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Smoking in Restaurants: Who Best to Set the House Rules?

DAVID R. HENDERSON¹

A COMMENT ON: BENJAMIN C. ALAMAR AND STANTON A. GLANTZ, “SMOKE-FREE ORDINANCES INCREASE RESTAURANT PROFIT AND VALUE,” *CONTEMPORARY ECONOMIC POLICY* 22 (4): OCTOBER 2004, PP. 520-525.

ABSTRACT

ON THE WEB SITE OF *CONTEMPORARY ECONOMIC POLICY*, A JOURNAL PUBLISHED by the Western Economic Association International, the publisher highlights the Best Article Award for 2004. The award was granted to an article by Benjamin C. Alamar and Stanton A. Glantz (2004).

As the title of their article suggests, Alamar and Glantz conclude that “Smoke-free Ordinances Increase Restaurant Profit and Value.” The final two sentences of the article are as follows:

These results add to the growing body of literature that should give restaurant and bar owners a real economic incentive to support smoke-free laws. Despite the rhetoric that smoke-free laws hurt the restaurant business, the marketplace indicates that these laws increase the profits and the values of restaurants and bars and are good for business. (525)

One naturally wonders: We know that some people like to smoke and prefer restaurants where they can smoke. What about the restaurant that specifically caters to smokers? Do Alamar and Glantz say that such a restaurant will benefit from a ban on its niche? Or do they mean restaurants on the whole? If “on the whole,” have they adequately accounted for the restaurants and would-have-been restaurants that lose?

Here I argue that Alamar and Glantz’s article is bad economics. It shows

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unsound reasoning, presents empirical findings that do not lead to the authors' conclusions, and omits important considerations.

The authors note that 35 studies have “concluded that smoke-free policies had a negative impact on the hospitality industry; all of them were funded by the tobacco industry or organizations affiliated with the tobacco industry” (524). They remark repeatedly on tobacco funding of certain studies. I find these remarks on where research “is coming from” to be entirely appropriate. It is important to know if a researcher is enmeshed in an organization with a strong commitment and orientation.

Benjamin C. Alamar is a Ph.D. economist and, at the time of the article, was a postdoctoral fellow at the Center for Tobacco Control Research and Education, University of California at San Francisco. Stanton A. Glantz received his Ph.D. in applied mathematics and is professor of medicine at the same institution, the Center for Tobacco Control Research and Education at UCSF. In other words, both Alamar and Glantz were or are funded by anti-smoking sources. Glantz, moreover, is a well-known anti-smoking activist whose views have been covered by, among others, PBS.² Glantz was prominent in the fight to end smoking in California restaurants and bars.

My disclosure: Although I am a non-smoker who offered my daughter \$2,000 if she made it to age 21 without smoking (she did), I am also a defender of people's freedom in such matters. An article that I wrote in *Fortune* (Henderson 1997) led a law firm that defends tobacco companies to hire me as an expert witness. Although I withdrew early in the process, my withdrawal had nothing to do with the merits of the tobacco company's case, which I found to be just, and everything to do with my concluding that the law firm needed an historian, not an economist.

SEARCHING FOR EXTERNALITIES

Consider an example. Suppose the law allows restaurants to decide their policy on allowing people with T-shirts, as opposed to shirts with a collar. Restaurant owners have three choices: (1) a no-T-shirt policy; (2) a policy that allows customers with T-shirts everywhere in the restaurant; and (3) a policy that limits customers with T-shirts to a section of the restaurant. I know of restaurants in the first two categories and certainly can conceive of the third.

Now, imagine that the government mandates that no restaurants be allowed to serve customers who wear T-shirts. For a restaurant that had previously banned T-shirts, there would be no direct loss; the law would simply prevent the restaurant owner from doing something he or she had already chosen not to do. Moreover,

² See <http://www.pbs.org/wgbh/pages/frontline/shows/settlement/interviews/glantz.html>, accessed May 24, 2007.

the restaurants in this category could benefit in the short run from such a mandate. The reason is that some customers who would have gone to a restaurant that allows T-shirts and who, therefore, did not patronize the restaurant that banned them, would now have one less good reason to avoiding the restaurant that previously banned them.³ But the restaurants in the second and third categories would suffer a loss because they would be prevented from serving a segment of customers that they wished to serve. If an economist found that the government's policy actually increased profits for restaurants and increased the resale value of restaurants, other economists would want an explanation.

Now, let's turn to smoking. One might think that the analysis is fundamentally different because smoking is a health issue: smokers' smoke affects the health, and certainly the comfort, of non-smoking customers. But because these non-smoking customers can choose whether or not to patronize a restaurant that allows smoking, the restaurant owner can capture a normal share of the value to them by disallowing smoking. If other customers value a smoke-free experience enough, they will be willing to visit more frequently or even pay a premium for it. Thus, a restaurant owner can then weigh those extra payments against the lost business of those who will shun the restaurant because they wish to smoke. The fact that second-hand smoking has a health dimension does not make it fundamentally different from T-shirts, which has a "second-hand" aesthetic dimension.

Of course, not just other restaurant patrons are affected by smoke. Waiters, waitresses, hosts, and hostesses are also affected. But they are quickly aware of the restaurant's smoking policy. Those who dislike working in a smoking restaurant will shun the job or, perhaps, take the job and receive a premium. Economists since Adam Smith have generally found that, all other things equal, a job with distasteful or risky aspects pays more than one without. To take one of many examples, DeSimone and Schumacher (2004) find that a 10-percent increase in the AIDS rate raises the earnings of registered nurses by about 0.8 percent. The restaurant owner can trade off the gains from catering to smokers against the lost business from non-smokers and the pecuniary and search costs of being staffed.

Thus, there is a strong reason to believe that a law banning smoking would reduce the profits (and resale value) of restaurants that otherwise would have allowed smoking. Alamar and Glantz seem to recognize this reasoning when they write:

The tobacco industry, working through the hospitality industry, opposes these policies using the claim that smoke-free policies will harm the hospitality industry. In a world of perfect information and efficient markets operating with no externalities, this claim of harm to the industry would make economic sense, because any regulation that

³ I'm indebted to a referee for this point. He noted the parallel between this point and my point later about restaurants that ban smoking before there is a government mandate.

restricts an owner's choice set would at best have no effect on profitability. (520)

But immediately following this statement, the authors write:

In the real world of imperfect information, external effects on consumers and employees, or other forms of market failure, however, a restriction on the choice set could increase profitability. (520)

It is hard to conceive of external effects on consumers and employees that would vitiate my analysis above, which concludes that restrictions would hurt profitability. Alamar and Glantz assert that externalities alter the situation, but they never really identify what those externalities are. Towards the end of their article, they criticize an article by Dunham and Marlow (2000) that argues that the restaurant can act as an intermediary between smokers and non-smokers. Alamar and Glantz write:

[S]mokers and nonsmokers are not two well-defined groups but are rather numerous individuals with varied tolerances for smoke and willingness to refrain from smoking or to go outside to smoke. Even if the staff of the restaurant is ignored, the number of interested parties is very large with greatly varied preferences in regard to the externality. The large number of interested parties would cause negotiation costs to be high, which violates the assumption of low costs in the Coase theorem. (524)

The restaurant owner, however, no more need get huge numbers of people with varying smoking preferences together to make bargains than he needs to get people together to decide the menu, the lighting, the music, and the air conditioning. In normal discussions of negative externalities, it is costly for the sufferer not only to negotiate but also to exit. It is usually assumed that they are stuck in the "game," and the emphasis is on the cost of negotiation. But in the matter of going to a restaurant, the parties in question can easily decide not to be party at all. They can drive to a different restaurant or eat at home. All the restaurant owner need do is decide on a policy, announce it to the world, and then see what happens.

In using the word "externality," Alamar and Glantz beg the question. That is, they assume that smoking in a restaurant is an externality when that claim is exactly what they need to establish.⁴

⁴ Alamar and Glantz are not alone in this respect. In his otherwise excellent textbook, *Public Finance*, Harvey S. Rosen writes:

Another type of inefficiency that may arise due to the nonexistence of a market is an **externality**, a situation in which one person's behavior affects the welfare of another

Skeptics of free markets might argue that market forces do not work the way I claim. But, if so, their disagreement is almost as much with Alamar and Glantz as it is with me. If non-smoking consumers are not willing to pay more, all other things equal, to sit in a non-smoking restaurant and if non-smoking employees are not more willing to work (or not willing to work for less) at a non-smoking restaurant, whence comes the gain in profitability from banning smoking? Alamar and Glantz argue that some of the gain would come from lower labor costs. Smoking, they argue, “is linked to increases in days lost due to illness and higher worker compensation costs” (520). In other words, Alamar and Glantz are arguing that ending smoking in restaurants would reduce days lost to illness and reduce worker compensation costs. But if Alamar and Glantz are aware of this, why would employers not be? Again, they have not established an effect that can reasonably be called “external.” They have not shown smoking has to be different, in any relevant way, from dress code, music volume, lighting, or air conditioning, to name just a few general conditions at a restaurant.

Alamar and Glantz seem to realize that their own reasoning requires a market failure and that this assumed failure needs to be explained. After all, if restaurant owners do not know their own interests, then there must be a reason that restaurant owners make systematic errors. Alamar and Glantz even refer to a survey of restaurant and bar owners showing that they “were fearful of smoking restrictions” (525). The authors, however, do not place much weight on the beliefs of entrepreneurs regarding matters directly connected to their personal motivation and local knowledge “because it is well documented that the tobacco industry regularly feeds misinformation to the hospitality industry to fight smoke-free ordinances” (525; see also 520, 524). In other words, Alamar and Glantz claim that people in the hospitality industry are duped into not knowing where their true interests lie.

How could individual restaurant owners be so easily duped into believing something that, if acted on, hurts them? This seems implausible. Individual restaurant owners have business acumen. Moreover, surely the various restaurant owners’ associations would have an incentive to investigate whether going smokeless would lead to increased or decreased business. Alamar and Glantz do not

in a way that is outside existing markets. For example, suppose your roommate begins smoking large cigars, polluting the air and making you worse off. Why is this an efficiency problem? Your roommate consumes a scarce resource, clean air, when he smokes cigars. However there is no market for clean air that forces him to pay for it. (47, bold in original)

Yet elsewhere Rosen writes, “As long as someone owns a resource, its price reflects the value for alternative uses, and the resource is therefore used efficiently (at least in the absence of any other ‘market failures’).” If the room in which the roommate smokes is privately owned, then there would appear to be no externality. Similarly, because the air in a restaurant is owned by the restaurant owner, there is no externality. Something about smoking seems to have made both Rosen and Alamar and Glantz give up standard economic reasoning.

attempt to explain how alleged misinformation from the tobacco industry could sway hospitality entrepreneurs in a way that would directly hurt their livelihood. Their conclusion that restaurant owners are fooled is unpersuasive.

EMPIRICAL ISSUES

Alamar and Glantz use a data set on the sale of restaurants to examine whether non-smoking laws had an effect on the ratio of the sale price of the restaurant to the restaurant's annual gross revenues. They call it the price-to-sales ratio, P/S. They find that, all other things equal, having a restaurant in a smoke-free location caused the ratio to be higher. On this basis, they reach the following conclusion:

A restaurant in a smoke-free location sold for a higher price (thus the higher P/S ratio) than a restaurant with the same sales in a smoking location. This smoke-free premium indicates that the businesses in smoke-free locales operate at a higher margin (i.e., more profits).
(522)

But neither of the sentences above follows from their empirical findings. First, they found that P/S was higher in a smoke-free location, not that P was higher. Recall that P is the sale price of a restaurant and S is the restaurant's gross annual revenues. Imagine that a no-smoking ordinance reduces sales of a restaurant by 20 percent and reduces the price the restaurant sells for by 10 percent. Then P/S would rise, but that would not mean that the ordinance made restaurants more profitable. In fact, the ordinance would have made the restaurant less profitable. It is a simple mathematical fact that if all one knows is that a ratio rose, one cannot tell whether the numerator rose. And to know the effect of the ordinance on profitability, one must know whether the numerator, P, rose or fell.

There is a second problem in drawing Alamar's and Glantz's conclusion from their empirical finding. In laying out the reasoning in the previous paragraph, I wrote as if the authors had found that P/S rose after a non-smoking ordinance was passed. But they didn't find that. What they found is that, all other things equal, P/S was higher in areas with no-smoking ordinances. So even if P were higher in such areas, one would not know that P was higher due to the ordinance. To know that, one would have to do a before-and-after study to see the effect of a non-smoking ordinance—something the authors did not do.

Even if smoking bans do increase restaurant profitability—something that the authors did not demonstrate—this could be due to a reduction in competition. Just as preventing the import of textiles makes domestic textile producers more profitable than otherwise, so banning restaurants that allow smoking could make

the restaurants that survive more profitable. This possibility is one that Alamar and Glantz do not consider. But if this is what in fact occurred, it would not follow that restaurant owners as a whole should, in their own self-interest, advocate no-smoking laws. Pakistani producers of textiles are hurt by U.S. government restrictions on textile imports and would therefore not advocate such restrictions. So, also, if owners of restaurants that allow smoking are hurt by restrictions on smoking, they should not be expected to advocate such laws.

THE FORGOTTEN RESTAURANTS

Alamar and Glantz push the idea that anti-smoking ordinances are good for restaurant owners. They do a Monte Carlo simulation that tells how much better owners do (in terms of P/S ratio) in smoke-free places than in places without the restrictions. But the whole logic ignores that the restrictions eliminated certain restaurants from existence. In the restricted locations, those restaurants have dropped out of the sample, and their losses are ignored. The article abstract begins: “This study estimates the value added to a restaurant by a smoke-free policy ...,” as though the result tells what an average restaurant could expect in added value from the new government policy. But that is not actually what the investigation does.

THE FORGOTTEN CONSUMERS

Alamar and Glantz confine their investigation to ratio of restaurant price to sales. But Alamar and Glantz fail to acknowledge the consumers. Consumers who prefer smoking would lose.

HOW ABOUT VOLUNTARY ALTERNATIVES?

Again, Alamar’s and Glantz’s favored explanation for the economic conundrum is that restaurant owners are duped by the tobacco industry. In Alamar’s and Glantz’s view, it was in the restaurant owners’ interests all along to ban smoking, but they didn’t realize it. But if this were the true explanation, there is an obvious voluntary solution: enlighten the unenlightened. If Alamar and Glantz are correct, then restaurant owners, once informed, would voluntarily make their restaurants smoke-free and gain a competitive advantage, with both customers and employees, over their smoky rivals. To hold that force is needed, Alamar and Glantz must believe not only that restaurant owners are misguided, but also that they are incorrigibly so. And, if restaurant owners do not voluntarily act on Alamar and Glantz’s

findings, which should we doubt more: the prudence of the restaurant owners or, despite its being chosen as the Western Economic Association International's "Best Article of 2004", the wisdom of their article?

