Econ Journal Watch

Scholarly Comments on Academic Economics

Volume 5, Issue 3, September 2008

WATCHPAD

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Econ Journal Watch,
Volume 5, Number 3,
September 2008, pp 269-293.

Editor's Note: Professors Ian Ayres and John J. Donohue have been invited to reply to this article. Their analysis will appear in the January 2009 issue of the journal.

The Debate on Shall-Issue Laws

Carlisle E. Moody[1] and Thomas B. Marvell[2]

Abstract

"Shall issue" right-to-carry concealed weapons laws require authorities to issue concealed-weapons permits, allowing the permit holder to carry a concealed handgun, to anyone who applies, unless the applicant has a criminal record or a history of mental illness. The shall-issue laws are state laws, applicable to all counties within the state.[3] In contrast, states with "may issue" laws allow considerable discretion to the authorities. In may-issue states, authorities typically require that the applicant demonstrate a particular need for a concealed weapons permit, and self-defense usually is not deemed sufficient. Consequently, shall-issue states are much more permissive of individual freedom to carry concealed handguns.

In 1997 John Lott and David Mustard published, "Crime, Deterrence and Right-to-Carry Concealed Handguns" in the *Journal of Legal Studies*. They found that shall-issue states had lower violent crime rates, presumably because the laws result in more people carrying concealed weapons. Criminals might be deterred by the greater likelihood of others being armed, and of arms being concealed. Lott and Mustard's article created a furor and the debate continues. Much of this debate takes place in op-ed columns, letters to editors, internet chat rooms, and web logs. In this article we concentrate on the academic debate. We review the main threads of the discussion in the literature and extend the debate with our own statistical analyses. In particular, we extend the investigation of influential work in *Stanford Law Review* by Ian Ayres and John J. Donohue III (2003a, 2003b), who, contrary to Lott and Mustard, claim to find that shall-issue laws actually lead to an overall increase in crime. The new statistical analysis contained in the present article finds that shall issue laws

1 Professor of Economics, College of William and Mary, Williamsburg, Virginia 23187, cemood@wm.edu

2 Director of Justec Research, Williamsburg, Virginia 23185, marvell@cox.net

3 Except for Philadelphia, which was initially exempt from Pennsylvania's shall-issue law.

are generally beneficial. Purists in statistical analysis object with some cause to some of methods employed both by Ayres and Donohue, by us, and by the literature in general. But the new investigation presented here upgrades Ayres and Donohue in a few significant ways, so, at least until the next study comes along, our paper should neutralize Ayres and Donohue's "more guns, more crime" conclusion.

The State of the Debate

In this paper, when we use the term "significant" to describe results of statistical investigation, the term means statistically significant at the 0.10 level.

The original study by Lott and Mustard (1997) used pooled time-series and cross-section data across all the counties in the United States for the years 1977 to 1992. They used the fixed-effects panel data model, which corrects for possible unobserved heterogeneity across counties. They also included time dummies, arrest rates, several income variables and a host of detailed demographic control variables. The target variable was a dummy variable that took the unit value for those counties in shall-issue states during or after the first full year of implementation, zero otherwise.[4] The primary set of results was reported in Lott and Mustard's Table 3 (1997, 20-23). The estimated coefficient on the shall-issue dummy variable was negative and significant for all the violent crimes (murder, rape, robbery, and assault), positive and significant for larceny and auto theft, and not significant for burglary. The estimated coefficients were also large enough numerically to cause substantial reductions in the estimated costs of crime. Lott and Mustard also offered a corresponding state-level model (1997, 27). They found that all violent crime categories were significantly reduced by shall-issue laws, again with large implied reductions in the costs of crime. They then engaged in a series of robustness tests all of which confirmed the basic finding that right-to-carry laws reduced violent crime.

Contrary findings appeared very quickly. Black and Nagin (1998) noted that Lott and Mustard, by using a single dummy variable for the shall-issue law, assumed the same effect for all states and all years. They extended the model to allow for separate dummies for each state and found that the results differed across states with some states significantly positive, some significantly negative, and some showing no effect. They also estimated a first-differenced model using pre- and post-law dummy variables for the five years before and after the adoption of the shall-issue law. Finally, they estimated a model with individual state trends as additional controls (but with a single shall-issue dummy). They concluded that the Lott and Mustard results were fragile and that, overall, the shall-issue law had no significant effect on crime.

4 Lott and Mustard also tried a shall-issue variable that took a fractional value indicating the proportion of the year the law was in effect in its first year, the results were unchanged.

Lott (1998) responded in the same issue of the *Journal of Legal Studies*. He pointed out that Black and Nagin ignored the models in which Lott and Mustard (1997) multiplied the shall-issue law dummy by the state population. Thus their criticism that the Lott and Mustard model relied on a single dummy variable for all states was misplaced. Lott's most telling criticism, however, concerned Black and Nagin's use of pre- and post-law dummy variables. If crime rates were generally increasing prior to the passage of the law and falling after, the series describing an "inverted V," as Lott and Mustard reported (1997, 35), the coefficients on the dummy variables for the two or three years before the law could be expected to be approximately the same as the corresponding coefficients for the two or three years after, implying no effect of the law, when the law in fact had a very significant effect on the crime rate.[5]

Black and Nagin also criticized Lott and Mustard for not including individual state trends as controls for potentially omitted variables. However, as Lott pointed out, the original paper had reported first differenced models, one of which included state dummies. In such a model, the state dummies are equivalent to individual state trends. Lott also argued that the original paper had allowed differential impacts across states in the sense that individual analyses were done for Pennsylvania and Oregon where data on the number of permits were available. Although Lott responded to each of the points raised by Black and Nagin, the issue remained unresolved.

At this point, the broad outline of the subsequent debate was already in place. Future work would have to address the problem of differing before and after trends (including the inverted V), allow for individual state trends, and allow the law to have differing impacts across states.

Two years later, Lott (2000) extended the sample to 1994 and introduced spline models to address the inverted V problem. Lott examined many alternative versions of the model and determined that the results were very robust. Shall-issue laws were found to significantly reduce violent crime.

In 2001 the *Journal of Law and Economics* published the proceedings of a conference on shall-issue laws. In that volume, several studies confirmed the hypothesis that shall-issue laws reduce crime. One year later, 2002, in the second edition of *More Guns Less Crime*, Lott extended the sample to 1996 and re-estimated the spline models, along with a host of alternative specifications. Shall-issue laws were again found to reduce violent crime.

At this point in the debate, the weight of evidence was firmly on the side of those claiming that shall-issue laws reduce violent crime. However, Ayres and Donohue (2003a) significantly shifted the debate. They noted that the aggregate model, which uses a single dummy for all states, was possibly susceptible to selec-

5 Note that this criticism of the dummy variable method only applies to short periods after the passage of the law. If the law remains in force for many years and crime falls continuously, the average effect estimated by the dummies will eventually be negative.

tion bias in the sense that early-adopting states are in the data set for many years and late-adopting states are barely represented. Thus, the aggregate model with a single dummy or trend for all states, when extended over many years, is eventually reflecting only a few states, not the entire country. For example, the aggregate model of the 24 states that have passed shall-issue laws since 1977, when extended out 14 years to 1990, includes only two states, Maine and Florida, with shall-issue the full period, giving them a dominant role in the model. Those two states may not be representative of the country as a whole.

Ayres and Donohue also claimed that the original 1997 Lott and Mustard paper, which was based on 1977-1992 data, included only states that adopted shall-issue laws in the 1980s when crime peaked because of the emergence of crack cocaine. Thus, the post-crack-wave fall in crime was being reflected in the negative coefficients on the shall-issue dummy variables. Ayres and Donohue argue that by extending the county data set to 1997, they are allowing the states that passed the law after the crack wave was over, to help determine the effect of the law and mitigate the crack-wave effect. However, Lott and Mustard estimated, but did not report, a model including the price of cocaine. They found that the results were not affected. Also, the presence of time dummies should mitigate the crack-wave effects unless the shall-issue states are more affected than other states. Finally, in his book Lott extended the sample to 1996 and included states passing laws after the crack wave, with no change in the general conclusions. The single additional year added by Ayres and Donohue is unlikely to have a significant effect. Nevertheless, the Ayres and Donohue criticism points to the need to control for the effects of crack cocaine.

Ayres and Donohue (2003a) estimated a model with individual state trends, individual state post-law dummies, and individual state post-law trends. This model, dubbed the "hybrid" model, is a generalization of the Lott spline model. The spline model assumes that the before and after trends look like a V or inverted V, thereby disallowing an immediate impact of the law. The hybrid model introduces a dummy variable that can be interpreted to measure the immediate impact of the law and a post-law trend which captures the long run effects. Ayres and Donohue concluded, using the hybrid model that, "For every crime type, there are more states where shall-issue laws produce a positive and statistically significant coefficient than states that produce a negative and statistically significant coefficient" (1232). They also computed the net effect of the law across all states. They estimated, "an increased cost ranging between $3 and $524 million" (1284). Thus, Ayres and Donohue present evidence that shall-issue laws increase crime.

However, Ayres and Donohue limit their analysis to the first five years after the law's passage. This five-year span has the effect of emphasizing the impact of the dummy variable and downplaying the impact of the long-run post-law trend. Since they find that shall-issue generally increases crime in the short run but decreases crime in the long run, the five-year span directly affects the overall result. We can show this by calculating the short and long run benefits and costs

using Ayres and Donohue's estimated coefficients.[6] Their results imply a short run increase of $4.23 billion in crime costs from the dummies, with an accompanying decrease of $1.25 billion per year from the trends. Thus, as Table 1 shows, the costs (negative values) increase for the first three years then start to decline in year four. Beginning in year six, the long run benefits (positive values) exceed the short run costs and the benefits grow continuously from then on. Ayres and Donohue stop their calculations at five years, ignoring the $1.25 billion per year reduction in crime costs in all further years. Since no shall-issue laws have been repealed and some states have had these laws on the books for decades (e.g., New Hampshire has had a shall-issue law since 1923), extrapolating the results to ten years is reasonable. Also, as of 2000 the last year of our data set, 14 of the 25 states that passed shall-issue laws within the sample period have had them on the books for six years or longer while nine of those states have had a shall-issue law for ten years or more (see Table 9 below.). Finally, as of 2008, *all* of the states that have ever passed shall-issue laws have had the law on their books for more than ten years—it is not as though the laws tend to come and go. We grant that an analysis should not allow an "eternity" of the trend effect determine the results. Rather, the analysis should extend out for some appropriate finite span. Based on the data and historical pattern of retaining shall-issue laws, we think that a ten-year span is appropriate, and that five years is certainly too short.

