

Sins of Omission and the Practice of Economics[†]

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This paper advances the proposition that economics, as a discipline, gives rewards that favor the “hard” and disfavor the “soft.” Such bias leads economic research to ignore important topics and problems that are difficult to approach in a “hard” way—thereby resulting in “sins of omission.” This paper argues for reexamination of current institutions for publication and promotion in economics—as it also argues for greatly increased tolerance in norms for publication and promotion as one way of alleviating narrow methodological biases. (JEL A11, B40)

1. Introduction

This article draws a distinction between “hard” and “soft.” It advances the proposition that economics, as a discipline, gives rewards that are biased in favor of the “hard” and against the “soft.” This bias leads to “sins of omission” in which economic research ignores important topics and problems when they are difficult to approach in a “hard” way. It recommends a reexamination of the institutions of publication and promotion of the economics profession.

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2. Hard and Soft

Since Comte (1853), it has been common to classify sciences according to a hard–soft hierarchy, with physics at the top and sociology, cultural anthropology, and history at the bottom.¹

This classification relates to precision; and it can be applied to subfields as well as whole disciplines.² Consider empirical methodologies within economics. Quantitative—as opposed to qualitative—analysis is one dimension of being harder. And, within quantification, causal statements are more precise than those that only concern correlation. Hence, empirical work that focuses on identification is considered especially “hard.” Economic theory is “harder” when it is expressed in mathematical models rather than in words; and mathematical models are considered harder when the math captures

¹See Cole (1983).

²See Smith et al. (2000).

fundamental underlying ideas/concepts more precisely.

3. *Sins of Omission: A Model*

This section presents a simple model of “sins of omission” in the spirit of Ellison (2002a).

An academic researcher selects from a set of possible research topics. These topics can be characterized along two dimensions: (1) hardness (i.e., the ease or difficulty of producing precise work on the topic) and (2) importance.³

The researcher values both hardness and importance; but the weight he places on hardness leads him to trade off hardness and importance in a socially nonoptimal way. In this sense, he is *biased*. (We will discuss the reasons for such bias presently but, for now, take it as given.)

Figure 1 depicts the solution to the researcher’s problem. While the researcher chooses a topic lying along the “frontier,” the frontier topic he chooses differs from the social optimum. His chosen topic (topic A) is both *harder* and *less important* than the social optimum (topic B).

If we aggregate across all researchers, we obtain a prediction about the “cloud” of topics the profession will address. Observe that there will be a set of important but soft topics that will not be pursued; in this sense, bias toward the hard in the profession generates “sins of omission.”

³ In Ellison’s model (2002a), researchers face a trade-off between investing in the q-quality of their research (which is equivalent to importance) and the r-quality of their research (which is equivalent to hardness). Ellison argues that different norms may prevail in the profession that place different weights on q- and r-quality.

4. *Reasons for Bias toward Hard*

The question remains: why do economists have hardness bias? I suggest three possible reasons, which also, at least partially, explain why this bias has become stronger over time.

Reason 1: Place in the Scientific Hierarchy.—In their article “The Superiority of Economists,” Fourcade, Ollion, and Algan (2015) argue that economists “see themselves at or near the top of the pecking order among the social scientists.”⁴ Economists take great pride in their view of their discipline as “the most scientific of the social sciences,” and they look down upon sociologists and political scientists for their “less powerful analytical tools.”⁵ This desire for place in the pecking order, I would argue, is a leading motive for hardness bias.

Reason 2: The Evaluation Process.—Rewards such as journal acceptances are generally meted out by committees (in the case of journals, the committees consist of editors and referees). When rewards are scarce, obtaining them requires that most/all committee members consent.

Precision is a relatively well-defined concept; hence, it is easy for people to agree regarding the hardness/softness of research. In contrast, importance is fuzzy, so that it is relatively easy to disagree regarding its importance. This tendency for disagreement on importance is exacerbated by tendencies to inflate the importance of one’s own work and deflate the importance of others’.

The implication is that evaluations by committees will be biased toward the hard.

⁴As slightly paraphrased and rearranged from the original. The phrase “see themselves” was italicized in the original.

⁵Also quoted by Fourcade, Ollion, and Algan (2015) from Freeman (1999, p. 141).