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Smoking in Restaurants: A Reply to David Henderson

BENJAMIN C. ALAMAR¹ AND STANTON A. GLANTZ²

ABSTRACT

“SMOKING IN RESTAURANTS: WHO BEST TO SET THE HOUSE RULES?” BY David Henderson (Henderson 2007) is a comment on our paper “Smokefree Laws Increase Restaurant Values” (Alamar and Glantz 2004) published in *Contemporary Economic Policy*. Henderson asserts that both our economic theory and interpretation of our statistics are wrong. In asserting that our theory is not correct, Henderson asserts that “In restaurants, smokers do not impose negative externalities on non-smokers because restaurant owner’s have well defined property rights that cause them to internalize the costs and benefits of smoking.” This assertion cannot be true. It is not possible for a restaurant owner to internalize the cost of second-hand smoke on the health of the staff or the patrons. There is no mechanism by which a restaurant owner can compensate a patron for any health costs related to second-hand smoke, therefore it is not possible for the owner to have completely internalized the costs of the externality imposed by the smoker. This fact is one reason that the public has demanded laws to make restaurants smokefree.

Henderson points to another paper by Dunham and Marlow (2000), which was supported by Philip Morris Management Corp., as being a more correct analysis of the situation regarding the effects of smokefree laws on restaurant profitability. In contrast to our paper, which was based on hard data from actual experience, Dunham and Marlow based their analysis on a survey of restaurant and bar owners about what the effects of a smokefree law might be. In particular, the owners that were polled did not operate businesses in areas that are smokefree. Henderson (2007) claims that we do not put enough faith in these entrepreneurs’ views while discounting the likelihood that their opinions on smokefree laws could have been unduly influenced by years of tobacco industry propaganda

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claiming that smoking restrictions hurt the hospitality business (Dearlove et al 2002). Unfortunately, the only information these entrepreneurs ever saw until recently were data put out by the tobacco industry. When these entrepreneurs only have the biased information given to them from the tobacco industry (without being told that it is coming from the tobacco industry (Alamar and Glantz 2004)) how are they to know that the information is biased?

Finally, Henderson asserts that the interpretation of our statistical analysis is not correct. Our analysis used a regression in which the dependent variable was the Price to Sales ratio (P/S), where price is the price at which a restaurant is sold and sales is the annual revenue of the restaurant. We found a positive effect on this ratio when smokefree laws were implemented which we argue implies an increase in the price of the restaurant. Henderson notes that because we use a ratio, the positive effect could just as easily come from a reduction in sales as an increase in price. While this is mathematically correct, Henderson ignores all of the previous literature on the topic. In a review of all the current literature at the time the our study was done, Scollo et al (2003) reviewed 97 studies on the economic effects of smokefree laws. Of those 97, 21 were found to 1) use objective data, 2) include all data points both pre and post implementation of a smokefree law, 3) controlled for secular trends and randomization and 4) controlled for overall economic trend (Scollo et al 2003). None of these 21 studies found a decline in revenue from the implementation of a smokefree law and in fact, the only studies that have ever found a negative impact on revenues from the implementation of a smokefree law are those studies funded by the tobacco industry. Our interpretation of the positive effect on the P/S ratio from the smokefree law is based upon this previous literature that finds no decline in revenue. If revenue does not decline (Scollo et al 2003) and the P/S ratio increases, then the price itself must increase.

Henderson (2007) does not accurately identify any problems either theoretically or statistically with our analysis. His criticism of our work is based on extreme interpretations of economic theory and an incomplete knowledge of the previous literature on the subject. The bottom line (Alamar and Glantz 2004) is that the implementation of a smokefree law to protect the public from the dangers of second-hand smoke increased restaurant values by a median of 16%. This result is consistent with all other literature on the subject that has not been funded by the tobacco industry.

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Professor Stanton A. Glantz has been a leading researcher and activist in the nonsmokers' rights movement since 1978, when he helped lead a state initiative campaign to enact a nonsmokers' rights law by popular vote (defeated by the tobacco industry). In 1983, he helped the successful defense of the San Francisco Workplace Smoking Ordinance against a tobacco industry attempt to repeal it by referendum. He is one of the founders of Americans for Nonsmokers' Rights. In 1982, he resurrected the film "Death in the West," suppressed by Philip Morris, and developed an accompanying curriculum that has been used by an estimated 1,000,000 students. He helped write and produce the films "Secondhand Smoke" and "120,000 Lives." Dr. Glantz conducts research on a wide range of issues ranging from the effects of secondhand smoke through the reductions in heart attacks observed when smokefree policies are enacted, to how the tobacco industry fights tobacco control programs. His work in this area was identified as one of the "top research advances for 2005" by the American Heart Association. He has written several books, including the widely used *Primer of Biostatistics*, and *Primer of Applied Regression and Analysis of Variance*. He is author of more than 200 scientific papers. His book *The Cigarette Papers* played a key role in the ongoing litigation surrounding the tobacco industry.

His book *Tobacco Wars: Inside the California Battles* chronicles the battles against the tobacco industry in California. He also wrote *Tobacco: Biology and Politics* for high school students and *The Uninvited Guest*, a story about secondhand smoke, for second graders. He is now running two educational projects, SmokeFreeMovies.ucsf.edu, which is working to end use of movies to promote tobacco, and TobaccoScam.ucsf.edu, which is countering tobacco industry efforts to co-opt the hospitality industry. Working with the UCSF Library, he has taken the lead in making nearly 50 million pages of previously secret tobacco industry documents available to the entire world via the internet. He served for 10 years as an Associate Editor of the *Journal of the American College of Cardiology* and is a member of the California State Scientific Review Panel on Toxic Air Contaminants. He was elected to the Institute of Medicine in 2005. He is a Professor of Medicine (Cardiology) and American Legacy Foundation Distinguished Professor of Tobacco Control as well as Director of the Center for Tobacco Control Research and Education at University of California, San Francisco. His email is glantz@medicine.ucsf.edu.

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Deposit Insurance and Moral Hazard: Capital, Risk, Malfeasance, and Mismanagement

GARY RICHARDSON¹

A COMMENT ON: LINDA HOOKS AND KENNETH ROBINSON, “DEPOSIT INSURANCE AND MORAL HAZARD: EVIDENCE FROM TEXAS BANKING DURING THE 1920s,” *THE JOURNAL OF ECONOMIC HISTORY*, 62(3) (SEPTEMBER 2002).

ABSTRACT

A *JOURNAL OF ECONOMIC HISTORY* ARTICLE BY LINDA HOOKS AND KENNETH Robinson, “Deposit Insurance and Moral Hazard: Evidence from Texas Banking During the 1920s,” contains a contradiction (Hooks and Robinson 2002). Pondering the contradiction in the paper reveals insights that the authors may have overlooked.

Hooks and Robinson’s article examines the experience of the banking industry in Texas during the 1920s. Texas operated a deposit-insurance system from January 1, 1910 until February 11, 1927. Deposit insurance was mandatory for all state banks, which were given the choice of two plans in which to participate. The preponderance participated in the depositors guarantee fund, to which they contributed a flat-rate premium and from which their depositors received reimbursement in the event of a failure. A small percentage of banks participated in the depositors’ bond security system, which required them to file annually with the state a bond equal to the amount of its capital. Nationally chartered banks in Texas did not participate in these deposit insurance systems, and therefore, serve as a control group from which conclusions can be drawn. Hooks and Robinson analyze a panel of data on a sample of state and national banks in Texas over the period 1919 to 1926.

THE CONTRADICTION

Hooks and Robinson examine balance sheet data for banks in Texas. Their Table 3 focuses on state banks during the 1920s and finds “evidence that declines

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in capitalization led to increases in asset risk, as measured by loan concentrations. Such activity on the part of insured banks would indicate that banks with weakened financial conditions increased the riskiness of their asset portfolios, which would be consistent with a moral-hazard effect at work” (848). They define ‘loan concentration’ as the ratio of loans to total assets. This variable indicates the share of the portfolio invested in loans, which were risky assets with high returns, relative to the share of the portfolio held as cash reserves, deposited in correspondent banks (including the Federal Reserve), or invested in securities. The authors lack information about the types of loans made by the banks or about diversification within loan portfolios.

Their Table 4 examines two control groups: state banks in 1909, and national banks during the 1920s. Neither control group exhibits correlations between capitalization and loan concentration, suggesting that a difference between the control and treatment groups, such as deposit insurance, encouraged poorly capitalized state banks to invest in loans to local farmers and businessmen and other assets with higher risk.

According to the regressions in their Table 2, however, such behavior did not increase the likelihood that banks failed. Banks whose portfolios contained a higher proportion of loans failed at lower rates than other banks, all else equal (see coefficient on the portfolio concentration variable, which measures the ratio of loans to assets, LOANASS). The beneficial effect of additional loans was large. An extra \$1 of loans reduced the probability of failure to the same extent as an additional 43 cents of retained earnings (-0.140/-0.327) (see Table 2, column 1). The authors acknowledge this result when they write “we expect a positive sign on LOANASS ... However, LOANASS is negative and significant, the opposite of what was hypothesized.” A positive coefficient would indicate that banks with loan-heavy portfolios failed at higher rates than other banks. The negative coefficient that they find, however, indicates that banks with loan-heavy portfolios failed, on average, at rates lower than other institutions.

Their un-realized hypothesis rests on the notion that “declines in capitalization can induce ‘go-for-broke’ strategies among insured banks. With less of their own funds at risk as capital declines, insured banks may be tempted to gamble on risky projects with a small probability of payoff. If these ventures prove successful, the bank gains, otherwise the insurance fund suffers the losses” (844). Their expectation of a “positive sign on LOANASS” presumes that investing in loans rather than safer assets, such as government securities and eligible commercial paper, resulted in large losses and high failure rates. Table 2’s regressions indicate the opposite. Texas banks benefited from lending more money. Their ex ante gambles yielded ex post profits. Historically, this is understandable, since the banks made these loans during the era known as the Roaring 20s, when Texas enjoyed booms in industry and oil, and the state’s economy flourished.

The regressions in Table 2 contain another variable, INSURED, an indica-

tor for state banks, all of which had to participate in one of the two Texas deposit insurance systems. The author's argue that "if moral hazard incentives are present at insured banks, leading them to pursue excessively risky activities, then we would expect a positive sign on INSURED" (843). The sign is indeed "positive and significant at the 10-percent level, indicating that deposit insurance significantly increased the likelihood of failure" (843).

This brings us to the contradiction in the article. The way the authors interpret LOANASS and INSURED in Table 2 is inconsistent with the way they interpret those variables in Tables 3 and 4. For the latter tables, the authors claim that the structure of a bank's assets, measured by loan concentrations in the portfolio, serves as a good proxy for risk. "Loans are one of the riskiest assets that banks can hold" (842). "A higher proportion of loans leaves a bank more exposed to credit risk and more vulnerable to adverse economic shock" (843). The statistically significant relationship between LOANASS, TOTCAP, and CAPLOAN, the authors assert, reveals moral hazard at work.

Yet for Table 2, the authors' argue that the coefficient on INSURED indicates that moral hazard induced excess risk taking and influenced the survival of banks. But Table 2's regressions also contain the variable LOANASS. If LOANASS serves as a good proxy for ex ante portfolio risk, as the authors argue throughout their essay and in Tables 3 and 4, then the variable INSURED cannot also do so. Regression analysis imposes a *ceteris paribus* assumption. This assumption means that the coefficient on INSURED measures the correlation between insurance and failure that is orthogonal to LOANASS, that is holding the loan-to-asset ratio (the measure of risk taking) constant.

In other words, if the coefficient on INSURED reveals moral hazard at work, it must reveal moral hazard operating through a channel other than distorting banks decisions concerning the bearing of risk. What might that channel be?

RESOLVING THE CONTRADICTION

The literature on deposit insurance discusses many manifestations of moral hazard. Moral hazard arises when the presence of insurance reduces the incentives for depositors to monitor the safety of their savings. Depositors stop monitoring the behavior of bankers, and no longer bother to move deposits from worse to better institutions.

One manifestation of moral hazard, for which Hooks and Robinson search, involves excess risk-taking by insured banks. The relationship is one between managers and regulators, who indirectly underwrite gambles gone bad. The Savings and Loan crisis inspired this line of research.

Another manifestation of moral hazard arises when bank managers engage in actions that serve themselves at the expense of their bank. For example, managers may fail to exert efforts to collect bad debts or may show leniency toward

friends. Managers may cut costs and corners, initiating what may become a race to the bottom, as self-serving actions become endemic. Bank regulators usually refer to the illegal aspects of this phenomenon as malfeasance or defalcation, which is defined as a monetary deficiency through breach of trust by one who has the management or charge of funds, or in other words, a fraudulent deficiency in monetary matters. Bank regulators refer to behavior which does not rise to the level of criminality, but which harms the health of banks, as mismanagement.

Malfeasance and mismanagement of this type may have been present particularly among the small state banks operating in Texas between the world wars. The owners of these banks were typically the managers, and in some instances, also the principal borrowers. These banks did business in a small community, where the owner/managers socialized with businessmen and merchants who were their biggest borrowers, and also often significant depositors. These intimate relationships might have made it difficult for the owner/managers to vigorously collect delinquent debts. In such circumstances, the deposit-insurance system served as a means of subsidizing an entire community. When adverse shocks or inappropriate behavior prevented borrowers from repaying obligations, management avoided making hard choices, and instead, ceased operations, forcing the deposit-insurance authority to cover the bank's liabilities and the community's losses.

Malfeasance and mismanagement of this type does not generate predictable patterns in bank balance sheets. Some classic examples of mismanagement illustrate this point. First, a bank never bothers to make loans. Instead, it invests in commercial paper, deposits funds in correspondent banks, and keeps the remainder of its resources in its vault. Its costs exceed revenues, and it goes bankrupt, because its portfolio has too low of a return and too little risk. Second, a bank invests in a large, expensive building with beautiful new furniture and artwork, which it cannot afford on current revenues, and goes bankrupt. Third, bank managers do not perform due diligence and fail to collect outstanding debts. Losses rise. Bankruptcy results. Fourth, bank managers extend loans to friend's and neighbors, rather than to the project with the highest potential return or to the investors willing to pay the highest (risk-adjusted) interest rate.

In the first and second examples, the balance sheets of banks afflicted by moral hazard would exhibit low ratios of loans to assets. This pattern is the opposite of what, according to Hooks and Robinson, would be exhibited by banks bedeviled by moral hazard. It may explain the sign on the variable LOANASS in their Table 2.

In the third and fourth examples, the balance sheet of a bank could exhibit any ratio of loans to assets. The variable LOANASS would not reveal moral hazard of this type. Lack of correlation with LOANASS makes this type of moral hazard a potential explanation for the positive coefficient on INSURED in Table 2, because the logic of regression analysis dictates that the factor generating the coefficient must be orthogonal to LOANASS and to the underlying phenomena,

portfolio concentration and asset risk, for which LOANASS proxies.