Discounting with a social discount rate between zero and four percent does not change the results. Using a social discount rate between .041 and .17 delays the appearance of net positive benefits by one year.[7]

The Ayres and Donohue article was followed in the same issue by a response by Plassmann and Whitley (2003), as well as a rejoinder by Ayres and Donohue (2003b). Plassmann and Whitley said that counting positive versus negative coefficients (by state) is not enough. Using Ayres and Donohue's own estimates from the aggregate model, they show that crime declines after shall-issue laws are

6 Their coefficients are taken from Ayres and Donohue (2003a, 1310-1311, Appendix Table 7). They are also available on Ayres' website, http://islandia.law.yale.edu/ayers/indexempirical.htm.

7 Ayres and Donohue's implied results were calculated as follows. Their coefficients on the individual state dummy and trend for each crime in each state, which represent percent changes, were multiplied by the level of each crime in each state in the year of passage, to get the change in crime due to the passage of the shall-issue law. The changes in crime were multiplied by the cost of each crime from Miller, Cohen and Wiersema (1996) converted to real 2000 dollars to get the implied change in the costs of each crime. The implied costs were summed across crimes to get the implied change in the total cost of crime for each state and then summed across states to get the implied change for the US as a whole. The spreasheet showing these calculations is available at C.E. Moody's website (link).

Table 1: Ayres and Donohue Implied Benefits of the Shall-Issue Law

Discount Rate	0			.025			.05		
Year	Dummy	Trend	Cumulative Effect	Dummy	Trend	Cumulative Effect	Dummy	Trend	Cumulative Effect
1	-4.23	1.25	-2.98	-4.23	1.25	-2.98	-4.23	1.25	-2.98
2	-4.23	2.50	-4.71	-4.13	2.44	-4.67	-4.03	2.38	-4.63
3	-4.23	3.75	-5.19	-4.03	3.57	-5.12	-3.84	3.40	-5.06
4	-4.23	5.00	-4.42	-3.93	4.64	-4.41	-3.65	4.32	-4.40
5	-4.23	6.25	-2.40	-3.83	5.66	-2.58	-3.48	5.14	-2.74
6	**-4.23**	**7.50**	**0.87**	**-3.74**	**6.63**	**0.31**	-3.31	5.88	-0.17
7	-4.23	8.75	5.39	-3.65	7.55	4.21	**-3.16**	**6.53**	**3.20**
8	-4.23	10.00	11.16	-3.56	8.41	9.06	-3.01	7.11	7.30
9	-4.23	11.25	18.18	-3.47	9.23	14.82	-2.86	7.61	12.05
10	-4.23	12.5	26.45	-3.39	10.01	21.35	-2.73	8.06	17.38

Note: costs are negative, benefits are positive.

passed. In their rejoinder, Ayres and Donohue (2003b) reiterate that their F-tests rejected the null hypothesis that the effect of the laws was the same across states, rejecting the aggregate model. Thus, they contend, the results of the aggregate model, presented by Ayres and Donohue (2003a) and used by Plassmann and Whitley, were originally presented only to show how wrong one can be when combining effects across states.

In 2004, the National Research Council of the National Academies produced a meta-study on gun violence that concluded with respect to shall-issue laws that "with the current evidence it is not possible to determine that there is a causal link between the passage of right-to-carry laws and crime rates" (National Research Council 2004, 150). However, the Committee did some independent analyses that indicated that shall-issue laws reduce murder (269-70).

In Table 2 we list the key research items of the debate. In our judgment, the weight of evidence—particularly that of peer-review—indicates that shall-issue laws reduce crime. Although Ayres and Donohue (2003a, 1397) conclude that "the best evidence suggests overall small increases in crime associated with the adoption of shall-issue laws," that conclusion relies on ignoring their own implied long-run reductions in crime. In the next section we offer a fresh statistical analysis based on the method of Ayres and Donohue, but our investigation improves the method and extends the data through 2000 (Ayres and Donohue's data was through 1997).

Table 2: Academic Evidence on the Relationship between Shall-Issue Laws and Crime

Shall-Issue Reduces Crime
Refereed journal articles and books
J.R. Lott and D.B. Mustard. 1997. Crime, deterrence, and right-to-carry concealed handguns. *Journal of Legal Studies* 26: 1-68. _____1998. The concealed handgun debate. *Journal of Legal Studies* 27: 221-243. W.A. Bartley and M.A. Cohen. 1998. The effect of concealed weapons laws--an extreme bound analysis. *Economic Inquiry* 36: 258-265. S.G. Bronars and J.R. Lott 1998. Criminal deterrence, geographic spillovers, and the right to carry concealed handguns. *American Economic Review* 88: 475-479. B.L. Benson and B.D. Mast. 2001. Privately produced general deterrence. *Journal of Law and Economics* 44: 725-746. C.E. Moody. 2001. Testing for the effects of concealed weapons laws: Specification errors and robustness. *Journal of Law and Economics* 44:799-813. D.B. Mustard. 2001. The impact of gun laws on police deaths. *Journal of Law and Economics* 44:635-657. D.E. Olsen and M.D. Maltz. 2001. Right-to-carry concealed weapons laws and homicide in large U.S. counties: the effect on weapons types, victim characteristics, and victim-offender relationships. *Journal of Law and Economics* 44:747-770. F. Plassmann and T. N. Tideman. 2001. Does the right to carry concealed handguns deter countable crimes? only a count analysis can say. *Journal of Law and Economics*, 44, pp. 771-798. J.R. Lott. 1998, 2001. *More guns, less crime : understanding crime and gun-control laws*. Chicago, University of Chicago Press. E. Helland and A. Tabarrok. 2004. Using Placebo Laws to Test 'More Guns, Less Crime.' *Advances in Economic Analysis & Policy* 4 : Issue. 1, Article 1.
Non-Refereed
F. Plassmann and J. Whitley. 2003. Confirming 'more guns, less crime.' *Stanford Law Review* 54: 1313-1369. J. R. Lott and W.M. Landis. 1999, 2001, 2003. Multiple victim public shootings, bombings and right-to-carry concealed handgun laws: contrasting private and public law enforcement. Link. Published as Chapter 6 of J. R. Lott. *The bias against guns*. Washington, DC, Regnery.
Unpublished
J. R. Lott. 2004. Right-to-carry laws and violent crime revisited: clustering, measurement error and state-by-state breakdowns. Working paper, American Enterprise Institute.

Shall-Issue Increases Crime
Referred journal articles: none.
Non-Refereed
I. Ayres and J.J. Donohue. 2003. Shooting down the more guns, less crime hypothesis. *Stanford Law Review* 54: 1193-1312. _____2003. The latest misfires in support of the 'more guns, less crime' hypothesis. *Stanford Law Review* 54: 1371-1398. J.J. Donohue. 2003. The impact of concealed carry laws. In J. Ludwig and P.J. Cook (eds.). *Evaluating Gun Policy*, Washington, DC: The Brookings Institution, 287-325.
Unpublished: none.
Shall-Issue Has No Significant Effect on Crime
Refereed
D.A. Black and D.S. Nagin. 1998. Do right-to-carry laws deter violent crime? *Journal of Legal Studies* 27: 209-219. H. Dezhbakhsh and P.H. Rubin. 1998. Lives saved or lives lost--the effects of concealed-handgun laws on crime. *American Economic Review* 88: 468-474. J. Ludwig. 1998. Concealed-gun-carrying laws and violent crime: Evidence from state panel data. *International Review of Law and Economics* 18: 239-254. M.V. Hood and G.W. Neeley. 2000. Packin' in the hood?: examining assumptions of concealed-handgun research. *Social Science Quarterly* 81: 523-537. G. Duwe, T. Kovandzic, and C.E. Moody. 2002. The impact of right-to-carry concealed firearm laws on mass public shootings. *Homicide Studies* 6: 271-296. T. Kovandzic and T.B. Marvell. 2003. Right-to-carry concealed handguns and violent crime: crime control through gun decontrol? *Criminology and Public Policy* 2: 363-396. National Research Council. 2005. *Firearms and Violence: A Critical Review*. Committee to Improve Research Information and Data on Firearms. Charles F. Wellford, John V. Pepper, and Carol V. Petrie, editors, Washington, DC: The National Academies Press. Kovandzic, T. V., T.B. Marvell, and L.E, Vieraitis. 2005. The Impact of 'Shall-Issue' Concealed Handgun Laws on Violent Crime Rates. *Homicide Studies*, 10: 292-323.
Non-refereed: none. **Unpublished**: none.

Shall-Issue Laws Revisited

We apply the Ayres and Donohue hybrid model to the county data set extended through 2000, encompassing three additional years of data and all addi-

tional law enactments.[8] Also, we modify their model by adding two new variables. Ayres and Donohue argue that the crack wave gave rise to a spurious correlation in Lott and Mustard's statistical analysis based on data from 1977 to 1992—an omitted variable problem. Fortunately, a measure of crack cocaine activity has been developed by Fryer et al (2005). The measure is derived from cocaine arrests, cocaine-related emergency room visits, cocaine-induced drug deaths, newspaper reports, and DEA drug busts. The inclusion of this variable should allay concerns of a spurious correlation with the crack wave.