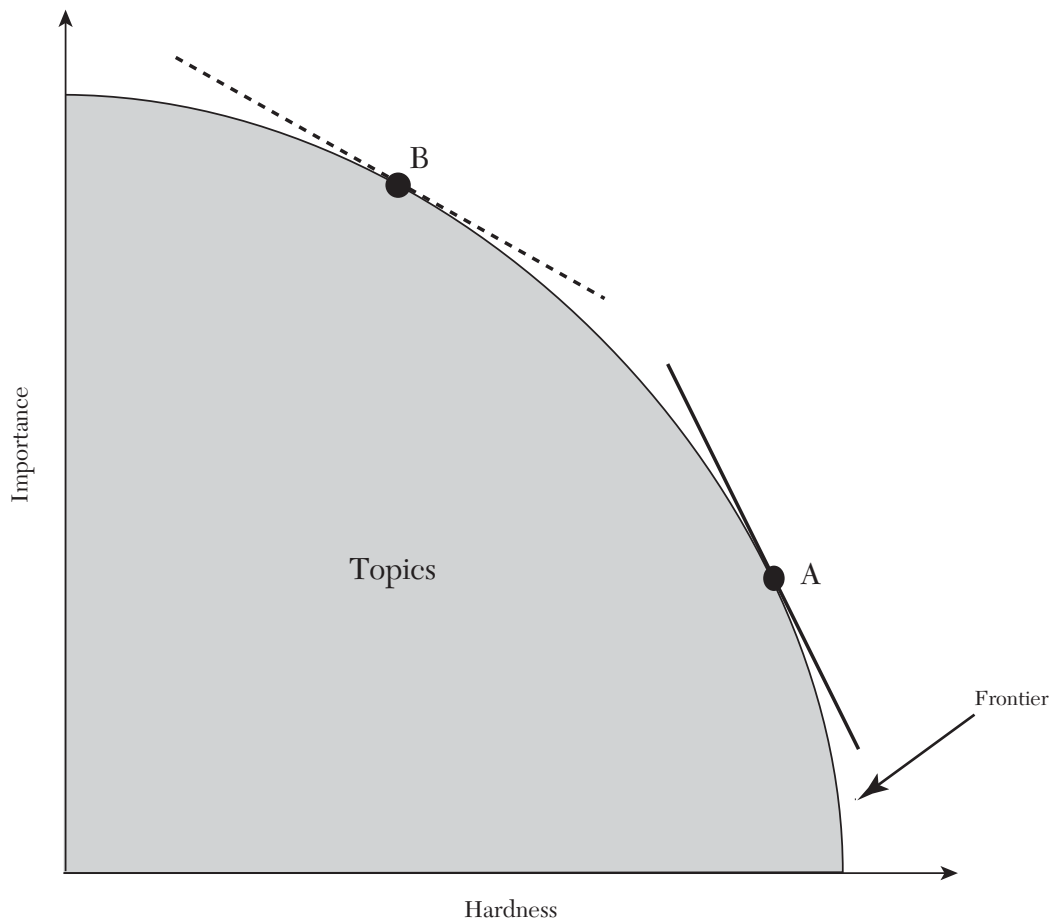


Figure 1

Note: Trade-off between hard and soft with choice of topics on frontier according to maximization of individual utility at point A, and according to maximization of social welfare at point B.

Furthermore, the scarcer the rewards, the greater is the likelihood of such bias.

Reason 3: Selection into the Profession.—Academics are not all the same. The greater the bias toward the hard in the profession, the greater will be selection into it of those with intrinsic tastes in that direction. Indicative of such selection, Mankiw (2006) has advised prospective applicants

to economics PhD programs to “take mathematics until it hurts.” But, Mankiw gently added that in his opinion these standards are too strict; if he were a member of the admissions committee, he “might argue with [his] colleagues’ ... excessive [*sic*] fondness of mathematics.”⁶

⁶ Mankiw (2006).

Just as rewards affect selection, so too does the mix of types within the profession affect rewards. When hard types are prevalent, they occupy more of the profession's powerful positions (such as journal editorships). From these prominent positions, they bias rewards: for instance, by selecting harder articles for publication. Of course, the same bias will affect promotions.⁷

One reason the profession seems to have gotten harder in recent years is a negative feedback loop. Biased rewards have caused the profession to *intrinsically value* hardness more; the intrinsic value placed on hardness has led to more biased rewards.

5. Some Consequences of Hardness Bias

This section explores three consequences of hardness bias.

Consequence 1. Bias against New Ideas.—So far, we have classified topics according to their “importance” and their “hardness.” Another relevant dimension is whether topics are *new* or *old*—or, in Kuhn's (2012) terminology, whether they entail “normal” or “revolutionary” science. Not all new topics

⁷Two papers—one by Brock and Durlauf (1999), the other by myself and Pascal Michailat (Akerlof and Michailat 2017)—show that beliefs in a scientific field will converge if its practitioners have a desire for conformity. In Brock and Durlauf, scientists continually adjust their beliefs to reduce the distance between their thinking and the beliefs of others. In Akerlof and Michailat, evaluators of candidates for tenure are biased in favor of those with similar beliefs and also against those with different beliefs. In both cases, the beliefs converge. Furthermore, that convergence will not necessarily be to the truth (or to best practice). On the contrary, because of reason 1 (the role of hardness in the scientific pecking order) and reason 2 (its facilitation of agreement), following from the comparative statics of equilibrium in Akerlof and Michailat, those uniform beliefs are likely to have hardness bias in turn. These theoretical findings of belief convergence under rather general conditions accord with Kuhn's (2012) view that scientists base their work on commonly-held paradigms. Those paradigms do not just pertain to subject matter; they include, as well, beliefs about appropriate methodology for the respective field.

are important; but, clearly, the most important topics are new. Hardness bias inhibits acceptance of new topics in at least two different ways.

First, old topics/paradigms have a variety of tools that aid precision: such as established terminologies, conceptual frameworks, and empirical methodologies. With bias toward the hard, academics working within such accepted paradigms have an advantage, since they can borrow at will from such toolkits to state their ideas precisely. In contrast, those who are presenting a new idea are disadvantaged, since they must develop their own tools. As expressed by Frey (2003, p. 212): “a new idea is less well-formulated than ... well-established ideas and therefore rejected for lack of rigor.” In this way, demand for precision (for hardness) impedes the introduction of new ideas.

Second, hardness bias reduces the ability to challenge existing paradigms. According to usual procedure in economics, as in science more generally, old ideas are only rejected when they are shown to be inferior in tests against new ideas. Since Friedman's (1953) classic essay, it has become all-but-uncontestable that new theories need to generate *testable predictions*. This belief may seem innocuous; but, in point of fact, it involves rejecting softer tests of theories, such as those that evaluate models based upon the quality of their *assumptions* as well as the quality of their *conclusions*. It especially entails exclusion of evidence from case studies, whose detailed evidence can be highly informative regarding context and motivation.⁸ While harder tests with statistical data may be a gold standard, restricting the set of permissible tests reduces—perhaps greatly—the ability to test theories. Hence, bias toward the hard makes us too accepting

⁸For the advantages of case studies, see Flyvbjerg (2006).

of existing theory and insufficiently willing to be self-critical as a profession.

Consequence 2. Overspecialization.—Bias toward the hard also encourages overspecialization. Generalists need to meet the standards of precision for multiple fields, while specialists need only meet the standards of one. Hence, it is easier to be hard as a specialist than as a generalist. The greater the bias toward the hard, the more specialization we should see in economics.

Indeed, specialization appears to be increasing in our field. As one symptom, departments are increasingly balkanized into subfields, each, for example, with its own respective seminar series. The proliferation of subfield journals is another symptom of the trend.⁹

Consequence 3. The Curse of the Top Five.—Hardness bias is also a cause of “the curse of the top five.” As documented by Heckman and Moktan (2017), tenure and promotion committees are increasingly relying upon journal metrics to make decisions, with the number of “top five” publications given particular weight.

Tenure, like most other rewards in the profession, is meted out by committees. One approach tenure committees can take is to evaluate the quality of candidates’ work. However, discussions of candidates’ work are likely to be fraught—especially when the profession is balkanized into subfields. As already explained, academics disagree about what is important; and there tend to be systematic differences of opinion across subfields.

This problem can be finessed by evaluating candidates by metrics. It is relatively easy to reach agreement regarding the quality

of journals—particularly those journals that are general interest, in which all subfields are represented. It is perhaps no surprise then that the most heavily weighted journals in the economics-tenure process (the “top five”) are general interest.¹⁰ (As Casadevall and Fang 2014a have also observed, there is an additional benefit in the use of journal metrics—for the “lazy.”)

6. *State of the Profession*

A collage of statistics suggests that research economists, especially young ones, find themselves in an environment that could easily lead to sins of omission because of excessive demands for compliance in favor of the hard relative to the important. Assistant professors at research universities are not in a good position to put up much fight against the dictates of what the journals want: especially, when, as documented by Heckman and Moktan (2017), top five acceptances play an outsize role in grant of tenure.

The demands by the journals are just one more in a long series of previous demands for academic compliance that begin in high school, if not yet earlier. These are demands: for high-school performance to obtain college acceptance; to obtain sufficient grades/letters of recommendation/GRE scores for admittance to graduate school; to obtain the PhD; for a graduate-school record sufficient to obtain a ladder-track academic job. Of course, all that compliance, usefully, forces economists to master the field’s current paradigm; those who wish to correct its omissions have special need for such understanding. But, just as there can be too little demand for compliance, there can also be so much that important problems are neglected: either because the problems themselves, or the best

⁹In line with the themes of this article, there has been push-back against excessive specialization (and also hardness) in biology. See Casadevall and Fong (2014b).

¹⁰Weighting publications by their journal impact factors is another mechanism for reaching consensus.

ways to tackle them, are deemed outside the frame of what is acceptable in the journals.

The statistics on acceptances suggest the need by researchers, especially those on a short tenure clock, to accord with the journals' demands. At the journals, rejection is the mode. According to Card and DellaVigna (2013), circa 2010, those influential "top five" had acceptance rates of only 6 percent. That figure was down some 60 percent from 15 percent some 30 years earlier.

These low acceptance rates are not only at the top five. For even the very top economics departments, only a small fraction of PhD graduates have a significant number of publications six years after receipt of their degrees. Conley and Önder (2014) counted what they call "AER-equivalent" publications of the annual cohorts of US PhD recipients from 1986 to 2000. "Even at the top five departments," they say, "it would be hard to agree that the bottom half of their students are successful in terms of economic research. The AER-equivalent papers at the median at year six was below 0.1 for all five of these schools, and in fact at zero in most of them."¹¹ Furthermore, even the eightieth percentile of graduates of all economics PhD programs, with the exception of Princeton (at 1.01) and Rochester (at 1.14), was less than one such AER-equivalent publication.¹² These same data do not just show the low level of publication of these cohorts, they also show rapid declines over time; between the 1987 and 1999 cohorts, AER-equivalent publication six years after graduation fell by approximately 40 percent, for all of the ninety-ninth, ninety-fifth, ninetieth, eighty-fifth, eightieth, seventy-fifth, and fiftieth percentiles (Conley et al. 2013, table 3, p. 1263).

¹¹Conley and Önder (2014, p. 212). "At zero" means rounds to zero, rather than to 0.1; .1 means rounds to 0.1.

¹²Their findings were, however, not without a silver lining: although the top-ranked schools did better on average than those of lesser rank, they did not dominantly do so.

These findings of Conley and Önder are also in agreement with a ten-year-afterwards survey of US PhD graduates of the academic year 1996–97 (Stock and Siegfried 2014).¹³ For the approximately half of respondents who had initially received "permanent academic positions" (i.e., tenure-track jobs), median top-50-economic-journals publications was 1; the mean of such publications was 2.0.¹⁴

Young academic economists facing the tenure clock thus have little choice. Even if they have opinions different from what's acceptable to the editors and referees, they still must comply. First, even before beginning a paper, they must consider whether the final product will be journal acceptable. And then they must decide how to frame it; for example, as an "AER-paper," or possibly as an "REStud." We have no statistics, in this regard, concerning how initial decisions are influenced by conceptions regarding what the journals will or will not accept; but Ellison (2002b) has compiled statistics regarding another aspect in which the journals have increasingly taken over from the authors.

According to Ellison, before the 1960s, "revise and resubmits" were fairly rare (2002b, p. 984). Insofar as they occurred at all, the author would quickly submit the revision; rejection was uncommon. But an array of statistics (Ellison 2002b) shows that the length of time between submission and final acceptance has increased greatly (both in economics and in other fields). The average increase at nine economics journals

¹³However, low response rates to the survey means the findings should be interpreted with considerable caution. Only 207 of the approximately 950 JEL-recorded graduates of that year responded to this survey, which was ten years after receipt of degree.

¹⁴Among the one-third of these initially lucky ones, who had received tenure at their initial institution, median top-50 publications was yet lower at 0 (perhaps because of low standards for tenure at their schools); as was the mean, at 1.8 (Stock and Siegfried 2014, table 4, p.297).

for which the data were available was 185 percent—from 6.1 months in 1970 to 17.3 months by 1999.¹⁵

Ellison does a further analysis for different journals regarding how these increases are divided between submission and receipt of first review, and between initial request for revise and resubmit and acceptance. He summarizes the evidence as saying that “[roughly] one-quarter of the slowdown may occur because journals may take longer to conduct initial review”¹⁶; so that the remaining three quarters of the increase are due to the increasing demands for revise and resubmit. But that process is, of course, almost entirely about answering the referees, mostly to make these to-be-accepted papers more precise. This evidence is symptomatic of increasing emphasis on hardness. Additionally, it shows that much of this increased demand for hardness is coming from demands by the journals themselves.

Ellison also suggests another indicator: pages per article. Economic journal articles, he says, have (2002a, p. 994–995) longer introductions, more extensions of main results, and more references. And all of these increases are, of course, associated with what this paper calls “greater hardness.” At the top five, between 1970 and 2010, paper length has almost tripled.¹⁷

In sum, the economics profession, especially for younger researchers, is highly competitive; it has rapidly become more so. The market for academic research, which is the economics journals, leaves the researchers with no choice but to foresee the dictates of editors and referees, even in their initial conception of papers. They must continue

to comply with those editors and referees, especially after they have been lucky enough to receive a revise and resubmit. The statistical evidence suggests what probably every research economist of my age knows from personal experience: as time has passed those demands have become increasingly insistent and have increasingly emphasized hardness.

Furthermore, the emphasis on hardness is likely at the expense of importance. A survey of economics graduate students by Colander and Klammer (1987, table 4, p.100) thus found that only 3 percent of economists thought it “very important” for their success to “have a thorough knowledge of the economy”; in contrast 65 percent thought it “very important” to be “smart in the sense of being good at problem solving.” Additionally, when asked retrospectively about their PhD programs’ emphases, more than half of two separate graduate cohorts said that their programs placed “too little emphasis on applying theory to the real world” (Stock and Hansen 2004, p. 267, table 1). These opinions are thus suggestive of an environment that could spawn sins of omission because of bias toward the hard and against the important.

7. *Examples of Sins of Omission*

This section presents a few examples of sins of omission from economics that are related to my own recent research. The following section will discuss implications of these examples.

Failure to Predict the Financial Crisis.—In the aftermath of the financial crisis of 2008, economists asked why no one had predicted it, at least exactly as it happened. Rajan (2011) said that such a prediction had not been made since it would have required detailed knowledge of theory and institutions in the disparate specialties of finance, real estate, and macroeconomics.

¹⁵Ellison (2002b, table 1, p. 953).

¹⁶Ellison (2002b, p. 958).

¹⁷Card and DellaVigna have computed that between the early 1970s and 2011–2012, the average page length of papers at the top five increased from 16 pages to 45.5 (Card and DellaVigna 2013, p. 150 and figure 4, p. 151). Page lengths here were “adjusted for density.”

Curiously, prior to 2008, those subfields had laid out all the elements that were later deemed to have been the cause of the crisis.¹⁸ Those contributions included the possibility of a fire-sale crash in asset prices, driven by the posting of dodgy assets as collateral; other sources of tail risk; a housing bubble; the erosion of standards for mortgage loans; the conflict of interest by ratings agencies paid by the issuers of the securities that they rated; and interaction between the macroeconomy and the financial system.¹⁹ All the elements were there. But only Rajan (2005) came close to crossing all the necessary subfield boundaries necessary to predict the crisis as it historically occurred.

There were incentives to present the key pieces of the puzzle, but none to put them together. Following Caballero (2010), regarding theory, a model with all the pieces could not have been published; it would have been considered too far from precise, simple ideas (such as those that motivate simple new Keynesian or dynamic stochastic general equilibrium (DSGE) models); and, in this way, too soft to merit publication.²⁰

¹⁸See, for example, the report of the Financial Crisis Inquiry Commission (2011) regarding those various causes.

¹⁹Reviews postcrash along with the precrash articles themselves indicate that every significant aspect of the crash had been the subject of work by economists: for example, on fire-sale crashes, see Shleifer and Vishny (2011) and Kiyotaki and Moore (1997); on mis-accounting of current profits that would encourage tail risk (for example, see Healy and Palepu 2003 and Partnoy 2003); on the housing market (for example, see Gramlich 2007 and Shiller 2012); on conflicts of interest regarding payments to ratings agencies (for example, see White 2010 and Jiang, Stanford, and Xie 2012); and on the interaction between the macroeconomy and the financial system (see, for example, Bernanke and Gertler 1995).

²⁰Caballero (2010) thus explains why macroeconomists did not predict the crisis. He divides macro models into what he calls “core” and “periphery.” The core, he says, is a DSGE or a standard Keynesian model. There was also a considerable peripheral literature, which explored deviations from this core, but only one such deviation at a time. Caballero gives the methodological reason for such a modeling strategy: “The periphery is about isolating specific mechanisms. [Therefore] it surrounds the sources of these

Regarding predictions from empirical evidence, the crucial data would have been of the wrong form. Data on tail risk would have been revealing. But an economist who had been lucky enough, or insightful enough, to obtain such data and perceive its implications would have had another hurdle to cross. Even if she had uncovered, for instance, AIG’s \$533 billion of commitments to insure securities such as CDs,²¹ she would have still needed to turn it into the basis for a publishable paper. Those \$533 billion indicated tail risk of sufficient size to threaten a gigantic crash of the financial system; but it was only a single number. It was not the statistical evidence that typically underlies empirical papers in economics.

This example of hard standards resulting in a sin of omission is still of importance today, some ten years after the crash. Reinhart and Rogoff (2009) have told us that “This Time Is Different”—meaning that it isn’t.²² Now, in 2018, for the sake of prevention, policy makers continue to need predictions of when, where, and how the next crash can happen—as much as they had needed such analysis back in the early 2000s. The hard standards for what is publishable meant that there was no incentive to make such a prediction then. That remains true now.

Motivations.—In traditional economic theory, motivations come from a priori assumptions regarding what people plausibly maximize. But there is a much less

mechanisms with assumptions designed to kill unwanted effects that would pollute the message” (p. 91). He says that theoretical models were allowed to make one deviation from either a standard Keynesian model or a DSGE model at a time; but the several deviations needed to predict the crisis were outside the range of the publishable.

²¹Financial Crisis Inquiry Commission (2011, p. 141).

²²Two other incidents prior to 2008 demonstrate the vulnerability of the modern economy to financial crash (even prior to the mortgage-backed securities bubble of the early 2000s). See Edwards (1999) and Lowenstein (2000) on the threat of bankruptcy of Long-Term Capital Management and Leland and Rubinstein (1988) on portfolio insurance and the crash of 1987.

restrictive, and more general, characterization of the range of possible motivations.²³ that people are motivated through the stories they are telling themselves at the time they make their decisions. In turn, insofar as human thinking can be described as occurring through stories, that means that people are motivated through the stories they are telling themselves.²⁴ The core of sociology and cultural anthropology is ethnography, whose goal is to uncover and interpret the stories that people are telling themselves. But the case-study, interpretive methodology of ethnography is considered soft.

The preceding logic suggests that, generally, the biases against the soft and the new cause behavioral explanations to be downplayed in economics. The logic further suggests that our class of behavioral models is still limited and have yet to sufficiently incorporate ideas from sociology and anthropology that emphasize the importance of stories.

Case studies help us see what constitute good assumptions for our models, as they also help make the case for behavioral over classical models. Economists' current Friedman-type approach, which eschews testing models based on assumptions, keeps us away from case studies. A good hunting ground for sins of omission will concern the stories people tell themselves, but that are outside the range of what economists would a priori surmise underlie "utility." Four examples will follow: each of them illustrating the unappreciated role of stories in economics.

²³This generality goes considerably beyond the considerations in current behavioral economics.

²⁴See Akerlof (2017) and Collier (2016) for the role of stories in economics. Likewise, Morson and Schapiro's (2017) *Cents and Sensibility* also emphasizes stories as a missing factor in economists' representations of motivations. McCloskey has also stressed the role of narrative; for a summary of her recent views on "what is wrong with economics," see McCloskey (2014).

*Example 1. The Soviet Union.*²⁵—The analysis of the economics of the Soviet Union has demonstrated the failures of its system of centralized planning.²⁶ But this analysis has ignored another, perhaps equally negative aspect of the Soviet economy. The Bolsheviks promoted the story that planned, forced industrialization would rapidly create an economic paradise, so that even the smallest interference with the plan would warrant the most severe punishment.²⁷

This story legitimated much cruelty. According to the first Five-Year Plan, industry would feed tractors to agriculture to make grain and agriculture would feed grain to industry to make tractors. But according to the story, when the deliveries of grain in the Ukraine fell short of plan targets, the fault could not be with the plan. Instead, the shortfalls must be due to saboteurs, who were quickly identified as kulak farmers and duly deported—some to Siberia. Those deportations, in turn, reduced grain deliveries yet further, especially since these richer peasants had been contributing more than their share to the plan's targets. As a final step, the peasants of the Ukraine were then forced onto collective farms, where they would use the tractors presumptively being produced by industry. This move aggravated the disaster yet further, since taking a horse (or a cow) onto a collective farm would have been a huge liability; it would have identified its owner as a "kulak." When the tractors either did not show up, or broke down if they did, grain output fell yet further. The Ukrainian famine, the *holodomor*, followed. At every step in this tragedy, "the story" played a major role, as it legitimated the forced measures used to carry out the dysfunctional plan.

²⁵This example is based on Akerlof and Snower (2016).

²⁶See, for example, Ericson (1991) for such analysis.

²⁷Also see Garai (2017) for such an interpretation of the role of identity under Soviet Communism.

Example 2. Smoking and Health.—There is a fine literature on the economics of smoking. For example, it estimates the effects of tobacco taxes on the demand for cigarettes.²⁸ But, regarding smoking and health, another type of public policy—largely ignored by economists—has also been remarkably successful. In the early 1960s, the US Surgeon General, Luther Terry, convened an advisory committee to spell out what was known about the question. The resulting report, *Smoking and Health: Report of the Advisory Committee of the Surgeon General of the Public Health Service*,²⁹ changed the legitimacy of views regarding smoking. With this document, the US government officially created the story: “smoking is stupid.”³⁰ It thereby refuted the contentions of the tobacco industry that the relation between smoking and health was undecided. Anti-tobacco activists then used this story down the road in crucial actions that resulted in the prohibition of tobacco ads on radio and TV and later, that justified regulations against indoor smoking in public places. (This ban on indoor smoking has been remarkably effective; with each puff, the outdoor smokers’ expressions propagate to all passers-by the original message that “smoking is stupid.”³¹) From the time of the Surgeon General’s Report to the present day, the fraction of adult smokers in the United States has declined from 42 percent to 15.5.³² The role of the story has only a walk-on part in smokonomics;³³ but, absent

the restrictions against soft theory and soft evidence, this story would be a star of the show.

Example 3. Global Warming.—The role of stories falls outside the range of the standard economics of global warming. Yet, beyond the physical problem of climate change itself, there is a second inconvenient truth. Among the US public, there are not only those who view global warming as outright hoax;³⁴ many more also fail to perceive its urgency.³⁵ The stories that justify continued inaction, year after year, are as important as the physical reality of global warming itself.³⁶ The impact of those stories, how they are formed, and how they might be altered, are as important as issues such as cap and trade arrangements and carbon taxes that are now central to the economics of climate change.

Example 4. Macroeconomics.—The role of stories in economics has been stressed by Shiller (2005) for some time; his American Economic Association presidential address on “Narrative Economics” (Shiller 2017) provides many further examples. Among them,³⁷ Shiller says that the near-constancy of

reluctant to vote for tobacco-tax increases in states with greater anti-smoking sentiment. This work goes a significant way to bringing the “smoking-is-stupid” story into smokonomics. But “anti-smoking sentiment” need not just be an independent variable, as in DeCicca et al. To explore the full role of “anti-smoking sentiment,” it must also be a dependent variable. That also adds an omitted policy variable to anti-smoking policy.

²⁸ See, for example, in DeCicca, Kenkel, and Mathios (2002).

²⁹ US Department of Health, Education, and Welfare (1964).

³⁰ See Akerlof and Shiller (2015, chapter 8), for this interpretation, including what follows.

³¹ Brandt (2007, p. 267 and p. 288).

³² For 42 percent smokers in 1964, see US Department of Health, Education, and Welfare (1979, table 2, p. A-10). For 15.5 percent current smokers in 1917, see US Centers for Disease Control and Prevention (2017).

³³ DeCicca, Kenkel, and Mathios (2002) and DeCicca et al. (2008) show that estimates of the elasticity of demand for smoking will be overestimated if legislators are less

³⁴ See, for example, Inhofe (2012).

³⁵ In a March 2017 Gallup environmental poll 45 percent of Americans said they “worried a great deal” about global warming. <http://www.gallup.com/poll/206030/global-warming-concern-three-decade-high.aspx>. But, when ranked with other issues they “worry about a great deal” it usually places at, or near, the bottom. In a March 2015 poll they ranked it fifteenth out of 15 issues. The previous year it had been fourteenth. <http://www.gallup.com/poll/182018/worries-terrorism-race-relations-sharply.aspx>.

³⁶ We further add, parenthetically, that the public’s antipathy to carbon taxes is another odd story that inhibits climate-change policy.

³⁷ Shiller (2017, p. 989).

the ratio between the money supply (M) and nominal income (Y) in the Great Depression may not indicate that M causes Y, as claimed by Friedman and Schwartz (1963). Instead, Shiller argues, it is likely that the stories people were telling themselves as the Depression unfolded and income declined would have decreased peoples' willingness to hold money. Thus, he attributes the conclusions of Friedman and Schwartz (1963) regarding the causality of M to the omission of a variable: the stories people were telling themselves.

8. *Comment on Examples of Sins of Omission and Why They Have Not Been Challenged*

The examples of the previous section allow us also to see a reason why many sins of omission in economics have remained unchallenged.

Kuhn's *Scientific Revolutions* (2012) describes scientific progress as occurring as "normal science" uncovers "anomalies" with existing, generally accepted paradigms. "Scientific revolutions" that explain accumulations of such contradictions then lead the way forward to new, better paradigms. But Kuhn's optimistic view of "scientific progress" fails to perceive a possibility that is particularly relevant to economics. Suppose the paradigm not only describes the *subject matter* of the field; suppose it also describes the field's appropriate *methodology*. In this case, observations that contradict the existing paradigm will be dismissed if they violate the prescribed methodology. The hardness police will rule them out as inadmissible evidence.

We can restate the previous proposition in another way. Webster's dictionary gives two definitions of "economics." First, it is described by its *subject matter* as "a social science concerned chiefly with description and analysis of the production, distribution,

and consumption of goods and services."³⁸ But, Webster's dictionary also has a second definition. Economics is "economic theory, principles, or practices." That corresponds to what is taught in graduate PhD programs in economics. According to Craighead (2010), "*Economics Ph.D. programs are trying to train students to become productive researchers, not to teach them about the economy*" [italics and underlining in the original]. That is, PhD students are taught the hard methods of economic research: mathematical modeling and statistical analysis.

A brief review of the examples of the previous section shows that none of them would qualify as "sins of omission," according to the second, methodological definition of the field: since each of the examples entails methods, or use of evidence, that is outside common practice taught in graduate schools. We saw (following Caballero) that theoretical analysis of the crash would have entailed going beyond the current methodology for economic theory; and its empirical prediction would have entailed examination of tail risk, for which the evidence was unlikely to be in statistical form. Furthermore, none of the four examples of the role of "stories" would have been classified as sins of omission with the methodological definition, since those stories, likewise, would be difficult to observe with statistical methods. However, with the first—subject-matter—definition of the field, each of the examples would be a respective sin of omission. Financial crashes are clearly within the purview of the subject matter of economics; and, with each of the four "stories," their omission significantly affects a respective economic problem of some importance.

³⁸ <https://www.merriam-webster.com/dictionary/economics>.

9. *Summary and Conclusion*

Before describing the implications of our analysis, it is important to emphasize what has—and what has *not* been—said. The theoretical and empirical accomplishments of modern economics, obtained with hard standards for the conduct of research, should be rightly celebrated. But such standards should not be uniformly applied to all economic problems. Especially, they should not be applied to those problems for which those standards are too restrictive: for lack of evidence or because motivation significantly differs from standard economic assumptions. Different terrains call for different vehicles. A sailboat is useless in crossing a (riverless) desert; a camel is useless in crossing a sea.

The norms regarding how economics should be done should call for flexibility of methodology—instead of insistence on methodological purity that might be perfect for some Important problems, but leaves other problems and other approaches outside the domain of economic research.

Historically, those paradigms—norms for how economic research should be done, and also for what constitutes “economic research”—have developed out of an evolutionary process. Neither the optimality of the resultant conclusions of the field nor of the resultant institutions for economic research can be taken for granted.³⁹ At the journals, the norms for what should or should not be published, and the selection of the editors and the referees, and their conduct, should be the subject of examination. Likewise, at the universities, the processes of promotion and tenure should also be examined. Just as medicine

in the United States was famously influenced by the Flexner Report of 1910 (Starr 2008), there is a need for a similar report today on publication and promotion in economics.

Such a report could be divided into two separate parts. The first part would analyze the norms regarding the role of journal editors and referees. As mentioned earlier, times between submission and acceptance are extremely long (Ellison 2002a, b), as authors and their ideas are strung out with often repeated requests for revise-and-resubmits according to the tastes of the editors and referees.

Returning ownership of papers to the authors would not only show greater respect to them. It would also accord with the stated purpose of two of the top-five journals: as the *AER* and *REStud* both have the word *Review* in their name. As I understand it, a “review” is a journal that takes submissions, and decides which to accept/which to reject. That means that the editors and the referees should be viewing themselves as helpmates, rather than dictators holding authors at ransom before accepting their submissions. A second part of the report would describe appropriate norms regarding criteria and methods of promotion. Special topics for examination would include the appropriate, and inappropriate, criteria based on publication metrics (such as the number in the top five), and, internationally, overdependence on publication in US journals and even on US data.

Recommended reform could reduce the sins of omission due to inappropriate emphasis on hardness. Furthermore, while not solving the problem of the competitive rat race for new entrants into the field, it would provide them some relief by encouraging them to bring out the best in themselves. And for all economists, it would allow us to express what we want to say as best we can: from the heart.

³⁹The nonoptimality from evolution follows from Brock and Durlauf (1999) and also from Akerlof and Michaillat (2017).

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