Recently discovered data on the causes of bank suspensions during the 1920s supports this supposition. The data come from the archives of the Federal Reserve Board of Governors. The data indicate the cause of failure for each bank that ceased operations from 1921 through 1930. From 1921 through 1926, when deposit insurance existed in Texas, mismanagement or malfeasance forced an annual average of 3.2% of all state banks to suspend operations. That fraction amounted to roughly eight out of ten state bank suspensions that occurred during the interval. From 1927 through 1930, the four years following the cessation of deposit insurance, mismanagement and malfeasance forced an annual average of only 1.2% of all state banks to suspend operations. That fraction amounted to only two out of ten state banks that suspended operations (see Chung and Richardson 2006a and 2006b and Richardson 2006 for a description of the new data).

A THEORETICAL BASIS FOR THIS OBSERVATION

Leibenstein's (1966) concept of "X-Efficiency" provides a theoretical basis for this argument. Leibenstein distinguishes allocative efficiency (and inefficiency), which occurs when firms properly (or improperly) respond to price signals, to X-efficiency (and inefficiency), which occurs when firms minimize costs of producing an allocation (or fail to do so). The failure to minimize costs occurs due to difficulties aligning the incentives of employees and the firms for which they work. This principal-agent problem leads firms to operate inside their production possibilities frontier.

Deposit insurance is the type of regulation that distorts the incentives of managers, leading them to pursue their own interests – such as high salaries, large bonuses, plush offices, corporate contributions to high-profile charities, and leniency towards borrowers who are their family and friends – at the expense of the public and their firm. In other words, deposit insurance may distort incentives WITHIN banks, as well as distorting incentives OF banks.

Like the large literature on the impact of deposit insurance, Hooks and Robinson focus on the latter issue, how deposit insurance distorts the risk taking decisions of banks. On that issue, their piece makes a useful contribution, but, I believe, their statistical analysis also illuminates the former issue, how deposit insurance distorts incentives of managers, and thereby, encourages mismanagement and malfeasance within banks. In this respect, Hooks and Robinson's piece follows in the footsteps of Leibenstein (1966). Leibenstein's seminal study shows that in a wide array of industries, legal limits on competition among firms (such as monopolies and price controls) reduce economic efficiency both by distorting the decisions of firms (i.e. allocative efficiency) and by diminishing management's incentives to minimize costs (X-inefficiency). His estimates indicate that welfare

losses due to allocative inefficiency were a small fraction of the welfare losses due to X-inefficiency. Hooks and Robinson's regressions lead to a similar quantitative conclusion.

ADDITIONAL POSSIBILITIES

Moral hazard can afflict financial institutions in many ways. The afflictions described here and by Hooks and Robinson do not exhaust the possibilities. For example, insurance also distorts incentives pertaining to bank capitalization. The distortion arises because depositors do not insist that insured banks retain large reserves. Depositors would rather have banks invest the bulk of their assets and pay large dividends. This enables insured banks to operate with less capital and lower reserve ratios than uninsured counterparts.

The data set constructed by Hooks and Robinson may not be able to reveal the quantitative significance of this channel. State chartered banks differed from nationally chartered banks along many dimensions including: capital requirements, reserve requirements, examination frequency and thoroughness, services offered, investments permitted, and access to liquidity. The coefficient on the single-indicator variable, INSURED, confounds all of these effects.

In my opinion, de-capitalization did occur during the era of deposit insurance in Texas during the 1920s. At that time, the number of state-chartered banks rose. The number of newly opened institutions exceeded the number of failing banks, as mismanaged banks left the industry and new institutions arose to replace them. Regulations allowed state chartered banks to operate with less paid-up capital and lower financial reserves than nationally-chartered institutions. These facts are consistent with the hypothesis that insurance enabled banks to operate with lower levels of capital.

DISCUSSION

Hooks and Robinson indicate that their article provides evidence supporting the conventional academic wisdom about deposit insurance, moral hazard, and risk taking. I say their essay does that and more. Their regressions demonstrate that while the prevailing paradigm explains a portion of the events that occurred during the 1920s, some other phenomena, hitherto overlooked, must also be at work. Evidence from the archives of the Board of Governors suggest that this phenomenon is mismanagement and malfeasance, which increases when insurance reduces depositors' incentives to monitor and react to the safety and soundness of banks. Economic logic suggests that de-capitalization may also have occurred. Other manifestations of moral hazard may have influenced the behavior of banks in Texas. Further research is required to determine their rela-

tive importance.

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Quantifying Moral Hazard: A Reply to Gary Richardson

LINDA M. HOOKS¹ AND KENNETH J. ROBINSON²

ABSTRACT

In his comment on our 2002 *Journal of Economic History* paper, Gary Richardson (2007) proposes that our work specifies moral hazard too narrowly. Richardson posits that fixed-rate deposit insurance leads to moral hazard which takes many forms. These include not only the usual notion of risk-taking in the asset portfolio, but also mismanagement, malfeasance, and reduced incentives for depositor monitoring. Richardson notes that one measure of moral hazard in our empirical evidence, the loan-to-asset ratio, quantifies risk-taking in the asset portfolio only.

Richardson identifies what he considers an inconsistency in our empirical results. In a model of bank failures, the coefficient on the loan-to-asset ratio is negative and statistically significant, which is not the prediction of moral hazard theory. However, the coefficient on the dummy variable for participation in the deposit-insurance program is positive. Richardson argues that this implies that risk-taking in the asset portfolio was not the only mechanism through which moral hazard worked. Instead, moral hazard also occurred through other routes, with our dummy variable capturing the additional types of risk associated with moral hazard.

Richardson also presents evidence that these other types of moral hazard were economically significant. Using data from the Federal Reserve Board of Governors, he finds that the rate of state bank failures attributable to mismanagement and malfeasance was higher during some of the years that Texas had deposit insurance than during the several years after the insurance program ended.

We agree that moral hazard among our sample of Texas banks likely took many forms. For the most part, we do not have the detailed data necessary to identify other routes of moral hazard. However, Table 1 provides some additional information on bank failures from records provided by the Texas Department of

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The views expressed are those of the authors and should not necessarily be attributed to the Federal Reserve Bank of Dallas or the Federal Reserve System.

Banking. The Department of Banking kept a log of bank failures that noted the bank name, date of closure, and a brief explanation. It shows that 42 percent of bank failures in the six years of our study were classified as involuntary.

For comparison, in the following six years after the demise of deposit insurance in Texas, involuntary failures fell to 35.6 percent of all failures. This evidence is consistent with Richardson's claim that mismanagement occurred more frequently during the years of deposit insurance than during the years without it. The drop in involuntary failures occurred primarily in the category identified in the log as "liquidated by the Department."

Table 1: Failures by Closure Type

Closure Type	1919-Feb 1927		March 1927-1934	
	Number	Percent	Number	Percent
Involuntary	197	42.0%	171	35.6%
Involuntary classifications:				
Liquidated, depositors paid in full	4	0.9%	2	0.4%
Merged	2	0.4%	0	0.0%
Reorganized	68	14.5%	69	14.4%
Assumed	0	0.0%	8	1.7%
Liquidated by Department	117	24.9%	81	16.9%
Sold	3	0.6%	8	1.7%
Voluntary	272	58.0%	309	64.4%
Total	469	100%	480	100%

Source: Charter Registry, undated, unpublished manuscript, Texas Department of Banking, Austin, Texas.

Richardson also thinks it likely that de-capitalization was a problem for state banks during the deposit-insurance program. This view, however, is not supported by our sample. National banks, which did not participate in the deposit insurance program, held less capital relative to assets, on average, than did state banks, as we showed in Table 1 of our paper. State banks recorded an average capital-to-asset ratio of 23 percent compared to national banks' average of 18.8 percent during the operation of the deposit insurance program. Although we did not report the fact in our original Table 1, this difference is statistically significant at the one-percent level ($t = 6.75$).

The sample data also show that state banks did not exhibit a steady decline in capital. Table 2 below shows that the mean capital-to-asset ratio for our sample of state banks varied without a clear pattern in the years of deposit insurance. We have sample data on these banks for only one year after the deposit insurance program ended, but the mean for that year is not unlike that for prior years.

Table 2: Capital at State Banks

Year	Mean capital-to-asset ratio
1919	20.8%
1920	18.4%
1921	26.1%
1922	24.9%
1923	22.4%
1924	19.3%
1925	20.2%
1926	22.1%
1927	23.8%
1928	18.8%

Source: Examiners Report of Condition, 1919-1928, unpublished confidential files, Department of Examination, archives of the Federal Reserve Bank of Dallas, Dallas, Texas.

Richardson's insights into other forms of moral hazard at work are difficult to quantify empirically. However, examiners' comments in the examination reports we used in our paper offer some support for his general claim. Each report concludes with a section titled "Management and General Condition." In it, examiners respond to the following questions: "Is the management safe?" "Competent?" Looking at a sample of 35 reports concerning banks that failed in the subsequent reporting period, we find that 18 of the banks were judged unsafe and incompetently managed. (Additional examiners' comments are reproduced in Table 3 below.)

A fixed-rate deposit insurance program of the type that existed in Texas and elsewhere can encourage excessive risk taking. This moral hazard effect can take a number of different forms. Our results indicate that insured banks' asset portfolios seemed to be affected by risk-taking incentives. Richardson posits that other forms of risk-taking activities were present. We agree with his hypothesis and offer some evidence in support of additional avenues for moral hazard to have played a role in the activities of Texas banks in the 1920s.

TABLE 3: COMMENTS FROM EXAMINERS AT TIME OF VARIOUS BANK FAILURES

<p>The Directors admit that the affairs of the bank are in unsatisfactory condition. Director (X) is a brother-in-law of Cashier (Y). President (Z) is the father of Cashier (Y) and he readily admits that he is getting old and leaves everything up to his son.</p>
<p>It is plain that there is a considerable amount of ill feeling existing between Cashier and the two assistant cashiers. The assistant cashiers stated to the State Examiner and me that they believed he was not absolutely honest.</p>
<p>This is the reason why the management of this bank cannot collect loans due this bank by other bankers, their customers, their kinsmen and their interests. This bank needs a banker to manage it who has no intimate friends or relatives in this section.</p>
<p>While listing the ledgers, (examiner) noticed that once or twice Cashier attempted to remove a couple of ledger sheets. (Examiner) noticed that the control account had been erased, the amount being increased \$22 million, and that the account with X bank showed to have been tampered with.</p>
<p>It seems to me this is one of, if not, the poorest managed banks it has been my duty to examine. Not only is the management incapable, but I am convinced that Active Vice President X is dishonest.</p>
<p>The Directors are more or less figure-heads who do not attempt to actually direct the bank's affairs. The clerical work is not kept up in a satisfactory manner and the general appearance of the bank room is untidy.</p>

Source: Examiners Report of Condition, 1919-1928, unpublished confidential files, Department of Examination, archives of the Federal Reserve Bank of Dallas, Dallas, Texas.

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The EITC Disincentive: The Effects on Hours Worked from the Phase-out of the Earned Income Tax Credit

PAUL TRAMPE¹

ABSTRACT

The biggest expansion to date of the Earned Income Tax Credit, legislated in 1993, was taking effect just as the welfare reform of 1996 was removing the federal entitlement to AFDC benefits and devolving the program to the states, resulting in a large drop in caseloads and spending on the program. As a result, the EITC has become the largest US assistance program to the poor in terms of dollars and in terms of number of recipients, surpassing AFDC (now Temporary Assistance to Needy Families or TANF) (Eissa and Hoynes 2005, 1). Long neglected in academic literature, the program has been receiving a great deal of attention recently, particularly its incentives and disincentives to work. Researchers have focused on the incentives of phase-in of the program, however, and have been less precise about the phase-out.

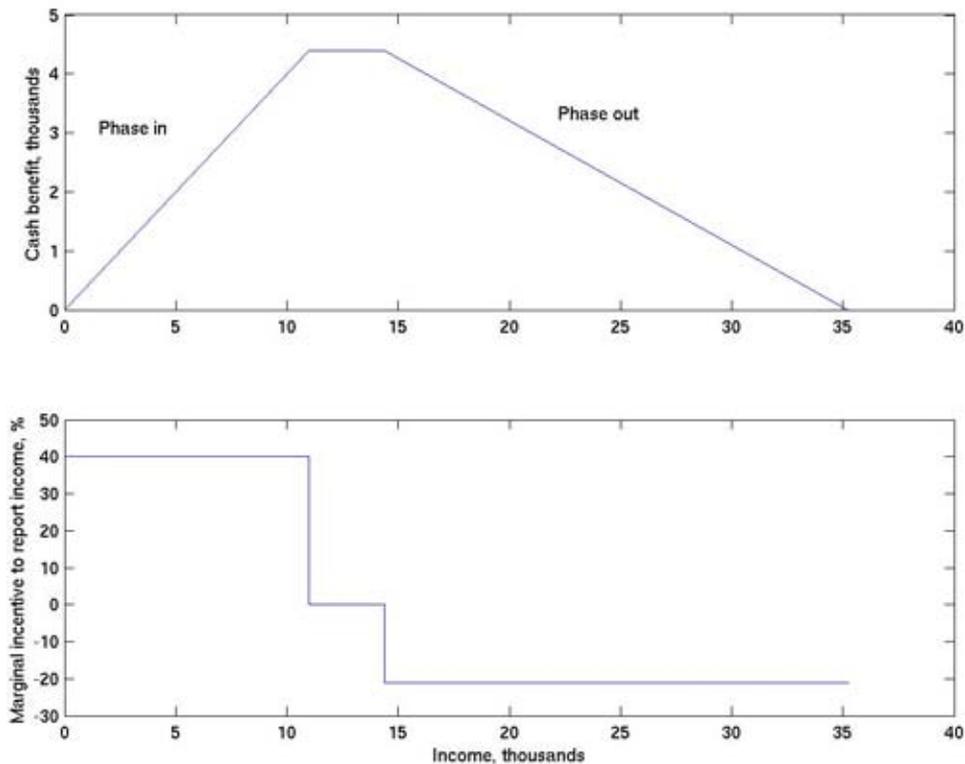
The EITC phase-in range, where benefits increase as income increases, would tend to encourage work, while the phase-out range, where benefits are withdrawn above an income threshold, would discourage work. This paper focuses on the phase-out. Most studies have not found solid evidence of discouraged work, but some of those papers have focused on the wrong population and two seem to have overlooked facets of their own data. The most thorough study of the phase-out, which did not find a discouragement effect from the increase in the EITC contained in the 1986 tax reform bill, has not been repeated for the much larger expansion legislated in 1993. After reviewing the literature on the issue I show a regression which does suggest an effect on hours worked for those in the EITC phase-out range from the 1993 expansion of the program, albeit a small effect.

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THE EITC IN BRIEF

The EITC was created in 1976 as part of an economic stimulus package. The idea grew out of theories from Milton Friedman and others that a negative income tax, such that poor families receive payments from the government which increase as their income rises, would encourage participation in the workforce by the poor. The idea was to reverse the perceived disincentive of traditional welfare programs which pay the most to those with zero income – i.e. those who do not work at all (Green 1968, 28). At first the program was quite small but it was expanded in 1984, 1986, 1988 and especially 1993, at which time the benefits were also indexed to inflation so in nominal terms it has continued to grow.

Figure 1: The EITC for Single Parent - Two Child Families



As of 2007, the program pays a benefit equal to 40 percent of income for the first \$11,000 for families with two children (\$4400 maximum benefit) and 34 percent of nearly \$7900 income for families with one child (\$2662 maximum benefit). In 2001 legislation intended to reduce the inherent marriage penalty in EITC moved the beginning point of the phase-out up \$2000 for two-parent families. Therefore, the phase-out now begins around \$14,400 for single parent families and \$16,400 for two parents.

Above that threshold, families with two or more children lose 21 cents of benefits for each additional dollar of income (see Figure 1). One-child families lose benefits above the threshold at a rate of 16 cents per dollar of income. This means that a single parent family with two or more children receives some benefits until reaching \$35,263 in income while a single parent with one child receives some benefit until reaching \$31,030 income. Two parent families' benefits are reduced at the same rates, so the benefits end at an income level \$2000 above their single parent counterparts.

It is a "refundable" tax credit meaning it is paid through the regular income tax system but if those eligible do not have any tax liability against which to take the credit (and most eligible do not) they receive cash from the government. As a kind of reverse withholding, it can be paid into the person's regular paycheck, but most choose to receive it in one lump sum when filing an IRS form 1040 each year.

PREVIOUS STUDIES: LABOR FORCE EXPANSION

The phase-in percentage being larger than the phase-out percentage means that the incentive is larger than the disincentive, but it also means that the disincentive covers a larger band of income and therefore affects more people. In fact, 65 percent of those receiving benefits have incomes in the phase-out range (Liebman 1998, 104). It is logical to ask which effect is greater. Does the program produce a net gain in work or a net loss?

The literature appears to be unanimous in reporting that each expansion in the Earned Income Tax Credit has brought about an increase in labor force participation by single mothers with children. The bulk of the AFDC and EITC population is made up of such families, so scholars have studied that group to measure the effects. According to Nada Eissa and Jeffrey Liebman, labor force participation by single women with children, controlled for participation rates of single women without children who were not eligible for the EITC at that time, rose 2.4 percent after the EITC expansion contained in the tax overhaul of 1986 (Eissa and Liebman 1996, 616). An earlier paper by Eissa and Liebman bunched the three years before the 1986 reform and the three years after and found a 1.4 percent increase in labor force participation by single women with children (Eissa and Liebman 1995, 2). In a separate paper written by Liebman, he found that the participation rate of women with children increased from 72.7 percent in 1984 to 82.1 percent in 1996 with three major expansions of EITC in between (Liebman 1998, 97). In a 2005 paper which Eissa wrote with Hilary Hoynes they find that labor force participation by women with children, defined as any work over a 12 month period, shows a jump from 73 percent in 1984 to 85 percent in 2003 (Eissa and Hoynes 2005, 12). They report that most of the change occurred between 1992 and 1999 as employment jumped 16 percent. Eissa and Hoynes state that there was not much change in labor force participation of single women without children during that period (Eissa and Hoynes 2005, 12).

Eissa and Hoynes and other authors treating the changes of the mid 1990s do not mention one caveat, namely, that over this same time period, AFDC was being reformed with the main effect being to push recipients into jobs. Even before the 1996 welfare reform at the federal level, the states had been using the waiver process to introduce all of the same reforms that would later be enacted nationally. Therefore since the EITC expansion and the welfare reform were happening virtually simultaneously, separating the effects is very difficult. However, Robert Greenstein reports that survey evidence suggests that more than half of the increase in labor force participation is connected to the EITC expansion (Greenstein 2005, 3).

PREVIOUS STUDIES: HOURS WORKED

While there have been many studies detailing the positive effects on workforce participation of the phase-in of benefits, they have been much less systematic in studying the negative effects of the phase-out. Several studies address the negative effects by looking at the average number of hours worked by all single women with children regardless of income. The rationale for doing so is, as Eissa and Hoynes put it, “that about three quarters of single EITC recipients have earnings in the flat and phase-out regions of the credit – thus the expectation is that the EITC will reduce the number of hours worked by most eligible single taxpayers already in the workforce” (Eissa and Hoynes 2005, 8). However, those in the flat range do not face a disincentive and the studies, including the one quoted above, use families from all income levels, those in the flat and phase-out ranges as well as the phase-in range and those outside the EITC program altogether.

Of course it is also possible to observe what appears to be a disincentive throughout the income scale due to the simple fact that the subjective marginal benefit of each additional dollar of income declines as income rises. However, the disincentive directly attributable to the EITC does not affect all single women with children. The phase out of EITC benefits is only going to affect the level of work for those who are actually in the income range to which those marginal tax rates apply. In fact, for those in the phase-in range, an expansion of the program would be expected to cause an increase in hours worked due to an increase in the subsidy for each hour worked.

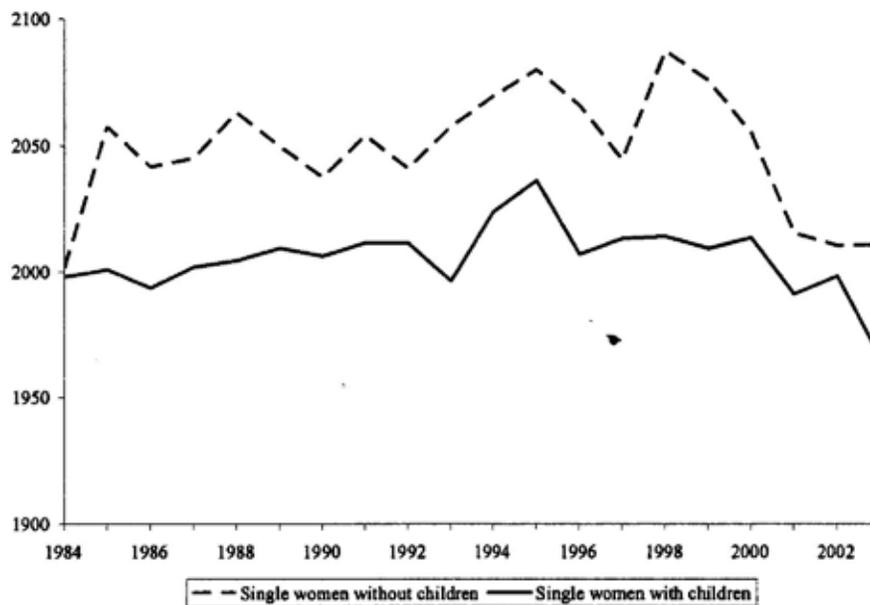
Most papers on the subject do address the point that the disincentive only exists for those in the phase-out range but seem to regard it as a minor nuance in the data, when in fact it makes most of the studies doubtful for measuring the effects of that disincentive. Eissa and Liebman (1996), which I discuss below, was the one major study which focused exclusively on those in the phase-out income range. It found no effect and that seems to have settled the question for other researchers. Researchers have continued to conduct studies involving single women throughout the income range, but, since Eissa and Liebman (1996), no one has isolated the families actually affected by the phase-out range.

The literature is almost as unanimous in the claim that EITC has no disincentive effect on hours worked as it is on the claim that the phase-in encourages women to enter the workforce. One exception is a study by John Karl Scholz, who projected that the 1993 expansion would reduce hours worked by 21 hours per year for each family in the phase-out range (Scholz 1996, 159). He was making the prediction from his model of the labor market before the data was in, although the prediction was based on past responses by the labor market to changes in marginal tax rates.

Jesse Rothstein (2007) reports that he could not find a difference in hours worked among women with children compared to women without. That study used the entire income spectrum (except zero, i.e. those who did not work at all), presumably including the phase-in range and those altogether beyond the EITC range. Bruce Meyer and Dan Rosenbaum (2001) found mixed results on hours worked for women with children – also using the entire income spectrum (Meyer and Rosenbaum 2001). Eissa and Liebman’s 1995 paper also found no decrease in hours worked by single women with children after the expansions of the 1980s (Eissa and Liebman 1995, 28-29). Again, none of these studies focused on the phase-out range.

Three years later, in a single author paper, Liebman argues that studying the effects on hours for expansions other than the one legislated in 1986 is difficult because the changes in the program were phased in gradually (Liebman 1998, 100). After discussing the lack of empirical evidence for a decrease in hours among women with children, throughout all income strata, presumably, he then dismisses the idea that the

Figure 2: Average Annual Hours Worked for Single Women in the Phase-Out Region of the EITC, 1984-2003 By Presence of Children

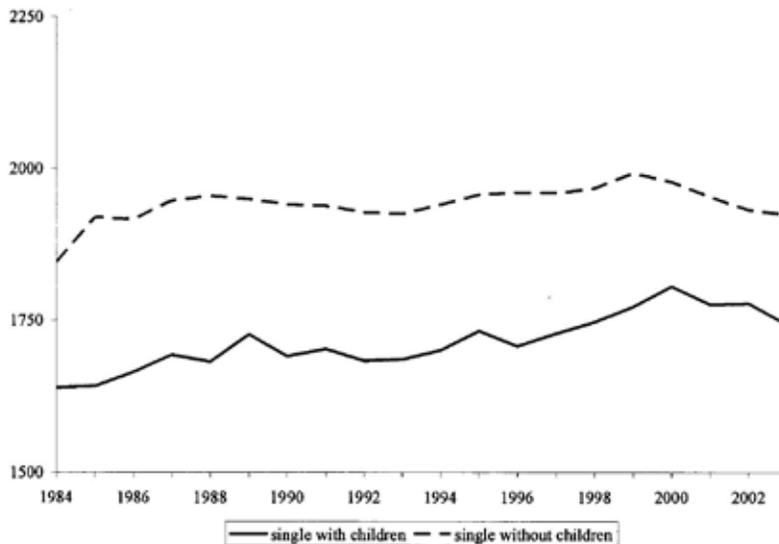


Reprinted from Eissa and Hoynes 2001, 31.

study might yield different results if it focused solely on those with incomes in the phase-out range by referring back to his and Eissa's 1996 paper (Liebman 1998, 104). More recently Eissa and Hoynes find "no evidence that hours of work decreased for single mothers relative to single mothers without children as the EITC expanded" (Eissa and Hoynes 2005, 14). Although they do not focus on those in the phase-out range, they do include a figure, (reproduced here as Figure 2) specific to that group.

The authors' conclusion from this figure is as follows: "Strikingly, this figure shows no pattern of a reduction in hours worked for single women with children relative to single women without children" (Eissa and Hoynes 2005, 14). However, they do not comment on the dramatic increase in hours worked by single women without children starting in 1984 which was not accompanied by a similar increase for those with children. They also do not comment on the dramatic rise and subsequent fall in hours for those without children between 1997 and 2000 while the hours of those with children stay relatively constant. Interestingly there were no such divergence among all single women with children apparent in the

Figure 3:
Average Annual Hours Worked for Working Single Women
by Presence of Children, 1984-2000



Reprinted from Eissa and Hoynes 2005, 30.

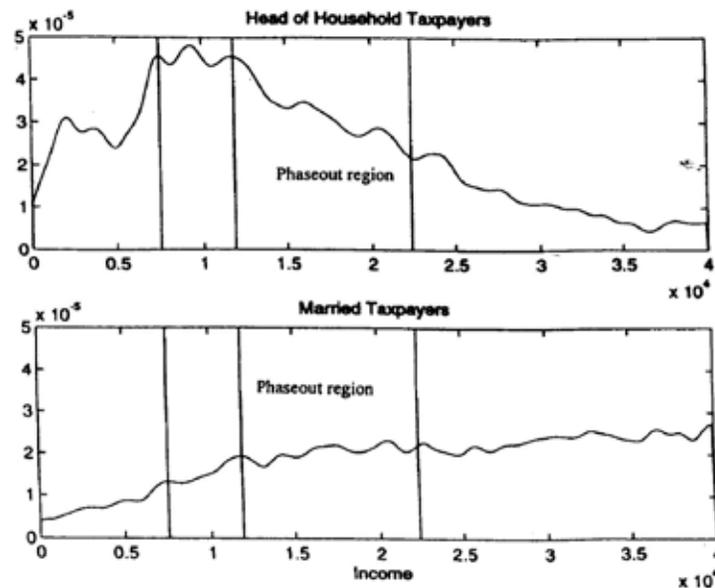
figure Eissa and Hoynes print just one page earlier, reprinted here as Figure 3.

In fact, between 1996 and 2000, hours worked appear to have increased faster for single women with children than for those without when looking at all incomes, whereas in the income range specific to the phase-out of EITC there was no increase at all for those with children but a dramatic rise in 1997 for those

without (the expansion of the program legislated in 1993 was fully implemented by 1996). Little wonder, then, that researchers basing their evaluations of the EITC phase-out on all single women with children get false results. For the years 1996-2000 at least, a researcher looking at all such families regardless of income would conclude that women with children increased their hours faster than those without. But those basing their conclusions only on families in the phase-out range would conclude the opposite, at least for the years 1996-1998. At least Eissa and Hoynes did include the figure in Figure 3, yet even they based their own conclusions on the total population of single women with children. The only other reference by Eissa and Hoynes to the issue of the phase-out range is, as by Liebman (1998), to refer back to the Eissa and Liebman paper of 1996 which will be discussed below.

Eissa and Hoynes are not the only authors who seem to overlook facets of their own figure. One other method of determining whether the phase-out has an effect is to chart incomes to see if people tend to congregate at the “bend points” – i.e. just after reaching maximum benefits and at the end of the plateau range – just before the phase-out begins. Liebman’s 1998 paper included figures showing the distribution of households by income distribution separated according to whether they filed their income taxes in a joint return (married) or as a head of household (single with dependents), reproduced here as Figure 4. Liebman reports that “There is little if any bunching at the kink at the beginning of the EITC phaseout range” (Liebman 1998, 105). In the lower figure, for two-parent families, that is

Figure 4:
1992 Distribution of Tax Returns for Taxpayers with Children



Reprinted from Liebman 1998, 106

certainly true. However, it is difficult to understand why Liebman does not see in the upper figure, representing single parent families, what plainly appears to be a concentration of incomes in the EITC plateau range (the rectangle just before the phaseout region). The implication of this figure would be that head-of-household taxpayers have learned how much they need to earn to gain the maximum EITC benefit and are able to arrange their hours such that they do not have to work extra hours for diminished after tax remuneration in the EITC phase-out range.

However, Liebman is using IRS data which likely includes a great deal of fraud. The IRS spends much time making sure that all genuine income gets reported. It is much less practiced in making sure that all reported income is genuine. The only Americans with an incentive to over-report income are those in the phase-in range. There is believed to be considerable fraud in the EITC, and using IRS data may create a false appearance of bunching. In preparing this paper, I prepared similar figures using CPS data and did not find any bunching.