Table 3: Variable Names, Definitions, and Means

Ratmur	murder rate per 100,000	5.307
rattrap	rape rate per 100,000	20.637
Ratrob	robbery rate per 100,000	45.925
Rataga	aggravated assault rate per 100,000	196.571
Ratbur	burglary rate per 100,000	758.450
Ratlar	larceny rate per 100,000	1777.471
Rataut	auto theft rate per 100,000	173.088
Shallf	shall-issue dummy	0.278
Crack	crack cocaine index	0.878
Prison	prison population per capita	0.003
Aovio	arrest rate for violent crime	74.247
Aopro	arrest rate for property crime	30.366
execrate	execution rate	0.002
unemprt	unemployment rate	6.097
Rpcpi	real per capita persional income ($1000)	11.408
Rpcui	real per capita unemployment insurance	61.923
Rpcim	real per capita income maintenance	182.912
Rpcrpo	real per capita retirement payments	1619.632
Povrate	poverty rate	14.025
Popc	county population	7.895
ppbm1019	percent population black males 10-19	0.008
ppbf1019	percent population black females 10-19	0.008
ppbm2029	percent population black males 20-29	0.007
ppbf2029	percent population black females 20-29	0.008
ppbm3039	percent population black males 30-39	0.007
ppbf3039	percent population black females 30-39	0.007
ppbm4049	percent population black males 40-49	0.005
ppbf4049	percent population black females 40-49	0.006
ppbm5064	percent population black males 50-64	0.006
ppbf5064	percent population black females 50-64	0.007

8 The dataset is available at http://www.johnlott.org. All the studies cited above make use of this dataset (though differing years of it).

ppbm65o	percent population black males 65 and over	0.006
ppbf65o	percent population black females 65 and over	0.007
ppwm1019	percent population white males 10-19	0.070
ppwf1019	percent population white females 10-19	0.071
ppwm2029	percent population white males 20-29	0.062
ppwf2029	percent population white females 20-29	0.063
ppwm3039	percent population white males 30-39	0.063
ppwf3039	percent population white females 30-39	0.064
ppwm4049	percent population white males 40-49	0.053
ppwf4049	percent population white females 40-49	0.054
ppwm5064	percent population white males 50-64	0.065
ppwf5064	percent population white females 50-64	0.067
ppwm65o	percent population white males 65 and over	0.063
ppwf65o	percent population white females 65 and over	0.067
ppnm1019	percent population neither males 10-19	0.003
ppnf1019	percent population neither females 10-19	0.003
ppnm2029	percent population neither males 20-29	0.003
ppnf2029	percent population neither females 20-29	0.002
ppnm3039	percent population neither males 30-39	0.003
ppnf3039	percent population neither females 30-39	0.003
ppnm4049	percent population neither males 40-49	0.002
ppnf4049	percent population neither females 40-49	0.002
ppnm5064	percent population neither males 50-64	0.002
ppnf5064	percent population neither females 50-64	0.002
ppnm65o	percent population neither males 65 and over	0.002
ppnf65o	percent population neither females 65 and over	0.002

Our second novel variable is a lagged dependent variable, which is included to capture effects through time. An equation with a lagged dependent variable is a first-order difference equation, which can display patterns of growth, decline, or oscillation. The Ayres and Donohue model is completely static. It suffers from potentially serious omitted variable bias if the lagged dependent variable is significant. In addition to these two variables, we include all the variables used by Ayres and Donohue, including individual state trends, county dummies, and year dummies. Like Ayres and Donohue we disaggregate the effect of the shall-issue law to the state level. The target variables are the individual state shall-issue dummy variables and corresponding post-law trends. The shall-issue dummies take the unit value in the first full year following the passage of a shall-issue law. The post-law trends are zero up to the year of passage with the trend starting in the first full year after passage. We use Lott's coding.[9] The sanction variables are the arrest rate for violent crime, the arrest rate for property crime, the per capita prison population, and, in the case

9 There is some disagreement as to the exact dates of the passage of the various shall-issue laws. In preliminary analyses we used both the Ayres and Donohue dates and the Lott dates. The results were the same. Here we use the Lott dates.

of murder, the execution rate.[10] The control variables are those used in previous analyses. The variable names, definitions, and means are presented in Table 3.

Again, shall-issue laws are state laws, applicable to all counties within the state.[11] Consequently, all counties within a state have the same values for the shall-issue dummy and post-law trend, implying that the errors are likely to be correlated across counties within states. This causes the usual standard errors to be underestimated and the t-ratios to be overestimated, potentially causing spurious correlation between the shall-issue laws and crime rates (Moulton 1990). To avoid that problem, we use heteroskedastic-consistent ("robust") standard errors corrected for clustering within states.[12] Because of the large number of zeroes in the murder and rape variables, 39 percent and 21 percent respectively, we add a small constant, .10, to these variables before taking logs. This changes the mean, but not the variance and therefore does not create measurement error. We recognize that there are good theoretical reasons for using methodologies specifically developed for count data, especially for relatively rare crimes such as murder and rape (see Plassmann and Tideman 2001). However, nearly all the articles in this literature, including Ayres and Donohue, use ordinary least squares and we continue the practice here. Also, the large number of observations (over 65,000) combined with the large number of variables (over 160) makes nonlinear procedures such as the negative binomial computationally difficult to carry out. Adding a small constant before taking logs is also standard practice. If we do not add this constant, all counties with zero crimes are dropped from the analysis. This has the effect of underestimating the effect of the shall-issue law because only positive crime rate counties are included, therefore the policy cannot reduce crime to zero. The coefficient on any crime policy variable is already biased toward zero in such cases because no policy can reduce the crime rate to a negative number. After inspecting Ayres and Donohue's paper, results and do files, and attempting to replicate their results, our best guess as to their treatment of zeros is that they used the variables as originally defined by Lott, who included a small constant. Therefore, it would seem that our treatment of the zeros should not be a significant source of discrepancy between our results and those of Ayres and Donohue.

The results with respect to the interesting control variables are presented in Table 4.[13]

10 The arrest rate is the clearance rate (arrests/crimes). The arrest rate might be endogenous in the crime equation. For that reason we dodge the simultaneity issue by using the arrest rate for all violent crimes in the murder, rape, robbery, and assault equations and the arrest rate for all property crime in the burglary, larceny, and auto theft equations. Doing so also alleviates the problem of zero arrest rates causing the county to be dropped from the data set.

11 Except for Philadelphia, which was initially exempt from Pennsylvania's shall-issue law.

12 Neither Ayres and Donohue nor Lott and Mustard corrected their standard errors for clustering.

13 To conserve space, we do not report the coefficients on the 36 demographic variables, the individual state trends, the year dummies, and the individual county intercepts. The coefficients on the shall-issue law shift dummy and post-law trend variables are presented in Tables 5a, 5b, 7a, and 7b below. Complete results, data, and Stata programs are available at C.E. Moody's website (link). We do not compute equa-

Table 4: Estimated Coefficients

	murder		rape		robbery		assault	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
Crack	0.0320	1.45	0.0447	1.57	**0.0709**	3.99	**0.0200**	2.60
Prison	**-91.7578**	-3.80	-50.0644	-0.43	**-101.1922**	-5.09	-14.6002	-1.13
Aovio	**-0.0004**	-3.06	**-0.0006**	-4.47	**-0.0009**	-5.94	**-0.0008**	-4.85
unemprt	-0.0143	-1.41	**-0.0140**	-1.29	-0.0008	-0.07	-0.0010	-0.21
Rpcpi	0.0016	0.29	**-0.0095**	-1.93	0.0025	0.34	-0.0044	-0.88
Rpcui	-0.0341	-0.16	-0.0614	-0.17	0.0002	0.68	**-0.2767**	-2.15
Rpcim	0.1289	0.44	0.5254	0.88	-0.0001	-0.30	**-0.2309**	-1.70
Rpcrpo	-0.0235	-0.21	0.0523	0.25	0.0000	0.48	**0.1293**	1.87
Povrate	-0.0005	-0.10	0.0085	0.90	0.0020	0.52	-0.0003	-0.14
Popc	**0.0006**	1.65	**-0.0040**	-4.77	**-0.0005**	-1.98	0.0001	0.25
Y(t-1)	0.0130	1.51	**0.1241**	4.24	**0.1104**	6.38	**0.3663**	13.81
R-square	0.65		0.66		0.85		0.83	
N	54169		54148		58844		58830	

	burglary		larceny		auto	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
Crack	**0.0284**	3.56	**0.0284**	3.56	**0.0634**	3.90
Prison	**-38.9346**	-2.61	**-38.9346**	-2.61	**-85.4660**	-5.72
Aovio	**-0.0005**	-5.95	**-0.0005**	-5.95	**-0.0006**	-5.10
unemprt	0.0077	1.46	0.0077	1.46	-0.0037	-0.41
Rpcpi	**-0.0078**	-2.91	**-0.0078**	-2.91	**0.0105**	1.87
Rpcui	0.0561	0.59	0.0561	0.59	0.3479	1.07
Rpcim	-0.0169	-0.15	-0.0169	-0.15	-0.1613	-0.39
Rpcrpo	0.0834	1.56	0.0834	1.56	0.0853	0.87
Povrate	0.0006	0.29	0.0006	0.29	0.0015	0.56
Popc	**-0.0006**	-3.29	**-0.0006**	-3.29	**-0.0011**	-1.80
Y(t-1)	**0.3656**	6.03	**0.3656**	6.03	**0.2788**	4.81
R-square	0.86		0.87		0.83	
N	61550		61550		61551	

Notes: The dependent variable is the crime rate logged. Because of the relatively large number of zeroes in the murder and rape counts we added .10 to the per capita rates before taking logs. Y(t-1) is the lagged dependent variable. Coefficients in bold are significantly different from zero at the .10 level, two-tailed. We suppress the estimated coefficients on the 36 demographic variables, the year dummies, the individual state trends, and the 24 shall-issue dummies and post-law trends. Complete results are available at C.E. Moody's website (link). The execution rate was not significant in the murder equation and was dropped. The overall results were unchanged.

tions for total crime, violent crime, or property crime because these aggregates merely count the various subcategories. Therefore, because there are so many more assaults than murders, rapes, or robberies, violent crime is virtually indistinguishable from assault. Similarly, property crime and total crime are dominated by larceny, the most common type of index crime.

The crack variable is significant and positive in all of the crime equations, except murder and rape, indicating that the crack wave had significant effects on most crime categories. Of the sanctioning variables, prison population has a significantly negative effect on murder, robbery, burglary, larceny, and auto theft. Arrest rates have negative and significant impacts for all crimes. Real per capita personal income (rpcpi) is negative and significant in the rape, burglary, and larceny equations and positive in the auto theft equation. Real per capita unemployment insurance payments (rpcui), real per capita welfare payments (rpcim), and real pension payments are significant only in the assault equation. The poverty rate is not significant in any of the crime equations. The population level (popc) is negatively related to rates of rape, robbery, burglary, and larceny and positively related to the murder rate. The lagged dependent variable is significant in all of the equations except murder, indicating the importance of dynamic effects in most crime categories. Although we suppress the thirty-six demographic variables for readability, they are significant as groups and are therefore retained in the regressions. The year dummies and individual state trends are also jointly significant.[14]

The results with respect to the state-specific dummy variables are presented in Table 5a and Table 5b.

14 In the rape, robbery, assault, and auto theft equations, we tested for and found significant negative autocorrelation. The effect of negative autocorrelation on the standard errors and t-ratios is unknown. Because we use heteroskedastic consistent standard errors corrected for clustering on states, we partially correct for autocorrelation. We believe that our hypothesis tests are valid.

Table 5a: Shall-issue Dummy Coefficients: Violent Crime

	Murder		Rape		Robbery		Assault	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
AK	0.125	1.38	**-0.517**	-4.31	-0.024	-0.31	0.038	0.89
AZ	**0.264**	6.2	-0.064	-0.82	**0.171**	4.70	**0.053**	1.97
AR	0.048	1.04	0.031	0.31	**-0.073**	-2.19	**0.099**	4.56
FL	-0.089	-1.22	**-0.181**	-2.87	**0.141**	1.88	**0.073**	2.82
GA	**-0.200**	-4.28	-0.052	-0.89	**-0.151**	-3.62	**-0.052**	-2.67
ID	**0.978**	23.04	**0.302**	2.24	0.093	1.20	0.030	0.99
KY	0.046	0.90	**-0.301**	-3.98	**0.277**	5.29	**0.160**	6.36
LA	**0.381**	6.15	0.113	1.62	**0.287**	4.44	0.056	1.58
ME	**0.460**	9.70	0.121	1.64	**-0.144**	-3.08	**-0.151**	-5.15
MS	0.067	1.21	-0.034	-0.47	**0.143**	3.38	**0.115**	4.28
MT	0.008	0.22	0.233	1.56	**-0.430**	-6.90	**-0.210**	-7.92
NV	**0.551**	12.65	**0.151**	1.99	**0.107**	2.48	**0.174**	5.24
NC	0.009	0.20	0.053	0.41	**0.090**	2.11	**0.102**	3.61
OK	**0.090**	2.21	0.060	1.14	-0.062	-1.33	0.006	0.27
OR	**-0.213**	-5.18	0.025	0.30	**-0.240**	-4.14	**0.049**	1.91
PA	-0.022	-0.51	0.064	1.19	**-0.061**	-1.87	**-0.051**	-2.27
PH	-0.024	-0.63	**-0.344**	-5.41	**-0.060**	-1.77	**-0.213**	-7.70
SC	0.050	1.05	-0.126	-1.54	-0.052	-0.98	0.055	1.60
TN	-0.026	-0.69	**-0.154**	-2.51	**-0.091**	-2.85	0.046	1.48
TX	-0.055	-1.16	0.103	0.44	0.046	0.85	0.024	0.88
UT	**0.100**	1.66	-0.034	-0.38	**0.078**	1.74	**0.214**	6.38
VA	0.030	0.60	**0.107**	1.97	-0.054	-1.39	**-0.040**	-2.09
WV	**0.285**	6.44	0.100	1.47	-0.064	-1.51	**-0.075**	-2.70
WY	**-0.266**	-3.92	-0.003	-0.02	**0.512**	7.69	-0.042	-1.18
US	0.006	0.06	-0.007	0.01	0.008	0.10	**0.031**	6.36
negative	8		11		13		8	
significant	3		5		8		7	
positive	16		13		11		16	
significant	8		3		9		9	

Notes: Coefficients in bold are significant at the .10 level. The test statistics for the US weighted average are F-ratios corresponding to the null hypothesis that the weighted average is zero. PH is Philadelphia.

For all crimes except robbery and burglary, the number of states[15] with a positive effect as measured by the coefficient on the dummy variable is greater than those with a negative effect. Also, the population-weighted average across all states is positive for all crimes except rape and burglary and significantly positive for assault and auto theft. We computed the harm-weighted long run effect of these laws by multiplying the implied change in the number of crimes by the cost to the victims of each type of crime. The victim costs are taken from Miller, Cohen and Wiersema (1996, Table 2) and are adjusted to real 2000 dollars using the consumer price index (cpi-u-rs). The relevant per-victim costs are as follows:

15 Because Philadelphia was excluded from Pennsylvania's shall-issue law until 1995, we treat it as a separate jurisdiction. However, for convenience, we still refer to "states" when counting jurisdictions.

murder $3.44 million; rape, $101,790; robbery $9.360; assault $10,998; burglary $1,638; larceny $433; auto theft $4, 329. The short-run cost associated with the passage of the shall-issue law is shown in Table 6 where we use positive values to indicate the costs of crime and negative values to indicate that crime and its related costs have been reduced.

Table 5b: Shall-issue Dummy Coefficients: Property Crime

	Burglary		Larceny		Auto Theft	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
AK	-0.021	-0.36	-0.044	-0.86	**-0.133**	-2.38
AZ	**0.073**	3.71	**0.058**	2.69	**0.175**	3.72
AR	**-0.075**	-4.27	0.004	0.24	-0.023	-0.72
FL	0.005	0.18	-0.014	-0.58	**0.154**	2.71
GA	**-0.124**	-5.11	**-0.081**	-4.65	**-0.167**	-4.74
ID	-0.015	-0.44	**0.070**	2.04	0.094	1.59
KY	-0.025	-1.03	**-0.079**	-4.06	**0.075**	1.70
LA	0.043	1.59	**0.052**	2.32	**0.235**	5.49
ME	0.007	0.32	**0.051**	3.01	0.077	1.61
MS	-0.031	-0.97	0.006	0.28	-0.044	-1.24
MT	**0.117**	3.79	0.029	1.01	0.023	0.43
NV	**0.159**	5.10	**0.107**	3.20	**0.142**	4.79
NC	-0.026	-0.83	**0.061**	2.29	**0.201**	5.66
OK	0.013	0.46	0.041	1.39	-0.029	-0.69
OR	**-0.084**	-2.73	0.016	0.64	-0.016	-0.31
PA	-0.021	-1.06	0.012	1.15	0.004	0.17
PH	**-0.107**	-2.76	**-0.235**	-8.04	-0.047	-1.18
SC	**-0.052**	-1.90	-0.025	-1.14	0.074	1.60
TN	**-0.036**	-1.68	**-0.037**	-1.71	-0.047	-1.41
TX	**0.073**	2.12	0.050	1.49	0.078	1.59
UT	**0.079**	1.96	-0.052	-1.58	**0.188**	4.55
VA	**-0.072**	-2.88	-0.012	-0.52	**-0.101**	-2.47
WV	**0.063**	2.06	**0.078**	4.26	**-0.093**	-2.12
WY	**0.145**	5.18	**0.071**	2.31	**0.165**	2.91
US	-0.010	0.30	0.009	0.39	**0.050**	6.47
negative	13		9		10	
significant	8		4		4	
positive	11		15		14	
significant	7		8		8	

Note: See notes to Table 5a.

The short run cost is $1.2 billion. All crime categories, except rape and burglary, show positive costs due to increases in crime.[16]

Table 6: Short Run Costs and Benefits, Millions of 2000 Dollars

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto	Total
AK	11.69	-13.68	-0.18	0.93	-0.15	-0.34	-1.59	-3.32
AZ	383.40	-9.29	10.40	11.02	5.73	3.67	29.71	434.65
AR	42.60	2.93	-2.12	9.84	-3.21	-0.17	-1.12	48.75
FL	-419.15	-111.36	56.84	55.88	9.05	0.69	59.40	-348.64
GA	-551.77	-16.46	-24.31	-14.03	-18.74	-6.43	-29.36	-661.10
ID	92.12	8.41	0.13	0.72	-0.41	0.66	0.50	102.12
KY	15.44	-13.95	8.10	10.09	-1.12	-1.60	1.28	18.25
LA	977.93	19.70	31.62	14.60	4.09	3.74	26.83	1078.52
ME	44.72	2.06	-0.38	-2.34	0.37	0.80	0.80	46.02
MS	43.52	-2.49	2.31	4.22	-2.48	-0.36	-0.92	43.79
MT	0.37	2.68	-0.40	-1.21	0.48	0.08	-0.21	1.79
NV	306.86	14.27	4.96	15.20	4.88	2.23	6.34	354.75
NC	21.77	12.42	10.72	32.54	-4.47	5.93	19.16	98.07
OK	122.97	8.92	-2.18	1.08	0.16	1.25	-2.88	129.30
OR	-79.23	3.37	-9.19	4.60	-5.31	0.10	-1.78	-87.44
PA	-21.16	14.02	-4.43	-9.42	-1.39	1.45	1.02	-19.92
PH	-35.87	-27.03	-7.65	-15.82	-2.37	-4.45	-2.34	-95.52
SC	57.22	-24.20	-3.16	16.57	-3.94	-1.44	5.12	46.16
TN	-39.44	-36.50	-8.86	10.85	-5.23	-2.25	-5.08	-86.50
TX	-321.52	89.52	14.46	19.90	18.25	8.78	29.22	-141.38
UT	26.31	-2.85	0.94	9.20	1.80	-2.43	5.62	38.59
VA	46.15	16.93	-3.22	-3.66	0.77	4.32	-2.08	59.20
WV	118.94	3.53	-0.47	-1.15	1.25	0.97	-1.24	121.83
WY	-15.71	-0.04	0.38	-0.46	0.64	0.35	0.48	-14.35
US	**828.17**	**-59.08**	**74.29**	**169.16**	-1.33	**15.53**	**136.88**	**1163.63**

Notes: Costs are positive values (crime has increased) and benefits are negative values (crime has decreased). Bold indicates that the sum across states is significantly different from zero at the .10 level. PH is Philadelphia.

However, the results with respect to the coefficients on the post-law trends, presented in Table 7a and Table 7b, tell a different story.

16 The results are similar if we use only coefficients that are significantly different from zero at the .10 level. In that case the overall net cost to the US is $1.5 billion.

Table 7a: Shall-Issue Post-Law Trend Coefficients: Violent Crime

	Murder		Rape		Robbery		Assault	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
AK	**-0.104**	-4.49	-0.041	-0.42	**-0.093**	**-4.58**	-0.023	-1.49
AZ	**-0.055**	-2.69	0.026	0.91	-0.003	-0.15	-0.010	-1.02
AR	**-0.108**	-6.22	**-0.081**	-2.98	0.004	0.17	**0.048**	5.21
FL	**-0.054**	-4.24	**0.032**	1.76	**-0.085**	-6.59	0.003	0.42
GA	0.010	1.29	**-0.066**	-5.39	-0.016	-1.26	-0.005	-0.80
ID	**-0.057**	-4.55	-0.003	-0.15	**0.077**	3.77	**0.032**	4.06
KY	-0.025	-1.16	**-0.103**	-4.81	**-0.050**	-2.17	**-0.081**	-6.86
LA	0.002	0.09	0.038	1.02	0.039	1.58	0.008	0.47
ME	**0.025**	2.95	-0.016	-0.71	-0.013	-1.16	**0.014**	2.35
MS	**0.053**	4.75	**0.059**	2.97	**0.084**	5.50	**0.067**	6.93
MT	**-0.025**	-1.84	-0.030	-1.51	**0.131**	13.39	**0.202**	24.85
NV	**-0.131**	-7.66	**-0.077**	-1.84	-0.023	-1.22	**-0.062**	-4.89
NC	-0.010	-0.57	**-0.083**	-1.65	-0.003	-0.15	-0.015	-1.34
OK	-0.002	-0.13	**-0.041**	-1.75	0.003	0.15	-0.002	-0.26
OR	**-0.083**	-8.24	-0.038	-1.64	**-0.038**	-3.43	**0.046**	6.33
PA	0.008	1.21	**-0.026**	-2.25	**0.030**	4.16	**0.011**	2.55
PH	-0.003	-0.28	**0.062**	2.50	0.032	1.18	**0.050**	4.27
SC	0.004	0.15	-0.069	-1.48	0.018	0.61	-0.020	-1.01
TN	**0.113**	9.97	**0.086**	4.74	**0.116**	6.81	**0.072**	8.03
TX	0.000	-0.01	-0.043	-1.61	-0.007	-0.45	**-0.016**	-2.26
UT	-0.016	-0.78	0.004	0.10	0.038	0.79	0.012	0.88
VA	0.001	0.09	0.004	0.42	**0.056**	3.34	**0.027**	3.81
WV	**-0.098**	-9.21	**-0.046**	-2.46	0.001	0.03	**0.105**	11.34
WY	**0.167**	7.53	-0.020	-0.81	0.018	0.77	**0.059**	5.78
US	**-0.017**	4.74	-0.022	1.77	0.0003	0.00	**0.011**	3.19
Negative	15		16		10		9	
Significant	8		8		4		3	
Positive	9		8		14		15	
Significant	4		4		6		12	

Note: see notes to Table 4a.

Table 7b: Shall-Issue Post-Law Trend Coefficients: Property Crime

	Burglary		Larceny		Auto	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
AK	**-0.030**	-1.95	**-0.030**	-2.06	**-0.046**	-2.34
AZ	-0.003	-0.34	**-0.024**	-2.87	**-0.060**	-4.38
AR	-0.013	-1.06	-0.005	-0.51	-0.004	-0.20
FL	**-0.031**	-4.43	-0.003	-0.45	-0.007	-0.49
GA	**-0.016**	-2.66	-0.007	-0.96	**0.027**	1.95
ID	0.004	0.50	**-0.021**	-2.62	0.016	0.95
KY	**-0.025**	-1.66	-0.007	-0.61	-0.016	-1.08
LA	**0.033**	2.32	**0.024**	2.21	**0.059**	2.07
ME	-0.004	-0.60	-0.001	-0.12	-0.003	-0.37
MS	**0.049**	7.05	**0.058**	8.50	**0.100**	6.46
MT	-0.002	-0.37	**0.017**	2.92	0.007	0.89
NV	**-0.024**	-2.52	**-0.048**	-4.73	-0.010	-0.57
NC	-0.015	-1.13	-0.012	-1.04	0.020	0.93
OK	-0.010	-0.97	-0.010	-0.88	0.002	0.11
OR	**-0.013**	-1.78	0.004	0.67	**-0.022**	-2.17
PA	0.000	0.03	**0.014**	3.54	-0.006	-1.08
PH	-0.012	-1.64	0.007	0.76	0.035	1.14
SC	-0.022	-1.19	-0.006	-0.34	0.042	1.33
TN	**0.054**	5.96	**0.061**	7.44	**0.077**	4.47
TX	0.005	0.52	-0.003	-0.28	-0.004	-0.25
UT	0.007	0.36	-0.009	-0.40	0.014	0.34
VA	0.002	0.36	0.004	0.60	0.025	1.52
WV	0.003	0.34	0.000	-0.05	**0.018**	1.71
WY	**-0.021**	-2.12	**-0.018**	-1.75	0.008	0.47
US	-0.004	0.02	0.003	0.02	0.008	0.35
Negative	15		16		10	
Significant	6		5		3	
Positive	9		8		14	
Significant	3		5		5	

Note: see notes to Table 5a.

The number of states with negative post-law trends is greater than the number with positive trends for murder, rape, burglary, and larceny. The US weighted average trend is significantly negative for murder, the most costly crime, and significantly positive only for assault. Because, as time passes, the trend will eventually dominate the shift, the trend is the only coefficient that matters in the long run. The implied costs and benefits are presented in Table 8.

Table 8: Long Run Costs and Benefits (Post-Law Trends), Millions of 2000 Dollars

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto	Total
AK	-9.70	-1.08	-0.72	-0.56	-0.16	-0.23	-0.55	-13.00
AZ	-80.37	3.81	-0.19	-2.03	-0.29	-1.97	-11.18	-92.22
AR	-96.03	-7.66	0.12	4.79	-0.52	-0.16	-0.14	-99.60
FL	-254.54	19.39	-34.08	2.11	-13.80	-0.50	-2.54	-283.96
GA	27.76	-20.62	-2.52	-1.34	-2.92	-0.72	4.81	4.47
ID	-5.32	-0.09	0.11	0.77	0.05	-0.26	0.12	-4.63
KY	-8.43	-4.76	-1.47	-5.10	-0.54	-0.11	-0.43	-20.83
LA	6.29	6.72	4.28	2.16	2.92	1.70	6.84	30.91
ME	2.47	-0.27	-0.03	0.21	-0.06	-0.01	-0.03	2.28
MS	34.48	4.28	1.37	2.47	1.81	1.02	1.79	47.21
MT	-1.14	-0.34	0.12	1.17	-0.01	0.11	0.04	-0.05
NV	-72.85	-7.27	-1.06	-5.46	-0.80	-1.11	-0.49	-89.03
NC	-22.43	-19.46	-0.37	-4.73	-2.42	-1.16	1.92	-48.66
OK	-2.14	-6.11	0.12	-0.40	-0.71	-0.43	0.13	-9.54
OR	-30.93	-5.17	-1.45	4.27	-0.67	0.16	-1.23	-35.03
PA	7.24	-5.67	2.20	2.10	0.01	0.97	-0.87	5.99
PH	-4.91	4.84	4.13	3.68	-0.32	0.13	3.65	11.20
SC	4.58	-13.20	1.10	-5.93	-1.75	-0.33	2.88	-12.64
TN	170.38	20.46	11.32	17.02	4.61	3.27	9.04	236.10
TX	-0.92	-37.90	-2.12	-13.74	1.64	-0.71	-1.78	-55.53
UT	-4.32	0.35	0.46	0.52	0.18	-0.33	0.45	-2.68
VA	1.06	0.69	3.35	2.48	0.13	0.25	1.90	9.86
WV	-41.03	-1.63	0.00	1.62	0.05	0.00	0.22	-40.76
WY	9.86	-0.33	0.01	0.64	-0.11	-0.12	0.03	9.98
US	**-370.94**	**-71.03**	**-15.33**	6.74	**-13.67**	-0.53	**14.61**	**-450.15**

Notes: Costs are positive values (crime has increased) and benefits are negative values (crime has decreased). Bold indicates that the sum across states is significantly different from zero at the .10 level. PH is Philadelphia

All crime categories except assault and auto theft show post-law benefits from the shall-issue laws. Murder, rape, robbery, and burglary show significant benefits across all states. The overall net benefit to the US is $450 million per year.[17] At this rate, it will take approximately six years for the initial costs to be offset by the eventual long-run benefits. After that, the net benefits increase continuously. The breakeven point is the same as that implied by the Ayres and Donohue analysis.

Another way to evaluate the effect of shall-issue laws is to estimate the cumulative effect through 2000 on the states implementing them. We

17 The numbers are very similar using only significant coefficients. In that case the annual net benefit from crime reduction is $398 million per year.

estimate the cumulative effect of the law by combining the estimated coefficient on the dummy variable with the corresponding coefficient on the trend variable using the formula,

$$E_i = \left(N + (N-1)\hat{g} + (N-2)\hat{g}^2 + \cdots + \left(N-(N-1)\right)\hat{g}^{N-1}\right)\hat{b}_{1i} + \left(\sum_{t=0}^{N} t + \hat{g}\left(\sum_{t=0}^{N-1} t\right) + \hat{g}^2\left(\sum_{t=0}^{N-2} t\right) + \cdots + \hat{g}^{N-1}\right)\hat{b}_{2i}$$

where E_i is the effect for state i, N is the number of years the law has been in effect, $\hat{g}$ is the coefficient on the lagged dependent variable, $\hat{b}_{1i}$ is the coefficient on the shall-issue dummy for state i, and $\hat{b}_{2i}$ is the coefficient on the post-law trend for the same state. This is the cumulative effect over all the years the law has been in existence in each state, up to the year 2000. The net effect for the U.S. as a whole is computed as the population-weighted average. The results are presented in Table 9.

The number of states experiencing increases in crime is larger than the number with reductions in murder, robbery, assault, and auto theft, confirming the Ayres and Donohue finding for those crimes. On the other hand, there are more reductions for rape, burglary, and larceny. The results are similar if we only count significant coefficients. Despite the fact that the number of states with increases in crime is larger than the number experiencing declines, the overall population-weighted effect for the US is significantly negative for murder and burglary. The only crime for which the net effect of these laws across the US is significantly positive is assault. The other crimes have cumulative effects that are not significantly different from zero.

Table 9: Cumulative-2000 Effect of the Shall-Issue Laws on Crime

	Year Passed	N	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto
AK	1994	6	**-1.430**	**-3.957**	**-2.087**	-0.252	**-0.808**	**-0.894**	**-1.771**
AZ	1994	6	0.422	0.169	**0.960**	0.112	0.293	-0.241	-0.304
AR	1995	5	**-1.376**	-1.067	-0.302	**1.221**	**-0.590**	-0.107	-0.220
FL	1987	13	**-6.064**	0.516	**-5.877**	**1.197**	**-2.563**	-0.190	1.538
GA	1989	11	**-1.532**	**-4.910**	**-2.685**	**-0.905**	**-2.244**	**-1.088**	-0.030
ID	1990	10	**6.257**	**2.570**	**4.314**	**1.696**	-0.105	**-0.462**	**1.367**
KY	1996	4	-0.068	**-2.232**	**0.606**	-0.169	**-0.460**	**-0.496**	0.032
LA	1996	4	**1.550**	**0.834**	**1.539**	**0.306**	**0.515**	**0.444**	**1.518**
ME	1985	15	**9.961**	-0.110	**-3.736**	-0.635	-0.104	0.904	1.054
MS	1990	10	**3.616**	**2.897**	**6.063**	**4.863**	**2.026**	**2.996**	**5.006**
MT	1991	9	**-1.034**	0.761	**2.018**	**7.201**	**0.774**	**0.869**	-0.041
NV	1995	5	**0.792**	-0.399	0.192	-0.066	**0.378**	**-0.237**	**0.495**
NC	1995	5	-0.099	**-0.981**	0.402	**0.288**	**-0.355**	0.121	**1.299**
OK	1995	5	**0.425**	-0.316	-0.259	-0.003	-0.145	-0.004	-0.176
OR	1990	10	**-6.700**	**-1.852**	**-4.482**	**3.022**	**-1.704**	0.225	**-1.518**
PA	1989	11	0.257	-1.006	**1.336**	0.189	-0.139	**1.127**	-0.340
PH	1995	5	-0.170	**-0.795**	0.186	**-0.322**	**-0.632**	**-1.009**	0.418
SC	1996	4	0.238	**-1.196**	-0.028	0.023	**-0.426**	-0.161	**0.715**
TN	1994	6	**2.219**	**0.889**	**1.899**	**1.776**	**0.767**	**1.025**	**1.358**
TX	1995	5	-0.278	-0.139	0.129	-0.126	**0.349**	0.122	0.263
UT	1995	5	0.255	-0.108	0.957	**1.253**	**0.460**	**-0.453**	**1.080**
VA	1988	12	0.407	**1.630**	**3.737**	**1.634**	0.255	**1.028**	1.629
WV	1989	11	**-3.352**	-1.936	-0.667	**6.119**	0.913	0.897	0.090
WY	1994	6	**1.912**	-0.440	**3.449**	**0.979**	0.326	-0.054	**1.032**
US			**-1.169**	-0.589	-0.571	**0.971**	**-0.522**	0.262	0.549
Negative			11	16	9	8	13	13	8
Significant			7	7	5	2	9	7	2
Positive			13	8	15	16	11	11	16
Significant			8	5	10	13	7	6	9

Notes: coefficients in bold are significant at the .10 level using standard F-tests. Coefficients are the estimated percentage change in crime over the N years the law has been in effect. PH is Philadelphia.

We can estimate the cumulative benefits of the law using the costs of each crime and the cumulative effects from Table 9. The results are presented in Table 10.

Table 10: Cumulative-2000 Benefits of the Shall-Issue Law, Millions of 2000 Dollars

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto	Total
AK	-134	-116	-17	-7	-6	-9	-26	-315
AZ	626	24	64	40	42	-21	-48	727
AR	-1238	-109	-10	163	-33	-4	-10	-1241
FL	-28938	339	-2602	1375	-1691	-53	736	-30834
GA	-4289	-1722	-480	-357	-601	-174	-19	-7641
ID	597	80	7	58	-2	-7	13	745
KY	-23	-113	20	-10	-13	-10	1	-147
LA	4013	159	184	106	60	42	216	4781
ME	979	-2	-11	-16	-2	17	12	977
MS	2357	234	108	256	107	74	115	3251
MT	-48	10	2	58	6	8	0	35
NV	448	-40	10	-1	19	-6	32	462
NC	-229	-249	53	134	-80	19	158	-195
OK	589	-50	-10	0	-13	0	-16	499
OR	-2521	-278	-190	402	-135	14	-111	-2819
PA	247	-243	106	44	-22	116	-59	188
PH	-255	-71	25	-38	-23	-28	52	-340
SC	277	-249	-2	15	-44	-12	60	45
TN	3363	217	194	548	79	68	186	4656
TX	-1638	-123	45	-131	163	47	154	-1482
UT	68	-10	13	74	16	-23	44	182
VA	644	289	245	214	30	106	160	1688
WV	-1414	-76	-5	134	27	15	1	-1318
WY	113	-8	3	13	3	0	4	128
US	**-26406**	**-2105**	**-2250**	**3075**	**-2114**	**177**	**1655**	**-27969**

Notes: Costs are positive values (crime has increased) and benefits are negative values (crime has decreased). Bold indicates that the sum across states is significantly different from zero at the .10 level. PH is Philadelphia.

Fourteen states experienced cumulative benefits while ten states experienced cumulative costs. However, the consequences are very different across states. Louisiana and Tennessee have suffered combined increases in crime costs of approximately $10 billion, while Florida and Georgia have enjoyed benefits of crime reduction of $38 billion. The estimated population-weighted net effect across all states is a reduction in crime costs of $28 billion. The results are similar using only significant coefficients, with an estimated net benefit of $28.4 billion in reduced crime.

The cumulative results through 2000 are dominated by Florida, which benefited to the tune of $30.8 billion from passing the shall-issue law in 1987. Since

the net effect across all states is $28 billion, the other states have experienced a net increase in crime amounting to a cost of $2.8 billion. However, this sum is not significantly different from zero. Also, even without Florida, there is a long run net benefit of $183 million per year, which is significantly different from zero. If the ethically proper social discount rate is reasonably low, then the only relevant result is the ongoing long-run effect, which is less crime. Therefore, even excluding Florida, the state which has apparently benefited most from a right-to-carry law, the overall long run impact of these laws is lower crime.

Summary and Conclusion

Many articles have been published finding that shall-issue laws reduce crime. Only one article, by Ayres and Donohue who employ a model that combines a dummy variable with a post-law trend, claims to find that shall-issue laws increase crime. However, the only way that they can produce the result that shall-issue laws increase crime is to confine the span of analysis to five years. We show, using their own estimates, that if they had extended their analysis by one more year, they would have concluded that these laws reduce crime. Since most states with shall-issue laws have had these laws on the books for more than five years, and the law will presumably remain on the books for some time, the only relevant analysis extends beyond five years. We extend their analysis by adding three more years of data, control for the effects of crack cocaine, control for dynamic effects, and correct the standard errors for clustering. We find that there is an initial increase in crime due to passage of the shall-issue law that is dwarfed over time by the decrease in crime associated with the post-law trend. These results are very similar to those of Ayres and Donohue, properly interpreted.

The modified Ayres and Donohue model finds that shall-issue laws significantly reduce murder and burglary across all the adopting states. These laws appear to significantly increase assault, and have no net effect on rape, robbery, larceny, or auto theft. However, in the long run only the trend coefficients matter. We estimate a net benefit of $450 million per year as a result of the passage of these laws. We also estimate that, up through 2000, there was a cumulative overall net benefit of these laws of $28 billion since their passage. We think that there is credible statistical evidence that these laws lower the costs of crime. But at the very least, the present study should neutralize any "more guns, more crime" thinking based on Ayres and Donohue's work in the *Stanford Law Review*.

We acknowledge that, especially in light of the methodological issues of the literature in general, the magnitudes derived from our analysis of crime statistics and the supposed costs of crime might be dwarfed by other considerations in judging the policy issue. Some might contend that allowing individuals to carry a concealed weapon is a moral or cultural bad. Others might contend that greater

liberty is a moral or cultural good. All we are confident in saying is that the evidence, such as it is, seems to support the hypothesis that the shall-issue law is generally beneficial with respect to its overall long run effect on crime.

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About the Author

Carlisle E. Moody is Professor of Economics at the College of William and Mary.

Thomas B. Marvell is an attorney-sociologist and is director of Justec Research in Williamsburg, VA.

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Econ Journal Watch,
Volume 5, Number 3,
September 2008, pp 294-315.

DO ECONOMISTS REACH A CONCLUSION?

Do Economists Reach a Conclusion on Subsidies for Sports Franchises, Stadiums, and Mega-Events?

DENNIS COATES[1] AND BRAD R. HUMPHREYS[2]

ABSTRACT

State and local governments in the United States have long been called upon to subsidize the construction of stadiums and arenas. Indeed, the first wave of government subsidization dates to the years between 1917 and 1926, the first boom in stadium construction. Since then, one thing that has changed substantially is the rationale for public-sector support. In 1926, *The Playground* said the goal was for "the stadium to have as broad a use as possible." It recommended the once-familiar horseshoe shape because it facilitated egress via the open end and included a long straightaway suitable for a procession. The implicit rationale was for the facility to serve the broad public interest by hosting pageants, parades, rallies, and festivals, as well as sporting contests of all sorts from track and field to football and baseball. Today, stadium subsidization focuses on a single use, namely, hosting professional sports franchises, which usually have substantial control over the facilities' availability for other events.

The change from public provision of venues available for a wide array of events to public subsidization of largely privately controlled facilities is a fairly recent phenomenon. The change occurred gradually. The first steps may have occurred with baseball-franchise relocations –the Braves' relocation in 1953 from Boston to Milwaukee, the Browns in 1954 from St. Louis to Baltimore, and the Athletics in 1955 from Philadelphia to Kansas City. In each case, new or recently renovated publicly owned facilities were made available to baseball franchises on quite generous terms. For example, *The New York Times* reported on March 15, 1953 that the Braves were offered a flat rental of $1,000 for the first two years on the new County Stadium in Milwaukee.[3]

1 Department of Economics, University of Maryland, Baltimore County. Baltimore, MD 21250.
2 Department of Economics, University of Alberta. Edmonton, Alberta, Canada T6G 2H4
3 It may be that what has come to be known as the major league city argument for attracting a profes-

Ralph Wulz (1957) argued that public ownership of stadiums was justified if "private enterprise could not provide the service which the public demanded and at the same time realize an adequate profit on its investment." However, Wulz did not foresee private businesses being subsidized in their use of these facilities, stating that subsidization might be suitable for "governmental activities and perhaps activities at which no admission is charged," but that "commercial type activities pay the full cost of the services or facilities which are provided" (93).

Wulz's (1957) discussion raises the question of the possible theoretical justification for the subsidies we see today. Subsidies can, in general, be justified either on efficiency or distributive grounds. For example, a subsidy could be justified if the unsubsidized market would supply too little of the good. This is the classic situation of positive externalities. The subsidy would induce greater provision. Alternatively, subsidies could be justified as a means of redistribution. For example, public education is paid for out of taxes, with wealthier individuals paying more in taxes than the cost of the services they receive and poorer individuals paying less than the full cost of the education. We will address each of these justifications for stadium and arena subsidies in turn.

To justify a stadium subsidy on efficiency grounds requires an explanation of how the market outcome will result in "too little" quantity. That is, one must explain how marginal social benefit from the stadium exceeds the marginal social cost. A difficulty in this case is that sports facilities are very lumpy; the debate often focuses on whether to build a facility, not about increasing the seating capacity by an additional seat. The market outcome, therefore, may be no construction of a stadium or an arena at all, and consequently no sporting events. This is the market failure justification implicit in the "build it and they will come" strategy of cities whose intent is to lure an existing franchise away from some other city or to induce a professional league to grant the city an expansion franchise. It is also the justification for a city to replace an existing facility to keep the current team or teams from moving.

A recent example from the NBA illustrates the kind of thing that often goes on now. The Seattle Supersonics were unhappy with their former home, KeyArena, and sought to have the city of Seattle build a new arena. Seattle refused and the team explored moving, which would require breaking their lease with the City of Seattle for KeyArena. A lawsuit ensued and they settled out of court, with the team moving to Oklahoma City, for the 2008-2009 season, and paying Seattle tens of millions of dollars to break the lease. Oklahoma City attracted the team by promising to spend $100 million renovating its existing arena to bring it up to current NBA standards and an additional $20 million to construct a practice facility. The existing arena in Oklahoma City was built without an occupant during the

sional sports franchise came from Lou Perini, President of the Boston Braves, discussing his decision to move the team to Milwaukee. *The New York Times* article quotes him as saying "Maybe Milwaukee isn't a major league city. I don't know, but I feel it will become one."

1990s as part of a downtown redevelopment plan. The Seattle-Oklahoma City case suggests relevant lessons: Professional sports leagues are able to restrict entry and play one city off against another to extract the best subsidy deal. In doing so, there is a significant positional element—one city's fan-base loses, another gains. And teams exploit the cities where politics most effectively taps the taxpayers.

In this paper we examine the economic research on subsidies for sports franchises, stadiums, and mega-events. We ask whether economists who conduct such research reach a conclusion. Our investigation suggests that such economists largely agree that subsidization is undesirable. Before turning to the economic literature, we examine the results of a recent survey, and frame the issue in terms of economic intuition.

The Average Economist Opposes Sports Subsidies

Survey evidence indicates that on some policy issues economists in general hold views different from those who specialize in the issue.[4] When it comes to sports subsidies, however, both sets of economists appear to agree. In a 2005 survey of a random sample of American Economic Association members, Robert Whaples (2006) asked of agreement with the following:

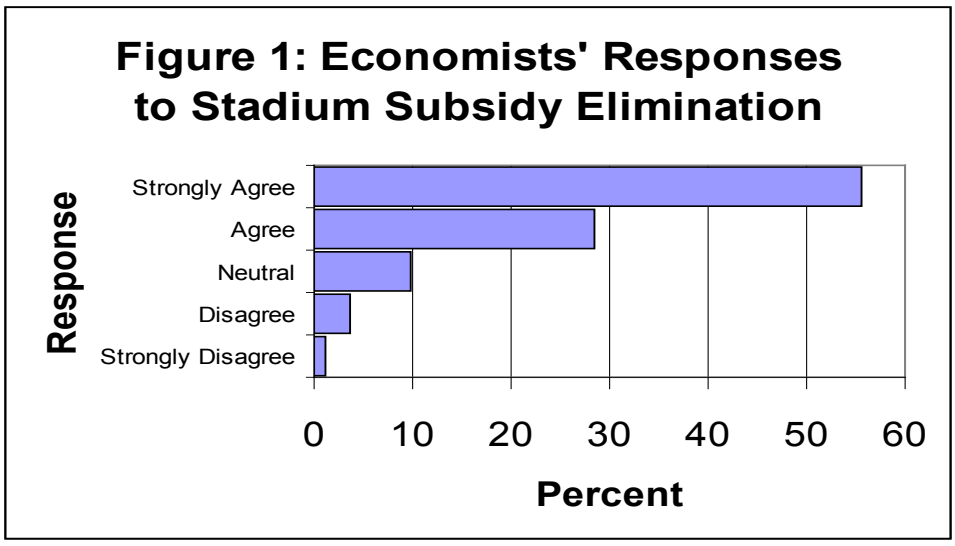
> *Local and state governments in the U.S. should eliminate subsidies to professional sports franchises.*

Possible responses were "Strongly Disagree," "Disagree," "Neutral," "Agree," and "Strongly Agree." Figure 1 shows that 58 percent strongly agreed, and 28 percent agreed. About 10 percent were "neutral." Only 5 percent disagreed. Sports subsidies was but one of about 20 policy issues included in the survey, but Whaples highlights this issue as one of exceptional consensus—the other standouts being free trade, outsourcing, and the elimination of agricultural subsidies. The sports question, in fact, received the largest "strongly agree" response in the entire survey.

Incidentally, similar economic intuition can be attributed to Adam Smith (1776). In writing of "public diversions," meaning displays "to amuse and divert the people by painting, poetry, music, dancing; by all sorts of dramatic representations and exhibitions," Smith favored the state's "encouraging" such activities, but he specifically clarified what he meant by "encouraging": "that is by giving entire liberty to all those who for their own interest would attempt without scandal or indecency" (796). Thus, where Smith specifically identified the form of encourage-

4 For example, on rail-transit projects and subsidies, Balaker and Kim (2006) show that the specialist economists are mostly opposed, likely much more so than the "average" economist; on the pharmaceutical policies administered by the Food and Drug Administration, Klein (2008) indicates that the specialist economists are substantially more supportive of liberalization than the "average" economist.

ment, he spoke of liberty and made no mention of subsidization—whereas he did countenance subsidization when it came to the schooling of children (785, 815).



Source: Whaples (2006)

THE ECONOMIC INTUITION BEHIND THE CASE AGAINST SPORTS SUBSIDIES

The consensus among economists on the question of sports facility subsidies likely stems from the basic economic intuition that government subsidies ought to address some "market failure," and economists sense that there is no compelling case to be made for sports subsidies. The argument for a subsidy always comes from a local context, when a city wants to attract a new team or hold on to an existing one, and we address these local issues. But one can also assess the subsidy argument from a global perspective, which is where we begin.

The Seattle-Oklahoma City case described above is a perfect example of the global case against subsidies. Oklahoma City offered larger subsidies than Seattle was willing to make, so the basketball franchise left Seattle for Oklahoma City. Basketball fans in Seattle lose, basketball fans in Oklahoma City gain. Perhaps there are more fans in Oklahoma City than in Seattle or fans in Oklahoma have more intense preferences for NBA basketball than fans in Seattle, so there might be some slight gain in average welfare as a result of the franchise move. By and large, however, the franchise changing cities is a zero sum game for basketball fans. The team is enriched by the larger subsidy available in Oklahoma, but the move is clearly not a Pareto improvement in the allocation of resources. From a social perspective, a better approach to maximizing welfare might be for the NBA to expand the number of franchises so basketball fans across the country had their

own local team to cheer. That, of course, is not in the best interests of the current league members who derive substantial benefits, like the subsidies, from their restriction of supply.

Calls for subsidies at the local level come from interest groups and their consulting firms—which we call "promoters" of subsidization—who talk up local benefits of sports franchises, stadiums, and mega-events. As we shall see, promoters' claims of such local benefits don't hold up empirically. But such rationales can also be countered by simple economic intuition.

The promotional literature often suggests that if the city attracts or retains a sports franchise, its people derive specific economic benefits from the presence of the team, including more local jobs, higher local income, and increased local tax revenues. In addition, some promoters suggest that the presence of a franchise generates intangible economic benefits for the city. For example, promoters consistently argue that having major league sports raises the status of the city and brings added national and world recognition that enhances the business prospects and even the self-esteem of the community. For example, Oklahoma City's quest to raise the sales tax by one cent to fund the improvements to its existing arena went by the name "Big League City" campaign. The prospect of a game being broadcast nationally or even internationally from the stadium or arena is touted as a wonderful advertisement of the city's virtues. These benefits, which the teams cannot capture, are used to justify a local government subsidy for the construction of the facility.

The promotional literature suffers from a long list of methodological and theoretical problems, all of which have been well-documented in the literature. Economic intuition suggests several of these problems:

1. The redistributive implication of moving a franchise from one city to another also applies to the context of moving a stadium from one part of a city to another. If a new stadium is built in the downtown area to revitalize that section of town, then at least a portion of any such vitality naturally comes from the part of the city around the original stadium.
2. Much of the consumer spending associated with professional sports comes out of the entertainment budgets of local residents. When a new sports franchise appears in a city, local entertainment spending on sports increases and local entertainment spending on other activities like movies, bowling, etc. decreases. The effective "local spending multiplier" on activities like bowling and attending plays or concerts is higher than the multiplier on professional sporting events because the owners of bowling alleys, theatres, and restaurants, as well as the employees of these establishments, live in the community while the owners and highly paid players (who receive a majority of team's expenditures) on

professional sports teams generally do not. Since spending on professional sports teams substitutes for other local consumer entertainment spending and has a lower local spending multiplier, professional sports can reduce local income rather than increase it.

3. Sports are one of many cultural activities within the city. For every individual who derives enjoyment from the presence of the sports franchises in the community, there are likely to be other individuals who are uninterested in sports or even resent being taxed to subsidize an activity they have no use for. Others argue that sports culture diverts people from more socially beneficial interests and pursuits. Before accepting that sports teams generate external benefits, a careful and thorough look at "external costs," and the alternative uses of resources devoted to subsidies—uses that might also have "external benefits"—is clearly warranted. Unfortunately, such debate quickly leads public discourse and policymakers into a briar patch of unpriced values that are easily misrepresented. Thus, economists generally urge that society steer away from government sponsorship of cultural activities not related to education.
4. Government expenditures on stadium and arena subsidies carry opportunity costs which are never addressed. Tax collections used to pay stadium debt, for example, could have gone for other public projects with higher social rates of return than a stadium. One never knows what the returns to alternative uses of the funds might be because alternatives are never discussed. These alternative uses could be construction or maintenance projects, on highways, mass transit systems, hospitals, or schools. Or, the alternative could be to reduce taxes.
5. Whatever inefficiencies might exist in a system without sports subsidies, economic intuition suggests that government subsidization introduces new distortions and imperfections, including the excess burden and administrative costs of raising and spending tax monies.

The Whaples survey did not ask economists the reasons for their views, so we cannot say that the foregoing points speak for economists in general. It seems clear enough, though, that economists are unlikely to warm to subsidies that do not plausibly foster a public good or serve redistributive goals. The case for sports subsides is weak, prima facie. Further, sports subsidies do not do much to advance political identity, as it is not the polity but the sport, the league, the team and their multi-million dollar players and managers who soak up the attention and identification. It is plausible, for example, that some economists warm to rail-transit projects and subsidies for their political and symbolic aspects, but sports subsidies lack even this "benefit."

Despite the strong intuitive case against stadium and arena subsidies, they exist and are valued in the billions of dollars. The local promoters have claimed

billions of dollars in benefits to the community and, apparently, their arguments have convinced many key local decision makers to give them access to the public purse. We turn now to the evidence on the local benefits from those subsidies.

The Promotional Literature Versus the Economists

The remainder of this paper surveys the literature on the subsidization of sports franchises through the provision of publicly financed stadiums and arenas to determine whether economists reach a conclusion on the efficacy of those subsidies as sources of economic development, income and tax revenue growth and job creation. This literature consists of two rather distinct types of analysis: analysis done largely by academics, mostly economists, but also regional scientists, urban affairs, and public policy scholars; and analysis done by consulting firms who may employ economists, accountants, or policy analysts. Work by this latter group is what we have referred to as the promotional literature.

Within the promotional literature, proponents of stadium subsidies argue that subsidies are warranted because of the local economic development benefits of building a stadium or arena, including the "big league city" benefits. They do not support subsidies based on the consumer surplus derived by game attendance nor from consumer external benefits from such activities as talking about the teams or following them through the print or broadcast media. The economic development benefits of interest to boosters are predominantly identified with income and job creation, and sometimes as increased tax revenue, and are generally called "tangible benefits" in the literature. Because the proponents of stadium subsidies focus on jobs, income, and tax revenue enhancement, the academic literature has focused its attention on these purported benefits as well until quite recently.

Some subsidy advocates have implicitly justified them as enhancing redistribution. This justification exists both in the promotional "economic impact" literature and in the academic literature, with most examples of the latter appearing recently. The justification is that building stadiums or arenas downtown, in the central city of metropolitan areas, will bring economic activity to those neighborhoods and aid in their revitalization. Downtown areas, especially in older cities, have become stagnant and decayed over time as people and businesses moved to the suburbs. Those older areas are, so the argument goes, deserving of assistance, even at the expense of the outlying areas. This justification rests on the downtown stadium or arena bringing new jobs and businesses into the downtown area.

We restrict our attention to the academic literature, much of which attempts to verify the claims of the promotional literature. Because several contributors to the literature do not hold a Ph.D. in economics, we have tried to distinguish "non-

economist" authors from authors who are economists. A necessary condition for being classified a non-economist is not holding a Ph. D. in economics. Absent information on the discipline of an author's doctorate, we also report in footnotes whether the author in question does not work or has not worked in economics departments, and if one does not publish predominantly in economics journals. The individuals who either do not hold a doctorate in economics or have not worked in economics departments or whose research is published primarily in public policy or urban or regional science journals tend to reach conclusions generally at odds with "economist" authors—that is, those that hold a doctorate in economics, work or have worked primarily in economics departments, or publish predominantly in economics journals.

The literature initially examined data on local or regional output, income, and jobs for evidence of an impact from sports franchises and facilities. More recently, the search for economic effects of franchises and facilities has turned to tax revenues and effects on rents and property values. Researchers have looked for evidence of economic impact flowing from the operation of sports facilities and from the construction of these facilities. In addition, we address the extent to which subsidies to sports franchises and facilities are connected to the city being selected as host for a sporting mega-event, such as the Super Bowl or the Major League Baseball All-Star Game, and estimates of the subsequent benefits to the city from hosting these events.

Both academic economists and consultants reach a conclusion about the economic impact of professional sports franchises and facilities, but these two groups reach opposite conclusions. The clear consensus among academic economists is that professional sports franchises and facilities generate no "tangible" economic impacts in terms of income or job creation and are not, therefore, powerful instruments for fostering local economic development. The clear consensus among consultants who produce "economic impact studies" is that professional sports franchises and facilities generate sizable job creation, incremental income increases, and additional tax revenues for state and local governments. We will not discuss further the promotional economic impact studies but instead refer the reader to four excellent criticisms of those studies, namely, Noll and Zimbalist (1997c), Crompton (1995), Siegfried and Zimbalist (2000), and Hudson (2001). In particular, the book *Sports, Jobs and Taxes: The Economic Impact of Sports Teams and Stadiums* by Roger Noll and Andrew Zimbalist (1997a) brought together a series of papers that addressed teams and stadiums as economic development tools. The title of their introductory chapter, "Build the Stadium – Create the Jobs!" (1997b), indicates just how far the thinking about sports facilities has changed since the 1920s. Since the publication of *Sports, Jobs and Taxes*, a large literature has developed assessing the impact of stadiums and franchises on city economies.

Here, we review the existing literature on the tangible economic impact of professional sports franchises and facilities published in peer reviewed journals.

Although a small and growing literature exists which estimates the value of "intangible" economic benefits, we do not survey this literature.

The Findings of Peer-Reviewed Economic Research on Economic Impacts

The academic research on the economic impact of professional sports franchises and facilities, in general, comes from retrospective econometric research, though some case studies also exist. In the econometric research, researchers collect time series or panel data from Metropolitan Areas (MAs) or states that were home to professional sports franchises and facilities and estimate reduced form econometric models of the determination of various economic indicators, typically real income per capita or total employment. These analyses generate estimates of the impact of a sports franchise or facility on the economy. If the coefficient on a facility or franchise variable is statistically significant and positive, then statistically that sports variable is inferred to induce an increase in the dependent variable measuring economic activity. If the sports variable is not statistically significant or is significant and negative, then the inference is that the variable does not induce increases in economic activity or that it causes a decline in activity. When the variable is positive and statistically significant, the coefficient is assessed for economic significance by the researcher and compared to the claims of sports boosters. For example, boosters may claim that hosting the Super Bowl will generate $300 million of new income, but the estimates associate the Super Bowl with only $30 million dollars of activity. Academic economists interpret disparities between boosters' findings and independent researchers' results as evidence that sports led development is not efficacious.

Professional Sports Franchises and Facilities

There now exists almost twenty years of research on the economic impact of professional sports franchises and facilities on the local economy. The results in this literature are strikingly consistent. No matter what cities or geographical areas are examined, no matter what estimators are used, no matter what model specifications are used, and no matter what variables are used, articles published in peer reviewed economics journals contain almost no evidence that professional sports franchises and facilities have a measurable economic impact on the economy.

Baade and Dye (1988) examined the economic impact of professional sports on the determination of annual manufacturing employment, real value added in manufacturing, and new capital expenditure by manufacturing firms in eight metropolitan areas over the period 1965-1978. The source of their data was

the Annual Survey of Manufactures. Explanatory variables included the population of the metropolitan area (MA), a time trend, and indicators for a new or renovated stadium, a professional football franchise, and a professional baseball franchise. They found little evidence that variation in professional sports franchises or facilities explained observed variation in employment, value added, or capital expenditure. Only four of the parameters on the franchise/facility indicators were statistically significant at the 5% level; three were positive and one was negative. Interestingly, this is the only paper in the literature to make use of MA data from the Annual Survey of Manufactures. This survey contains detailed MA level data on the composition of businesses in the local economy and should be used more often when assessing the economic impact of professional sports.

Baade and Dye (1990) next examined the economic impact of professional sports on annual real MA personal income, and the ratio of MA personal income to regional personal income in nine MAs over the period 1965-1983. The explanatory variables were the same as in their earlier study and they found no evidence that variation in the presence of sports franchises and facilities explained any of the variation in the real personal income across MAs.

Baade (1996) examined the economic impact of professional sports on the determination of real per capita income and the metropolitan area's share of state employment in the Amusement and Recreation industry and the Commercial Sports industry in 48 metropolitan areas over the period 1957-1989. The dependent variable, real per capita income, in these reduced form regression models was transformed using a complex function of the average level of per capita income across the cities in the sample and first differences. The 48 metropolitan areas in the sample included both cities with professional sports teams and cities with no professional sports teams. Separate regressions were run for each metropolitan area. The explanatory variables included the number of sports franchises and the number of sports facilities less than ten years old in the metropolitan area. In general, the sports facility and franchise variables were not statistically significant, and the few that were significant displayed no consistent pattern of signs. