence on public policy, like that of Hayek in Europe, as distinct from the celebrity generated by the congeniality of his ideas to government leaders, probably was small. If one asks oneself whether Reagan and Thatcher needed Friedman to see the economic libertarian light, the answer is likely to be no. For it is not as if Friedman ever made a compelling economic case for libertarianism. His main argument for it, like Hayek's, was not so much economic as moral and political. People had a "right" to guide their lives, decide what to buy, what to pay an employee, etc. The government should respect that right rather than try to increase social welfare by regulatory programs or by redistributive policies requiring heavy taxes. Markets could safely be left alone because they are self-regulating (Adam Smith's "invisible hand"). A powerful government leads to tyranny. These were assertions not backed by evidence. Capitalism beats communism—sure; we knew that much even before communism collapsed. Yet the 1950s were a highly prosperous period for Americans and the overall American economy, but according to *Capitalism and Freedom* the scope of regulation and level of taxation were dangerously high then, for it was against the economic policies of that decade that Friedman was writing. It is the period since 2001 in which Friedman's ideals have been most fully realized in the United States, yet this has been a period of economic and political decline, in significant part as a result of regulatory laxity in banking and securities regulation (financial markets, it turns out, are not self-regulating) and exaggerated belief in the economic efficacy of reducing taxes as a method of stimulating economic activity, a dogma that risks starving government for funds needed for infrastructure, basic research, and combating global warming. What is called "behavioral economics," including its subfield "behavioral finance"—but should be called the psychology of economic activity—has undermined the economic model of man as a rational maximizer of his self-interest and helped to expose the rampant exploitation by business of consumer psychology. Businesses know, and economists are learning, that consumers are easily manipulated by sellers into making bad choices—choices they would never make if they knew better—in borrowing and investing, and in buying goods and services, such as food, health care, and education.

Although economic libertarians advocate a number of sensible reforms, such as ending the war on drugs, authorizing physician-assisted suicide, allowing the sale of kidneys and other organs, deregulating the adoption market, abolishing tax deductions for employer-provided health insurance, liberalizing immigration, privatizing the postal service, and abolishing agricultural subsidies, many of these and other libertarian reforms are politically infeasible. The Friedmanite tide has

crested and is receding. Reagan and Thatcher are dead, and a second coming of Friedman does not seem imminent.

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Why Is There No Milton Friedman Today?

Robert Solow

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My response to this question comes in two parts. First, there is no reason to expect the appearance of another Milton Friedman at either end of the political-economy spectrum. I suspect that the Milton Friedman that we had was the result of a rare coincidence.

There was of course the person: Milton was an ideologue, a True Believer, not given to skepticism or self-doubt. There are many of those, but he was an ideologue equipped with a very sharp and quick mind. And, what is not the same thing, he was a superb debater and a happy warrior: relentless, plausible, tactical, convincing, good with an audience, always smiling. (When we argued, he would often say: “Bob, I don’t understand you.” What he meant was: “How can you be so stupid?” But he would not say that.)

That was half of the coincidence. The other half was that the time was right. No doubt Reagan and Thatcher were able to profit from Milton’s skills as an economist-ideologue. But Reagan and Thatcher were riding a much more profound and pervasive right-wing surge. Its roots lay in deeper social forces than mere free-market ideas. (No doubt their personalities counted for something too.) Reagan and Thatcher would have been much the same and as successful without Milton’s reinforcement. But the combination certainly helped to promote Milton into a star.

After all, why should there be another Milton Friedman in the future when none had appeared before him? I suppose the likeliest candidate for that role in the past was J. M. Keynes. He, too, had an extraordinarily sharp and quick mind, according to all reports of contemporaries, and appears to have had the same facility—and joy—in debate. But there is a vital difference: Keynes was not an ideologue. Of course he took strong positions, and had no lack of self-confidence.

But Keynes avoided extremes, and changed his mind often. The usual complaint was that he was too flexible. There was a joke that a group of 12 economists would have 13 opinions on any significant issue, because Keynes would have two. You would never read Friedman's name into that sentence.

So Keynes was not a Milton Friedman type of the past. I can't think of anyone who was. (I hope no one wants to mention Karl Marx, because that would raise many different kinds of questions.) It goes without saying that there are talented economists today who want to and do play a public role. It does not seem to me that any of them have or want the same kind of identification with a Cause.

The second part of my response to the question is that I'm glad there is no Milton Friedman anywhere on the political-economy spectrum today. I think that Milton Friedmans are bad for economics and bad for society. Fruitless debates with talented (near-)extremists waste a lot of everyone's time that could have been spent more constructively, either in research or in arguing about policy issues in a more pragmatic way. I suppose that such debates also help to clarify implicit assumptions and shady arguments, but I think that is a small benefit compared with the cost in sheer hassle.

I know where I stand on at least some of the unresolved issues about economic theory that divide the profession, and I know where I stand on at least some of the live issues of economic policy. Listening to hard-liners on either side of those questions does nothing for me, sometimes less. I have had the experience of listening to someone argue my side of an issue in such take-no-prisoners terms that I find my initial convictions weakening, or I even start looking for counter-arguments against my own position.

Most economists realize that serious questions in economics are rarely, if ever, simple or transparent. (By "serious" I mean "embedded in a real-world situation" as well as "important.") Too much may depend on implicit assumptions or neglected, inconvenient influences or unspecified values. Everybody, except perhaps the next Milton Friedman, knows what I mean.

To be quite clear, I am very much in favor of good, clear-minded economists playing a part in public debates, explaining the economics of complex problems that civilians do not understand, and arguing aggressively for their own, explicitly stated, positions on matters of principle and policy. That happens now: we all know examples all along the political-economy spectrum, though probably not as extreme as Milton Friedman. They, at least the valuable ones, are not Milton Friedmans (or perhaps I should say *milton friedmans*). If they were, the question motivating this symposium would be empty.

This is not an easy role to play. It is hard to explain economic mechanisms simply and well—the "well" is harder than the "simply"—with a level of technicality finely tuned to the audience. The Lorelei are singing: "These folks

aren't going to understand how externalities affect market equilibrium, so just forget about them." The ideologue listens.

About the Author



Robert Solow is Institute Professor Emeritus at the Massachusetts Institute of Technology, where he has been a professor of economics since 1949. Professor Solow studied at Harvard and received the Nobel Prize in Economics in 1987 for his theory of growth. For a number of years he served as member of the Board of Directors of the Federal Reserve Bank of Boston and was Chairman of that Board for three years. He is past president of the American Economic Association and the Econometric Society, a member of the National Academy of Sciences, a Fellow of the British Academy, and former member of the National Science Board. He is currently a Foundation Scholar at the Russell Sage Foundation.

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