The 1996 Eissa and Liebman paper presents the one extensive study of the effect on hours worked of the EITC on the phase-out range alone. The 1986 EITC expansion was unique in that, while the maximum benefit was increased, the phase-out range was decreased. The result was that the income range of the phase-out increased dramatically, from a range of \$6500 to \$11,000 before the expansion to a range of \$8510 to \$15,432 after. This created a large group of families subject to the EITC phase-out who had not been before. Eissa and Liebman used that fact to run a regression using a binary variable to differentiate between cases before the EITC expansion and after. The correlation coefficients (they ran more than one regression using populations defined more narrowly than just single women with children, e.g. single women without a high school diploma) came out positive, meaning single women with children in the new phase-out range increased their hours anyway; however, the 95 percent confidence intervals for the coefficients crossed zero, making the findings inconclusive (Eissa and Liebman 1996, 633). I regard the method to be sound, but it has not been repeated for later EITC expansions. Researchers have run new studies of hours worked by single women with children for all incomes, and they brush off questions about the phase-out with a reference to this one study, as if the difference between those in the phase-out range and those not is not an important distinction.

MEASURING THE EFFECTS OF THE 1993 EXPANSION ON HOURS WORKED

Therefore, I sought to create something along the lines of the Eissa and Liebman study of 1996, using, as they did, data from the Census Bureau's Current Population Survey March Supplement, only analyzing the much larger increase in the EITC legislated in 1993. Unlike every previous study, however, I did not limit my sample to women. Most researchers base their study on single women with children because they make up the bulk of the EITC recipient population. How-

ever, those studies were primarily concerned with the phase-in and since most of them did not distinguish the income level, using female headed households was the only way to hone in on the EITC population. In the phase-out income range, which today reaches past \$35,000 a year, female headed households are much less dominant. In the 600 randomly selected households for my study 197 were single parent households headed by women.

I generated a random selection of 200 households with incomes corresponding to the phase-out range of the EITC from the 1993 survey, 200 from the 1994 survey, and 200 from the 2006 survey. The legislation passed in 1993, and that, therefore, was the last year before the increased phase-out rates. The legislation set 1994 and 1995 as transition years, though by 1995 the phase-out rates were nearly identical to the permanent ones. The three years I chose were the last year before the expansion of the program began, the transition year 1994, and the most recent year for which data was available.

Rather than a before/after binary variable as Eissa and Liebman used, I established the EITC phase-out rate that applied for each household. Those in the 1993 survey with one child faced a 13.1 percent phase-out rate and those with more than one child a 13.9 percent rate. Among the households from 1994, they had a phase-out rate of 16 percent if they had one child and 17.7 if they had two or more. For those from the 2006 survey, the rates were 16 percent and 21 percent for one and two child families. Unlike Eissa and Liebman, I did not have a control group of people without children.

My dependent variable in the regression was hours worked at all jobs last week. I controlled for age, gender, number of children, education level, whether or not the parent was currently enrolled in school (and if so whether it was part time or full time) and marital status. It should be noted that the Current Population Survey (CPS) has a seven point scale of marital status. As the point of including this item as a control variable is to account for the assumption that, for women in particular, unmarried individuals have to work more hours, the CPS scale was rearranged such that it was in the order of likelihood of receiving income from a spouse. CPS had number 1 as married and spouse at home – civilian, and number 2 married with spouse at home – military. For purposes of this study, this was not a meaningful distinction so 2 was recoded as 1. Also, CPS coded legally separated at number 6, above divorced or widowed. I recoded legally separated as number 3, since legally separated is closer to married than are divorced or widowed.

Two cases, both from 1994 had to be dropped as the respondent apparently did not answer the question regarding hours worked last week. Further caution needs to be taken in recognizing the fact that the EITC phase-out is far from the only marginal tax rate faced by households in the sample. Those who are in the EITC phase-out income range and are also eligible for Food Stamps would be facing an income based phase out of Food Stamps as well. Those in public housing pay 30 percent of their income in rent. Those at the higher end of the EITC

phase-out range have liability under the income tax. Such factors alter work disincentives and are not controlled for here. I see no other significant data issues.

RESULTS

The results of the regression are shown in Table 1. The EITC phase-out rate variable (*eitcrate*) carries a small but statistically significant (to the .05 level) negative coefficient. Although relatively small, the coefficient is important. Since the variable was entered as a number signifying the percent of income lost in benefits, the coefficient represents the number of hours per week a typical person in the phase-out range will forgo for each percentage point of marginal tax rate. The implication is that, all other factors being equal, the 1993 expansion of EITC caused those in the phase-out range to reduce their hours of work by 2.7 hours per week if they have two children (0.38 times a 7.2 percentage point increase in the phase-out range). Those with one child would be expected to reduce their hours by roughly 1.1 hour per week, as the phase-out rate was increased 2.9 points compared to 7.2 for two-child families. Measuring the effects of the entire phase-out disincentive, with a negative 0.38 coefficient, a person facing a 21.1 percent phase-out rate, all else being equal will reduce her work by 8 hours per week as

Table 1: Effect on Hours Worked

Source	Sum sqs.	dgs. fr.	Mean sqs.	Number of obs = 598	
Model	8995	7	1285	F(7, 590) = 10.98 Prob > F = 0.0000	
Residual	69083	590	117	R-squared = 0.115 Adj R-squared = 0.104	
Total	78078	597	130	Root MSE = 10.821	
Variable	Coef.	Std. err.	t	P> t	Beta
age	.08	.04	1.66	0.097	.065
sex	-7.56	.97	-7.78	0.000	-.325
maritalst	.68	.21	3.17	0.002	.134
children	.21	.55	0.38	0.706	.016
education	-.28	.19	-1.25	0.211	-.049
enroll	-9.44	3.46	-2.73	0.007	-.106
eitcrate	-3.86	.18	-2.12	0.034	-.092
cons	59.63	8.23	7.25	0.000	

Data provided in link at Appendix.

compared to what they would be doing in the absence of the EITC altogether.

It may seem unfair to estimate the implied effects of the entire program when most studies have reported only the effects of an expansion of the program. If the results from studies which show the positive effects on workforce participation from merely expanding the EITC are extrapolated we can see that the effect of the entire program must be enormous. If, as studies suggest, the 1993 expansion of the EITC reduced the unemployment rate for women with children by 12 percent, we can only assume that the effect as compared to the absence of the program altogether would be more than a quarter of that population working when otherwise it would not.

POLICY IMPLICATIONS

This article, then, is not anti-EITC. Although the regression found a statistically significant negative effect from the phase-out of the program, that effect is dwarfed by the positive effect of the phase-in. It does indicate that families would be helped, and economic efficiency improved, if the disincentive from the phase out of the EITC can be reduced without harming those aspects of the program which encourage work.

Opportunities to do that passed us by when the \$500 per child tax credit was created and expanded to \$1000. Because the EITC does not pay additional benefits for having more than two children, the \$500 credit was made refundable only to the point where it cancels out payroll taxes that have not already been cancelled out by EITC payments, but only for families with three or more children. The dollar-width of the income range over which families do not have income tax liability against which to take the new credit but have enough earnings such that their EITC does not cancel out all of their payroll taxes is about \$2500. Only families with more than three children in that narrow income band benefit from refundability of the \$1000 credit. If, instead, the credit was made part of the EITC, but the \$1000 per child remained a universal refundable credit, the EITC would only have to be phased down to that amount rather than to zero, enabling a smaller marginal tax rate in the phase-out income band.

Furthermore, if the personal exemption for children were eliminated, the universal credit could be in the neighborhood of \$1600 without reducing revenues to the government. Then only \$1200 of the EITC amount for two children would have to be phased out and that could be done with perhaps a 5 percent rate without extending the phase out range much beyond the income level it reaches now. Replacing the current \$1000 credit and personal exemption with a universal credit of \$1600 would be a significant tax cut for low income families and a corresponding increase for those with children in the top brackets, for whom the personal exemption is worth more than for those in the lower brackets. Politically, this would be controversial.

CONCLUSION

The original question was whether or not the positive incentives from the phase in of the EITC had a greater effect than the disincentives of the phase-out, given that the phase-in was a higher percentage but the phase-out affected more people. Previous researchers have focused on the positive effects and treated the phase-out more as an afterthought. Except for the landmark study by Eissa and Liebman (1996), researchers have also tended to ignore the fact that only those actually in the phase-out income range face the disincentive. Using hours worked, as some have, is bound to bring about mixed results since an expansion encourages those already in the phase-in range to increase hours. The study by Eissa and Liebman (1996) of the response to the 1986 expansion had not been repeated for later expansions.

This paper conducted a study similar to Eissa and Liebman (1996), with some differences, surrounding the major expansion of 1993. The result was a small but statistically significant negative affect on work by those in the phase-out range. However, in answer to the original question, this effect appears much smaller than the positive effects on workforce participation found in numerous other studies. Therefore, on the whole, the EITC seems to encourage work – if I have doubts about that it would only be because we might be misled by fraudulently reported income in the phase-in range and unreported income in the phase-out range. Assuming the numbers are not terribly misleading, some changes to the program and integration with other tax advantages for families with children could make it an even more effective work incentive.

APPENDIX

The data from the 598 Current Population Survey cases used for Table 1 can be found here. Included is a brief explanation of the scale for variables that may not be self explanatory. [Link](#).

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The EITC Disincentive: A Reply to Paul Trampe

HILARY HOYNES¹

ABSTRACT

PAUL TRAMPE RAISES AN IMPORTANT POINT IN HIS PIECE, “THE EITC Disincentive: The Effects on Hours Worked from the Phase-Out of the Earned Income Tax Credit.” His main thesis is that the EITC is expected to lead to a reduction in hours worked for those in the phase-out region of the credit, and furthermore, that the literature has not given much attention to the issue. After summarizing the literature, Trampe goes on to present some estimates of his own on the impact of the EITC on hours worked for women in the phase-out region of the credit.

I agree with his point that we expect hours to decrease. In my view, however, it has been examined in the literature. But the literature has failed to find a consistent negative impact of the EITC on hours worked. This, I think, is a bit of a puzzle. That puzzle led Nada Eissa and I (Eissa and Hoynes 2006) to review the literature along with a theoretical discussion as to why hours might not respond.

The discussion and analysis in Trampe’s work, however, is not up to the standards of the papers he cites. In this reply to his comment, I discuss several omissions, errors in interpretation, and problems with his empirical analysis.

THE THEORY

Trampe’s analysis of the impact of the EITC on labor supply ignores the income effect of the EITC. This leads to misstatements about the expected impacts of the EITC. Basic labor supply theory shows that an increase in income will lead to a reduction in labor force participation and hours work. This is known as the income effect. Theory also shows that a compensated increase in wages leads to an increase in labor force participation and hours worked. This is known as the

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wage or substitution effect

The expected impact of the EITC on hours worked varies with the region of the credit. In the phase-in region, the EITC leads to an ambiguous impact on hours worked due to the negative income effect and positive substitution effect. Trampe, by ignoring the income effect, incorrectly concludes that the EITC is work-promoting in the phase-in region. In the flat region, the EITC produces a negative income effect leading to an unambiguous reduction in hours worked. Again, by ignoring the income effect, Trampe (311) makes an incorrect statement (“those in the flat range do not face a disincentive”). In the phase-out region, the EITC produces a negative income and negative substitution effect leading to an unambiguous reduction in hours worked. Moreover, the phase-out of the credit alters the budget set in such a way that some taxpayers with incomes beyond the phase-out region may choose to reduce their hours of work and take advantage of the credit.

With this background, one can see that, among those likely to be affected by the EITC, hours are expected to decrease for all those except possibly those in the phase-in region. It is this observation that leads many researchers to examine overall hours worked rather than focusing on the selected sample of those in the phase-out range (more on this later). While not mentioned by Trampe, many authors limit their sample to less educated women in an effort to target those most likely to be impacted by the EITC.

THE EXISTING LITERATURE

The literature review in Trampe’s comment is missing many EITC papers that are relevant for his analysis. I briefly mention them here. Meyer and Rosenbaum (2000) present estimates using many different control groups. Further, Meyer and Rosenbaum (2001) is cited by Trampe but an important feature of their work is overlooked. Trampe states in his criticism of Eissa and Hoynes that they “do not mention one caveat, namely, that over this same time period, AFDC was being reformed with the main effect being to push recipients into jobs” (310). Meyer and Rosenbaum use data during the welfare reform period and very carefully control for its impacts.

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

While Trampe focuses his review on single mothers, it is important to also recognize my work with Nada Eissa which examines impacts of the EITC on the labor force participation (Eissa and Hoynes 2004) and hours worked (Eissa and Hoynes 2006) of married couples. This is quite important given that in Trampe’s own empirical analysis he pools single women and married women.

INTERPRETING THE LITERATURE

At several points in the comment, Trampe cites descriptive statistics on trends in labor supply alongside estimates of the impacts of the EITC on labor supply. Obviously, descriptive trends are not conclusive as to the impact of individual policies because there is much else changing over time. The second paragraph in the section “Previous studies: Labor Force Expansion” (310) makes this mistake.

Trampe presents Figure IX from my work with Nada Eissa (Eissa and Hoynes 2005) to illustrate the tendency in the literature to misread graphs. I disagree with his analysis and conclusions. First, Trampe states that “they do not comment on the dramatic increase in hours worked by single women without children starting in 1984 which was not accompanied by a similar increase for those with children” (313). The EITC did not expand until the Tax Reform Act of 1986 so any change by single women without children prior to 1986 is not relevant. Our statement holds if, as one should, one looks POST the policy expansions. Second, Trampe states that “They also do not comment on the dramatic rise and subsequent fall in hours for those without children between 1997 and 2000 while the hours of those with children stay relatively constant” (313). There was no policy change after 1993 so any fluctuation between 1997 and 2000 should not have anything to do with the program! With these basic errors in analysis, it seems quite inappropriate later to state that “Eissa and Hoynes are not the only authors who seem to overlook facets of their own figure...” (314).

CRITIQUE OF TRAMPE’S ECONOMETRIC ANALYSIS

The basic approach taken in the comment is to create a sample of observations for women with income in the phase-out region of the credit. Then Trampe regresses hours (for those working) on various demographic controls and the EITC phase-out rate that the woman faces. The sample includes observations pre and post 1993 Omnibus Reconciliation Act expansion.

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

(2004). Another alternative is to use structural modeling (Heim 2005, Keane and Moffitt 1998).

Ignoring the selection problem, however, there remains a fundamental problem with the empirical model. The main policy variable, the phase-out rate, varies with family size and year. I suspect that the majority of the identification comes from the tax-reform induced yearly variation in the phase-out rate. That, in and of itself, is fine. The problem is that there is no control for year fixed effects in the model. Therefore, if there are any other factors that vary by year (labor market effects, other trends, other policies) the estimates will be biased unless there are perfect controls for these features (and in point of fact, there are NO controls of this sort in the model). This is a fundamental problem with the empirical model and in fact is the main reason that people use control groups; ideally they are selected such that they face the same environment except for not facing the policy change.

The empirical analysis pools both single and married women: “In the phase-out income range, which today reaches past \$35,000 a year, female headed households are much less dominant. In the 600 randomly selected households for my study 197 were single parent households headed by women” (315). This seems fine (in fact note that Eissa and Hoynes (2004, 2006) already have recognized this issue and this motivated our analysis of the impacts of the EITC on the labor supply of married couples) but the results should be separately estimated for the two groups. The determinants of labor supply of married couples differ from singles and this should be reflected in the empirical model. This would also seem important given the interest in comparing results to the literature, where single and married women are always analyzed separately.

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

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ECONOMICS IN PRACTICE

Got Replicability?

The *Journal of Money, Credit and Banking* Archive

B.D. McCULLOUGH.¹

ABSTRACT

During the period 1982-1984, all submissions to the *Journal of Money, Credit and Banking* (*JMCB*) were required to submit data and code to The journal's office. The results of this project, published in Dewald, Thursby and Anderson (1986), shocked the economics profession. The vast majority of the research could not be replicated. As a result, the *American Economic Review* (*AER*) adopted its "replication policy" whereby authors were required to provide their data and code to other researchers upon request (Ashenfelter et al 1986). The *AER* provided no formal enforcement procedure or penalty for failure to comply. Dewald, Thursby and Anderson (1986) recommended a mandatory archive instead of a mere "policy" lacking formal accountability. Many journals followed the *AER*'s lead and adopted "policies". When McCullough and Vinod (2003) showed that the *AER* policy was ineffective, then-editor Bernanke (2004) instituted a mandatory data+code archive. At that point, no one had ever analyzed any of the existing archives.

Kerry Anne McGeary, Teresa Harrison, and I analyzed several years of the *JMCB* data+code archive, 1996-2003. Of 266 articles published during that time, 193 were empirical and should have had data and code deposited in the archive. Of these, only 69 actually had anything in the archive; 11 of them had data only, and seven articles required software or other resources we did not have. Excluding these seven, the *JMCB* archive only enabled replication of 14 of 186 empirical articles. Two primary reasons that the archived data and code did not reproduce the published results (assuming that the results actually are replicable) is carelessness on the part of the authors and a failure of the editors to provide proper instruction to

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For comments and useful suggestions, thanks are due to Teresa Harrison, Kerry Anne McGeary, H. D. Vinod, and a referee.

authors on how to prepare replication files. The interested reader is invited to consult our paper (McCullough, McGeary, and Harrison, hereafter abbreviated MMH 2006), which gives several examples. The obvious conclusion is that the mandatory data+code archive of the *JMCB* did not work to ensure replicability. Based on our experience attempting to use the archives to replicate all those articles, we recommended several procedures to enhance the replicability of *JMCB* articles.

RECOMMENDATIONS WE MADE FOR AN EFFECTIVE ARCHIVE

Here I reiterate the recommendations we made for an effective archive, and make comments to explain or elaborate.

[A] The README file should list all the replication files with a brief description of each. It should clearly indicate which programs correspond to what results in the paper.

- H.D. Vinod and I attempted to replicate all the empirical articles in the June 1999 issue of the *American Economic Review* (McCullough and Vinod 2003). We noted that one of the authors (who was on the AER editorial board at the time), “after several months and numerous requests, finally supplied us with six diskettes containing over 400 files – and no README file. Reminiscent of the attorney who responds to a subpoena with truckloads of documents, we count this author as completely noncompliant” (McCullough and Vinod 2003, 887-888). Trying to reproduce the results without a good README file was hopeless. This problem also occurred when MMH attempted to replicate results in the *JMCB*.

[B] The README file should also contain a data dictionary that defines each variable and gives the provenance of all the data.

- Levitt (1997) published an article in which his primary contribution was the creation of an instrumental variable that would break the simultaneity between police and crime. This variable was constructed by noting the timing of mayoral and gubernatorial elections. When McCrary (2002) attempted to reconstruct this variable he was unable to do so. In his reply to McCrary’s comment, Levitt (2002) admitted that he was unable to reconstruct this variable either. If Levitt had a data dictionary for his original article, the error would not have occurred. MMH attempted only to use the supplied data and code, not to verify that the data were correct. If they had attempted to verify the data, in most cases they would have been frustrated because most authors did not specifically describe where their data came from. It is one thing to say that data came from the *Survey of Current Business*. It is another thing to say which issue, table and page the data came from, since economic data may be revised several times.

[C] The README file should identify the version of the software used (by version number and/or release date) and similarly for the operating system on which the software runs.

- Different versions of the same software package sometimes produce different answers to the same problem, for example, when a bug is fixed. Run on different versions of an operating system, the same software can produce different answers. McCullough and Vinod (2004, 394) document a case in which the more recent version of the operating system had better math libraries. A very small number had incorrectly been evaluated as “zero” by the old operating system, producing a log-of-zero problem that did not appear with the more recent version of the operating system. With the old version the program came crashing to a halt because the log of zero does not exist.

[D] All data should be provided in ASCII format, and the version of the code submitted to the archive should call these same ASCII files.

- Data that are provided in machine-readable form often cannot be read by another package. Thus, someone trying to use a software package different than the one used by the original author might not be able to read the data. Further, the requirement that the code call the ASCII data ensures that the code will work with the data that are provided, and that the data provided really do reproduce the published results (below I tell of this problem as encountered in trying to replicate the *JMCB* article by Lastrapes in the December 2006 issue of the *JMCB*), as discussed below.

[E] Authors should provide the original data from which the final dataset is derived and all instructions/code necessary to turn the original data into the dataset analyzed.

- Often data from different sources are combined to produce a usable dataset, or data are transformed before use. Without the exact record of how that was done, it may be impossible to replicate the dataset. A good example of this is the Caroline Hoxby/Jesse Rothstein debate over schooling. Rothstein (2004, 8) wrote, “A major difficulty in replicating Hoxby’s sample is the matching of NELS schools to MSAs, as the NELS offers several indirect indications of schools’ locations but no direct MSA code. Hoxby reports an 8th grade sample size of 10,790 students from 211 MSAs, but does not report her geocoding algorithm. I am unable to replicate her exact sample size.” Naturally, if Rothstein couldn’t get the correct sample, he couldn’t reproduce Hoxby’s published results.

[F] The author should provide code such that the data and code, when placed in the same subdirectory, will execute. Also, the output from doing so should be provided. The author should check to make sure that this runs correctly and produces the results in his paper.

- Xiangrong Liu and I encountered that problem when trying to replicate the Meiers and Mueller article in the December 2006 issue of the *JMCB* (McCullough and Liu 2007), as discussed below.
- [G] The archive should list each paper regardless of whether the paper has been exempted from the rules. Sometimes a paper has been exempted from the data requirement. In such cases, the archive should say that the paper has been exempted from the data requirement, and the code should still be required.
- That problem marks the Brevoort and Hannan (2006) article in the December 2006 issue of the *JMCB*.
- [H] The journal should issue conditional acceptance letters, with a formal acceptance letter being sent only after the data+code have been archived.
- The *Journal of Applied Econometrics* has 99% compliance, because the editors do not send an acceptance letter until the archive manager, James MacKinnon, has informed them that he has received the required material. MMH (2007) compared the proportion of empirical articles that have archive entries for several journals. Some of their results are summarized in Table 1.

Table 1: Lifetime Compliance

Journal	Empirical articles	Entries	Compliance %
<i>J. App. Econometrics</i>	292	290	99
<i>Fed. St. Louis Review</i>	219	162	74
<i>JMCB</i>	193	66	34
<i>J. Bus. Econ. Statistics</i>	342	121	35

Number of empirical articles (that should have archive entries), actual number of archive entries, percentage of empirical articles that have archive entries.

Most journals with archives do not bother to make sure that anything is archived. The missing 1% of *JAЕ* articles were due to special issues that were exempted from the usual process. Note, however, that the *JAЕ* requires only data, not code, so there is still room for significant improvement.

[I] Managing the archive should be an editorial function.

- Let us learn from the success of the *Journal of Applied Econometrics*, where managing the archive is an editorial function. I think it fairly easy to find a fastidious junior professor who would pay necessary attention to the archive. A major problem with the old *JMCB* archive was that managing the archive was a secretarial function.

[J] The pages of the journal should give space to replication attempts, at a minimum by publishing a one-page summary with supporting materials placed in the archive.

- In the history of the *JMCB* archive surveyed by MMH, most articles could not be replicated. During that period, the editors published not a single note on the replicability of any article. Authors knew that the lack of replicability of their research would not be exposed by the journal. The editors should welcome replication attempts, and publish responsible ones. (In the meantime, send your critiques to *Econ Journal Watch!*)

Again, the *Journal of Applied Econometrics* sets a good example. The *JAЕ* has a Replication Section, edited by Baldev Raj, and will correct published errors—see, for example, Kleiber and Zeileis’s (2005) correction of Bai and Perron’s (2003) influential article and software on structural breaks. The *JAЕ* is not afraid to admit that researchers might make mistakes or (in the case of Bai and Perron) that software has bugs.

***JMCB* RESPONSE TO McCULLOUGH, McGEARY, & HARRISON**

The MMH article was formally accepted in 2003, but for whatever reason it was not published until June 2006, which is when the editors followed with a brief editorial about new procedures (*JMCB* Editors 2006). They announced in the June 2006 issue:

Since January 2005, the *JMCB* research assistants have been replicating some of the results from the *JMCB* Archive for the purpose of helping the editors to evaluate various methods to improve the policy. So far, we have adopted the following new procedure:

1. When a paper is submitted, the *JMCB* office writes to the corresponding author the following: Publication of any paper regarded as empirical is conditional on compliance with the data archiving policy below. If your paper is empirical, please start to prepare data, programs, and a README file now. If providing data is infeasible, please write to the *JMCB* office to ask for an exemption in writing as soon as possible.
2. If the exemption is requested, then the Editor will notify the *JMCB* office whether or not the data requirement is waved for the particular paper, and the *JMCB* office will let the author know by the time of the first turn-around time. If an exemption is requested and refused, the Editor will let the author know as soon as possible.
3. When a paper is accepted, the *JMCB* office will remind the corre-

sponding author that the publication is conditional on our receiving data, programs, and a README file unless that the paper is given an exemption by the accepting Editor. (The *JMCB* Editors 2006, 1108)

The three new procedures are in the spirit of the MMH recommendations. The editors refrained from the other recommended procedures, stating that they “do not agree with all the arguments made by [MMH].” The editors did not say which arguments they disagreed with, or the grounds for disagreement.

Since the editorial appeared in June 2006 and states, “we have adopted the following new procedures,” it seems reasonable to suppose that articles appearing four issues later, in December 2006, would be subject to the new regime. Thus, enough time has passed to see whether the changes implemented by the editors have been effective at ensuring that *JMCB* publishes replicable research. The present paper examines the December 2006 issue of the journal and concludes that new policy is not yet working.

Table 2: Articles in the December 2006 issue of *JMCB*

		Archive?	
		Empirical?	
1.	“A New Analysis of the Determinants of the Real Dollar-Sterling Exchange Rate: 1871-1994” by Ivan Paya and David A. Peel	✓	0
2.	“Commercial Lending and Distance: Evidence from Community Reinvestment Act Data” by Kenneth P Brevoort and Timothy H. Hannan	✓	0
3.	“Is There a Cost Channel of Monetary Policy Transmission? An Investigation into the Pricing Behaviour of 2,000 Firms” by Eugenio Gaiotti and Alessandro Secchi	✓	0
4.	“When Did the FOMC Begin Targeting the Federal Funds Rate? What the Verbatim Transcripts Tell Us” by Daniel L. Thornton	✓	0
5.	“Dollarization Traps” by John Duffy, Maxim Nikitin, and R. Todd Smith	0	0
6.	“Fleshing Out the Monetary Transmission Mechanism: Output Composition and the Role of Financial Frictions” by Andre Meier and Gernot J. Mueller	✓	✓
7.	“Technical Trading-Rule Profitability, Data Snooping, and Reality Check: Evidence from the Foreign Exchange Market” by Min Qi and Yangru Wu	✓	0
8.	“Inflation and the Distribution of Relative Prices: The Role of Productivity and Money Supply Shocks” by William Lastrapes	✓	✓
9.	“Linking Individual and Aggregate Price Changes” by Attila Ratfa	✓	0
10.	“Cities and Countries” by Andrew K. Rose	✓	0
11.	“Inflation Inertia and the Optimal Hybrid Inflation/Price-Level Target” by Oisten Roisland	0	0

The December 2006 issue has 11 papers, which are listed in Table 2. As can be seen, 9 of the 11 papers were decidedly empirical and yet, the archive had entries only for two, the paper by Meier and Mueller and the paper by Lastrapes.

EMPIRICAL ARTICLES WITH NO DATA/CODE IN THE ARCHIVE

With respect to the seven December 2006 *JMCB* empirical articles for which there was nothing in the archive,² it is of interest to ascertain why there are no archive entries and whether data and code actually exist. To do this, an email was sent to all authors of each article, inquiring whether the data and code were submitted to the *JMCB* and asking for the data and code. Our interest is only in the extent to which the journal ensures replicability of published results, not whether the articles themselves are replicable. So no attempt is made to replicate the articles in this section.

Ivan Paya and David A. Peel

No response was received from either author.

Kenneth P. Brevoort and Timothy H. Hannan

Brevoort responded that their article used confidential data—something not indicated in the archive. Further, the journal only exempted the data from the archive requirement, not the code. If some researcher wishes to apply the same method to a different data set, should he have to re-invent the wheel? Even if the data are not available, ought not the code be available for inspection? MMH made two recommendations that are relevant here. First, if confidential data are used, this should be noted in the archive. Second, if confidential data are used, the code nonetheless should be made available.

Eugenio Gaiotti and Alessandro Secchi

No response was received from either author (aside from an “out of the office” auto-reply from Gaiotti’s email account).

Daniel L. Thornton

Thornton’s article made minimal use of the data, computing only summary statistics. Thornton replied that he no longer had the original data, but did quickly put together some similar data and supplied them. The original data on which the results are based are now lost and unrecoverable. If the journal had requested his data at the time of publication, this article would be replicable. Ideally, he would have supplied code and information about the software used (including version)

² In fact, for those seven articles, there continued to be nothing in the archive right up to August 2007 when the present article was finalized.

and the operating system, since, as noted, both can affect computed results.

Min Qi and Yangru Wu

Qi responded that he supplied the data and code to the journal in August 2005, and that he had no idea why it has not been posted to the archive. In that same email he sent the data and code. Perhaps his email never made it to the journal, but it is clear that somebody at the journal should be, but is not, checking to see whether data and code have been submitted before sending articles to the publisher.

Antilla Ratfai

No response was received.

Andrew K. Rose

Rose responded that the data and code are posted at his personal website, and he did not recall whether he submitted the data and code to the journal. I checked his website (on 22 August 2007) and did not find the code available. I did find the output from running his code. The output contained the original commands, but to produce usable code a replicator would have to do much cutting and pasting.

EMPIRICAL ARTICLES WITH DATA AND CODE IN THE ARCHIVE

There were two empirical articles with data and code in the archive. A tenet of Gary King's "Replication Standard" is that the replication should be feasible without contacting the original author or anyone else (King 1995, 444). The two papers were ultimately replicable, but only with the assistance of the original authors.

The authors of the first of the two empirical articles with data and code, Andre Meier and Gernot J. Mueller, submitted machine-readable MATLAB code that could be read only by MATLAB; similarly, their data files were readable only by MATLAB. They did have a README file in ASCII, but it was missing much of the information recommended by MMH. If the purpose of the archive is only to reproduce published results, then there is no need for the code to be readable by humans. The primary purpose of the archive is to support the extension of research, and the archived data and code should facilitate the porting of the code from one package to another. Machine-readable data/code cannot do this. MMH recommended that data and code be human-readable.

When Xiangrong Liu and I ran Meier and Mueller's data and code, the output produced results that clearly differed from the published results (McCullough and Liu 2007). The authors, when contacted, provided necessary details for modifying the code so that it would reproduce some of the published results. That

information should have been in the README file. Even then, only two of three tables could be reproduced, and it was necessary for the authors to provide yet further instruction before the third table could be reproduced. What should have been a simple matter of loading data and code into a common directory and executing the code took several hours and required multiple communications with the authors spread out over several days.

The second empirical article with data and code in the archive was authored by William Lastrapes. In the course of replicating the Lastrapes article, Bret Meyers and I discovered that Professor Lastrapes had omitted a variable from his dataset, and also included the incorrect version of a necessary subroutine (McCullough and Meyers 2007). Professor Lastrapes, when contacted, quickly provided the missing variable and the correct version of the subroutine. The journal should have provided him with instructions on how to provide replication files that satisfy King's Replication Standard. For example, Lastrapes' code loaded the data from several Excel files. The journal told him to provide ASCII data, so he just exported the data to unlabelled ASCII (no variable names, just numbers) and did not change his code. Since a variable was missing, there was no possible way that we could correctly identify the names of the variables in the ASCII data files. Furthermore, even if the missing variable had been provided, only someone with a great deal of expertise in RATS could have matched the variable names in the code to the columns in the data files.

Again, MMH recommended that the journal instruct authors to prepare their data and ASCII code so that it will all run from one subdirectory. This would have enabled Lastrapes to catch his errors before submitting his data and code.

Further, there were version-dependent errors produced by the code that could have been avoided if the version of RATS run by Lastrapes had been known (this information was obtained by contacting Lastrapes). MMH recommended that authors be required to place this information in the archive.

Many researchers are under the impression that if they just provide some version of their data and code, then any other researcher can reconstruct the results. That was shown to be untrue by, among others, Dewald et al (1986), McCullough and Vinod (2003), and MMH. Even original authors are often unable to reproduce their published results. It is clear that authors need some written instruction on how to prepare replication files, and that the journal should provide it.

CONCLUSIONS

Some say that economics needs math for rigor, clarity, and accountability. Surely that view contains some truth. Yet critics have often argued that math can also produce opacity and a lack of accountability. As noted at the start of this paper, investigations into the replicability of econometric results have dispelled

the vision of the applied econometrician as saint of replicable research. Investigations I have undertaken have convinced me that accountability will not be achieved by fuzzy hopes and gestures. In addition to the failures in documenting and providing data and code, there is the simple matter of authors' responding to inquiries about their work. As indicated here, a significant portion of authors simply "blow off" research inquiries.

MMH criticized the *JMCB* archive project, identifying two major flaws: (1) most authors of empirical articles did not contribute data and code; and, (2) when data and code were contributed, the data and code did not reproduce the published results (MMH, 1005). In response, The editors instituted a new set of procedures.

A spot check of the December 2006 issue of the journal reveals that the new procedures have not ensured that the data and code in the archive can reproduce the published results. Of eleven articles in that issue, nine were empirical and only two had data/code in the archive. Furthermore, the data/code in the archive failed to reproduce the published results.

Perhaps the December 2006 issue of the journal was a fluke. Perhaps all the other issues since the publication of *The editors* (2006) have data and code archived for each empirical article, and the data and code reproduces the published results of each article. The interested reader is invited to check the *JMCB* archive ([link](#).)

Such a reader would find as of this writing (September 1, 2007) that the archive stops with December 2006, even though 2007 issues have been published for February, March/April, June and August. It is difficult to imagine that it would take more than six months to archive the data and code from the February issue, for example.

All available evidence indicates that replicable economic research is the exception and not the rule, and this appears to be the case at the *JMCB* even though it nominally has a mandatory data+code archive. The editors of the *JMCB* are to be commended for wanting to make The journal more scientific and for making gestures toward ensuring that results published in their journal are replicable. Achieving this, however, will require more than mere gestures. I believe that the procedures needed were recommended by MHH. They will require serious editorial commitment.

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B. D. McCullough received a B.A. in economics from Georgetown University in 1981 and a Ph.D in economics from the University of Texas at Austin in 1989. After five years as an assistant professor of economics at Fordham University, he joined the Federal Communications Commission as a senior economist where he began his work on the accuracy of statistical software and the replicability of published research. He has published replicability results and commentary in *American Economic Review*, *Journal of Economic Methodology*, *Journal of Money, Credit and Banking*, *Indian Journal of Economics and Business*, *Journal of Economic Perspectives*, and *Journal of Economic and Social Measurement*. Since 2001 he has been on the faculty of the Decisions Sciences Department at Drexel University, being promoted to Professor in 2006. His email is bdmccullough@drexel.edu.

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ECONOMICS IN PRACTICE

Thriving at Amazon: How Schumpeter Lives in Books Today

ARTHUR M. DIAMOND, JR.¹

ABSTRACT

Capitalism . . . is by nature a form or method of economic change and not only never is but never can be stationary. . . . The process of Creative Destruction is the essential fact about capitalism.

-- Joseph Schumpeter, *Capitalism, Socialism, and Democracy* (1950, 83-84)

SCHUMPETER FAMOUSLY ARGUED THAT THE ESSENTIAL FACT ABOUT CAPITALISM is creative destruction, and evidence generally supports the claim (Diamond 2006). Academics are increasingly recognizing the importance of Schumpeter's work (Diamond 2007d). Since the mid-1990s annual citations to *Capitalism, Socialism and Democracy* have even exceeded annual citations to Keynes's *General Theory*.

Nonetheless, the economics profession generally has neglected the process of creative destruction in its research, in what it teaches (Diamond 2007b; Johansson 2004), and in its policy advice. I see two main reasons for the neglect. One involves the difficulty in representing some important aspects of creative destruction in formal models. Another is the common belief that creative destruction's negative effects on labor outweigh its positive effects on consumers (I discuss the first reason in Diamond 2007a and the second in Diamond 2007c).

Life is continual action. To act effectively, part of what business practitioners, government policy makers, and voters need to know is how the economy works. Un-

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I appreciate useful comments from Charles McCann and an anonymous referee. I am grateful for excellent and substantial research assistance on this project from Angela Kuhlmann. More recently, I have also received substantial, able research assistance from Molly McGrath. Able assistance has also been provided by Miaomiao Yu. The current paper is a small part of a broader project arguing that creative destruction should be given increased attention in research, teaching and policy. Other components of the project can be found, and downloaded from artdiamond.com.

fortunately for these actors, most academic economists seem not to care much about how useful their research is to these practitioners. Academic economists generally write for each other through journal articles. Book authors, in contrast, are more likely to care about how useful their research is to practitioners. So one way to learn what practitioners find useful is to look at the supply and demand of ideas as represented by the Amazon book market. Does the market make much use of Schumpeter's ideas? And if it does, which of Schumpeter's ideas does it find most useful?

The phrase "creative destruction" has stimulated a wide audience of thinkers and doers. No fewer than 14 books have appeared since the mid-1990s with "creative destruction" *in the title*.

On October 23, 2003, Amazon introduced a new feature called "Search Inside the Book" (Wolf 2003). *Wired* journalist Gary Wolf reported that as of that date, Amazon had over 120,000 books available through the feature. That amounted to more than 33 million pages, from more than 190 publishers, "including Wiley, Time Warner Book Group, Simon & Schuster, Inc., Random House, Inc., and many others" (Price 2003). As of November 2006, information scientist Peter Jasco estimates that approximately 280,000 books were in the "Search Inside the Book" database (Jasco 2006).

If a publisher submits a book to the "Search Inside the Book" feature, then Amazon customers are able to search the entire contents of the book for pages where any word appears. The customers can then read the pages where the word appears, as well as the preceding two and following two. Since its debut, not much additional information about the feature has become available.

Because the "Search Inside the Book" resource is still evolving and unexplored, the results reported here are tentative. Since there would be many more books that mention Schumpeter than could be examined in a first effort, it was necessary to select a sample.

Using Amazon's "bestselling" ranking criterion, on February 29, 2004, a "Search Inside the Book" search for "Schumpeter" yielded 2,692 "results." On May 11, 2004, a "Search Inside the Book" search for "Schumpeter" yielded 2,866 "results," when ranked by the "bestselling" criterion. So over a period of about 10 weeks, 174 Schumpeter-referencing books were added to the "Search Inside the Book" feature. This represents a 6.5 percent increase over the period.

At intervals since "Search Inside the Book" began, I have entered Amazon and searched for "Schumpeter" under the "books" heading. The "results" number has increased steadily, as seen in Table 1. Admittedly, the increase could be simply due to an increase in the number of books included in the "Search Inside the Book" list, but it might also reflect an increased propensity of authors to reference Schumpeter.

I established eight content categories, and a ninth miscellaneous category, as shown in Table 2. Graduate student Angela Kuhlmann then examined the mentions of Schumpeter, and classified them for each book. If Schumpeter was mentioned on several pages of a connected discussion, then that was counted as one reference. Only if mentions of Schumpeter were in separate sections, or chapters, or were separated by at least several pages, were they counted as multiple references. In cases

where there were 10 or fewer pages with mentions of Schumpeter, all of these pages were examined. In cases where there were more than 10 pages with mentions of Schumpeter, a sample of the pages was examined from each connected collection of mentions.² In the infrequent cases where a reference could be placed in more than one category, a judgment was made of which category was the main one.³

We currently have names and titles of 3,719 books in the Schumpeter Amazon database. Of these we have done content-analysis for 1,176 books. On the basis of book titles, books were also classified as being about business, economics, both business and economics, or neither business nor economics. Any book with any form of the word “economics” in the title was considered “economics.” We classified broadly, in the sense that if we were not sure if a book belonged in a category, we included it. Any book that was about some aspect of business, or intended to be useful to those engaged in business, or about some aspect of technology, was considered “business.”

The oldest book in the sample of 1,176 had a copyright date of 1957, while the most recent had a copyright date of 2005. The mean was 1998.3, while the median was 2000. An Excel spreadsheet listing the 1,176 books, and their classification by content category and by “economics” or “business” can be found at www.artdiamond.com.⁴

Table 3 provides the basic results of the content-analysis of the 1,176 books that reference Schumpeter. There are two ways to measure the number of references to Schumpeter. The “book” measure counts each book that refers to Schumpeter as one reference. The “reference” measure counts each separated reference within a book as a separate reference. In Table 3, the top numbers are “books,” while the bottom numbers are “references.” For example, if a book had three references to creative destruction, it would be recorded under “Creative Destruction” as “one” under the books measure and “three” under the “references” measure. (It follows that the “books” are always less than or equal to the “references” in each cell.) The main result in Table 3 is that a significant number of the references to Schumpeter are on issues related to creative destruction. This main result remains true whether we examine the “books” measure or the “references” measure.

Table 4 addresses the question of whether books on business, economics, or other fields were more likely to have their Schumpeter references be references to creative destruction. The “narrow” business category included books that were categorized as being on business, but not as being on economics. The “broad” business category included the books in the “narrow” category, and also included the books that were classified as both on business and on economics. (A similar distinction

² Three prominent examples are: Foster and Kaplan’s *Creative Destruction* with 26 pages of mentions, Heilbroner’s *The Worldly Philosophers* with 38 pages of mentions, and Muller’s *The Mind and the Market* with 67 pages of mentions.

³ Endnotes were not counted as separate references except in the infrequent cases where they included a discussion of Schumpeter that did not correspond to a Schumpeter reference in the main text. Mentions in bibliographies or in indexes were not counted.

⁴ The data set is too large to provide this information as a table in the paper itself. Also posted on the web site, is a document called “Amazon Data Details,” that provides more information on variable definitions and other aspects of the data set.

was made between the “narrow” and the “broad” economics categories.) The main result of Table 4 is that *references to Schumpeter in business books were much more likely to be references to creative destruction than were references to Schumpeter in economics books.*

I do not think that Amazon customers are always right. In this particular case, however, I think that they have found a concept to be useful, that academic economists have too often ignored.

Table 1: Number of Amazon Books that Reference Schumpeter Over Time*

Date Searched	Number of “Results”	
2004-02-29	2,692	*Just the last name was used as the search term. The rate of increase in books referencing Schumpeter would reflect some combination of the increase in the number of books included in “Search Inside the Book” and any increase in authors’ propensity to reference Schumpeter.
2004-05-11	2,866	
2005-02-18	5,307	
2007-02-01	7,747	
2007-05-03	8,086	

Table 2: Elaboration of Content Analysis Categories

Type of Reference	Elaboration of What Counted
Creative Destruction	Had to specifically use the phrase “creative destruction.”
Views on Democracy	Examples: the definition of democracy, a discussion of the democratic process, or the competition for votes.
In Role as Historian of Economic Thought	Referring to his writings, or interpretations, as an historian of economic thought.
Long Cycles	Reference to his theory that technology innovations occur in long waves (usually in the context of Kondratieff cycles).
Prediction of Capitalism’s Demise	Refers to his prediction that capitalism will collapse and be replaced by socialism.
Theory of Imperialism	Refers to his theory of imperialism.
Evolutionary Method in Economics	Emphasis on change, not equilibrium; had to use some form of the word “evolution” somewhere in the discussion.
Dynamic (Leapfrogging) Competition	Dynamic competition rather than price competition; had to use the word “dynamic” somewhere in the discussion.
Hypothesis that Large Firms Innovate More	A reference to his hypothesis that larger firms were more likely to innovate.
Miscellaneous	Anything that did not fit neatly into any of the above categories.

Table 3: Content Analysis of References to Schumpeter for 1,176 Books Analyzed*

Type of Reference	Topic Area of Books				
	Just Bus.	Both Bus. & Econ.	Just Econ.	Neither Bus. nor Econ.	All
Creative Destruction	133 (168)	23 (31)	45 (53)	164 (191)	365 (443)
Views on Democracy	2 (2)	2 (4)	9 (13)	132 (178)	145 (197)
In Role as Historian of Economic, Thought	3 (5)	8 (11)	38 (63)	69 (84)	118 (163)
Long Cycles	18 (18)	3 (4)	10 (16)	19 (21)	50 (59)
Prediction of Capitalism's Demise	5 (5)	4 (5)	12 (12)	22 (25)	43 (47)
Theory of Imperialism	0 (0)	0 (0)	5 (5)	33 (42)	38 (47)
Evolutionary Method in Economics	8 (9)	3 (3)	6 (7)	10 (11)	27 (30)
Dynamic (Leapfrogging) Competition	10 (10)	2 (3)	8 (9)	7 (7)	27 (29)
Hypothesis that Large Firms Innovate More	4 (4)	1 (1)	2 (2)	6 (6)	13 (13)
Miscellaneous	121 (165)	31 (52)	115 (179)	341 (429)	608 (825)
Totals	304 (386)	77 (114)	250 (359)	803 (994)	1,434 (1,853)
*The top numbers count the number of books making reference. The bottom numbers (in parentheses) are sums of the number of references---there may be more than one reference from the same book, if the mentions of Schumpeter are separated by several pages.					

Table 4: Content Analysis of References to Schumpeter for 1,176 Books Analyzed*

Field of Book	Percent of All References that are to Creative Destruction
Narrow Business	43.75
Broad Business	40.94
Narrow Economics	18.00
Broad Economics	20.80
Both Economics and Business	29.87
Neither Economics Nor Business	20.42
All	25.45

* The percentages are calculated from the Table 3 "counts" numbers.

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ABOUT THE AUTHOR



Arthur Diamond is a professor of economics at the University of Nebraska at Omaha. His past research has often been on the economics of science, history of economic thought, and labor economics. He recently has published articles on the contributions to the economics of science and technology of Edwin Mansfield, Zvi Griliches, and George Stigler. Currently, he is writing a book in which he argues that the process of creative destruction should be more central to economists' research, teaching, and policy advice. Entries in his blog, [artdiamondblog.com](#), often focus on free markets, creative destruction, or entrepreneurship. His email is adiamond@mail.unomaha.edu.

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CHARACTER ISSUES: CLASSIC REPRINT

Economists Against Smoot-Hawley

ABSTRACT

THE SMOOT-HAWLEY TARIFF OF 1930 SIGNIFICANTLY RAISED IMPORT RESTRICTIONS, reduced trade and prosperity, provoked protectionist retaliation by foreign governments, and damaged the spirit of peace, cooperation, and goodwill.

A summary of economic scholarship on the topic is found in the open-access article by Anthony O'Brien, "Smoot-Hawley Tariff," EH.NET Encyclopedia, edited by Robert Whaples, August 15, 2001 ([link](#)).



Representative W.C. Hawley and Senator Reed Smoot ([link](#))

Economists mustered a remarkable protest to the bill. The chief author seems to have been Paul H. Douglas. In his memoirs, *In the Fullness of Time* (Harcourt Brace Jovanovich, 1972, 71), Douglas wrote:

The six months at Swarthmore were crowded with activity. With Clair Wilcox I drafted an appeal to President Herbert Hoover urging him to veto the Smoot-Hawley tariff, which raised duties to their highest levels. In this we pointed out how the increase in duties on imports

decreased the ability of other countries to buy goods from us. Also, it would provoke them to retaliatory tariffs. No fewer than 1,028 economists signed the appeal, and I think poor Hoover wanted to take our advice. His party was so strongly committed to protection, however, that he felt compelled to sign the bill, with the result that all our predictions came true. The Depression deepened and the Western democracies fell apart. Our letter did make it somewhat easier for Congress later to pass Cordell Hull's reciprocal-trade bill, and thus helped to lead the way to a reversal of our trade policy.

Here we reproduce by permission of the American Economic Association the account of the episode by Frank W. Fetter published in the *American Economic Review* 32(2), June 1942, pp. 355-356, and the petition and signatories as recorded in *Congressional Record-Senate*, of May 5, 1930, pp. 8327-8330.

Incidentally, in 2007, a petition of economists against protectionism was organized by the Club for Growth ([link](#)). Inspired by the 1930 petition, the new petition also gathered exactly 1,028 signatures. That number, however, makes a much smaller portion of economists than in 1930. We commend the organizers, but regret that economists nowadays do not do more to make economic verities more effective in combating bad policies. Robert Whaples's survey of American Economic Association members ("Do Economists Agree on Anything? Yes!," *Economists' Voice*, November 2006, [link](#)) suggests that economists preponderantly oppose protectionism, agricultural subsidies, and sports-franchise subsidies, and lean (on average) toward school vouchers and the elimination of United States Postal Service monopoly privileges. Can economists today do more to combat policies they seem to mostly agree are bad? Would leading economists cooperate across ideological lines to mobilize economic opinion on at least the policies of strongest consensus (such as trade, agricultural subsidies, and sports subsidies)?

-- The Editors

The Economists' Tariff Protest of 1930

FRANK WHITSON FETTER

[from the *American Economic Review*, June 1942]

The economists' statement in opposition to the Hawley-Smoot tariff bill was a unique document. No pronouncement by American economists has ever attracted the public attention that this received. It seems desirable, while memories

are reasonably accurate and some of the correspondence relating thereto is still available, to give a brief history of this protest. Since my friend, Professor Clair Wilcox of Swarthmore, who was the leading spirit in the matter, declines to tell the story, I take the liberty of doing so.

With the sharp division of economic opinion in recent years on so many issues of public policy, it is hard today to realize the almost unanimous opposition of economists, in the spring of 1930, to the tariff bill then pending in Congress. Economic faculties that within a few years were to be split wide open on monetary policy, deficit finance, and the problem of big business, were practically at one in their belief that the Hawley-Smoot bill was an iniquitous piece of legislation. What later developed into a statement backed by over 1,250 economists originated in a very modest way out of the desire of Wilcox and some of his associates at Swarthmore to voice their protest. At the suggestion of Wilcox, Professor Paul Douglas of the University of Chicago, who was then temporarily at Swarthmore, drafted a statement in March that, with some changes in phraseology, was the one given to the press five weeks later. It was decided to ask an economist at each of various eastern universities to sponsor the statement, and then to send it to a member of the economics faculty at each American college, with the request that he solicit signatures from his colleagues. Professors E. M. Patterson of Pennsylvania, Frank D. Graham of Princeton, Henry Seager of Columbia, Irving Fisher of Yale, and F. W. Taussig of Harvard, were asked to join Wilcox and Douglas in sponsoring the statement. This they all agreed to do. As a result of the comments of these men a few changes were made in the text, and at the suggestion of Fisher a paragraph was added pointing out the significance of tariff policy in connection with America's creditor position.

Fisher also made the suggestion that the entire membership of the American Economic Association be circularized, and offered to pay the difference between the cost of this and the estimated cost of the original plan. This was done at a total cost of \$137, of which Fisher contributed \$105. With the clerical assistance of Swarthmore students, the statement was sent out to over 2,500 members of the American Economic Association with a request for signatures. The response was an amazing one. Inside of ten days nearly a thousand signatures had come in, including those of most of the leading figures in American economics. What had started on a simple scale had snowballed into what promised to be a document of national significance.

Wilcox delivered a copy of the text and signatures to President Hoover, Senator Smoot and Congressman Hawley, and gave the material to the press in Washington for release on Monday, May 3. Political opponents of the bill and newspapermen who sensed the news value of the statement took care of the publicity. Senator Pat Harrison had the statement and the list of signers read into the *Congressional Record* of May 5.

Veteran newspapermen, to whom the nation-wide attention that the statement

received seemed to indicate a high-powered publicity campaign, backed by ample appropriations, were almost incredulous when they learned that the protest had been organized and carried through at an expense of less than \$140. This was possible only because of a virtual unanimity of economic opinion on an important issue and the release of the statement to the press at a particularly opportune moment.

The Tariff and American Economists

[from *Congressional Record-Senate*, May 5, 1930]

As in legislative session,

Mr. HARRISON. Mr. President, I ask unanimous consent to have printed in the RECORD and to lie on the table, with the names, a statement signed by 1,028 economists who are known throughout the Nation protesting against the tariff bill.

The VICE PRESIDENT. Without objection, the statement will lie on the table and be printed in the RECORD.

The statement is as follows:

The undersigned American economists and teachers of economics strongly urge that any measure which provides for a general upward revision of tariff rates be denied passage by Congress, or if passed, be vetoed by the President.

We are convinced that increased protective duties would be a mistake. They would operate, in general, to increase the prices which domestic consumers would have to pay. By raising prices they would encourage concerns with higher costs to undertake production, thus compelling the consumer to subsidize waste and inefficiency in industry. At the same time they would force him to pay higher rates of profit to established firms which enjoyed lower production costs. A higher level of protection, such as is contemplated by both the House and Senate bills, would therefore raise the cost of living and injure the great majority of our citizens.

Few people could hope to gain from such a change. Miners, construction, transportation and public utility workers, professional people and those employed in banks, hotels, newspaper offices, in the wholesale and retail trades, and scores of other occupations would clearly lose, since they produce no products which could be protected by tariff barriers.

The vast majority of farmers, also, would lose. Their cotton, corn, lard, and wheat are export crops and are sold in the world market. They have no important competition in the home market. They can not benefit, therefore, from any tariff which is imposed upon the basic commodities which they produce. They would lose through the increased duties on manufactured goods, however, and in a double fashion. First, as consumers they would have to pay still higher prices for the products, made of textiles, chemicals, iron, and steel, which they buy. Second, as

producers, their ability to sell their products would be further restricted by the barriers placed in the way of foreigners who wished to sell manufactured goods to us.

Our export trade, in general, would suffer. Countries can not permanently buy from us unless they are permitted to sell to us, and the more we restrict the importation of goods from them by means of ever higher tariffs the more we reduce the possibility of our exporting to them. This applies to such exporting industries as copper, automobiles, agricultural machinery, typewriters, and the like fully as much as it does to farming. The difficulties of these industries are likely to be increased still further if we pass a higher tariff. There are already many evidences that such action would inevitably provoke other countries to pay us back in kind by levying retaliatory duties against our goods. There are few more ironical spectacles than that of the American Government as it seeks, on the one hand, to promote exports through the activity of the Bureau of Foreign and Domestic Commerce, while, on the other hand, by increasing tariffs it makes exportation ever more difficult. President Hoover has well said, in his message to Congress on April 16, 1929, "It is obviously unwise protection which sacrifices a greater amount of employment in exports to gain a less amount of employment from imports."

We do not believe that American manufacturers, in general, need higher tariffs. The report of the President's committee on recent economics changes has shown that industrial efficiency has increased, that costs have fallen, that profits have grown with amazing rapidity since the end of the war. Already our factories supply our people with over 96 percent of the manufactured goods which they consume, and our producers look to foreign markets to absorb the increasing output of their machines. Further barriers to trade will serve them not well, but ill.

Many of our citizens have invested their money in foreign enterprises. The Department of Commerce has estimated that such investments, entirely aside from the war debts, amounted to between \$12,555,000,000 and \$14,555,000,000 on January 1, 1929. These investors, too, would suffer if protective duties were to be increased, since such action would make it still more difficult for their foreign creditors to pay them the interest due them.

America is now facing the problem of unemployment. Her labor can find work only if her factories can sell their products. Higher tariffs would not promote such sales. We can not increase employment by restricting trade. American industry, in the present crisis, might well be spared the burden of adjusting itself to new schedules of protective duties.

Finally, we would urge our Government to consider the bitterness which a policy of higher tariffs would inevitably inject into our international relations. The United States was ably represented at the World Economic Conference which was held under the auspices of the League of Nations in 1927. This conference adopted a resolution announcing that "the time has come to put an end to the increase in tariffs and move in the opposite direction." The higher duties proposed

in our pending legislation violate the spirit of this agreement and plainly invite other nations to compete with us in raising further barriers to trade. A tariff war does not furnish good soil for the growth of world peace.

ORIGINATORS AND FIRST SIGNERS

Paul H. Douglas, professor of economics, University of Chicago.
Irving Fisher, professor of economics, Yale University.
Frank D. Graham, professor of economics, Princeton University.
Ernest M. Patterson, professor of economics, University of Pennsylvania.
Henry R. Seager, professor of economics, Columbia University.
Frank W. Taussig, professor of economics, Harvard University
Clair Wilcox, associate professor of economics, Swarthmore College.

ADDITIONAL SIGNATURES

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Arizona

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Arkansas

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CORRESPONDENCE

Why Should We Care What Klein and Romero Say About the *Journal of Economic Theory*?

THE FOLLOWING EMAIL EXCHANGE WAS INITIATED BY JOHN QUIGGIN, who is coauthor of one of the *Journal of Economic Theory* articles covered in Daniel Klein's and Pedro Romero's *EJW* article "[Model Building versus Theorizing](#)." The Quiggin article is #43 in the Excel [appendix](#), and was scored by Klein and Romero as passing *Theory of what?* but failing *Why should we care?* and *What merit in your explanation?* The scoring on six sub-tests can be found in the linked worksheet. The exchange is published with Quiggin's and Klein's approval.

JOHN QUIGGIN TO DANIEL KLEIN (19 MAY 2007):

I happened to run across your paper with Romero on *JET*. As it turns out, I have a personal interest since my only article in *JET* (apart from a minor expository note) appeared in 2004, the year you covered. I share some of your concerns about *JET*, and I was particularly pleased to get acceptance for an article which I thought made a pretty clear link between economic theory and improved empirical analysis of questions of practical relevance, as the first paragraph of the concluding comments makes clear.

The tractability of the mean-standard deviation model of choice under risk has made it the standard tool of applied financial analysis. However, the variance is not a particularly attractive index of the riskiness of financial assets, particularly in view of the large body of analysis showing that investors prefer distributions of returns that are skewed to the right. In this paper, the crucial property of the standard deviation as an index of risk, namely its independence of location and scale, has been abstracted and systematically analyzed. The starting point has been the observation that this property is analogous in many important respects to the more restrictive property of constant risk aversion.

I was rather disappointed that you apparently concluded that the paper did not even claim to contribute to improving theory in an area about which economists should care. I'd be interested to know on which of your criteria you claim the paper fails.

DANIEL KLEIN TO JOHN QUIGGIN (3 JUNE 2007):

Pedro and I recognize that we might have mis-scored the paper. We looked again.

It still seems to us that the paper does not say much to answer *Why should we care?* Can you elaborate on a real-world problem that your formulation helps to resolve? Do you maintain that your formulation is a better way to get at the results of traditional investment models? If so, better in what way? Is the betterness mainly a matter of economic insight, or mathematical sophistication? We still find that the motivation is obscure.

We understand that asset price standard deviation may not really get at risk meaningful to investors. Is overcoming that failing the paper's motivation? Does your formulation get at how investors think of risk? If so, we still have trouble seeing that motivation.

JOHN QUIGGIN TO DANIEL KLEIN (4 JUNE 2007):

I suppose our disagreement comes down to tastes in how you write articles. I don't think it's necessary to convince an audience of economists that price determination in asset markets is an important problem, and having suggested a potential improvement interesting enough to satisfy the referees, I'll leave it to the profession to judge whether or not it's worthwhile. You obviously feel these points should be spelt out more in the article.

As a result my interpretation of the message of your article is "*JET* articles aren't written the way we (Klein and Romero) would like". This is fair enough, but fails, for me, to answer your own question, "Why should we (the profession in general) care?"

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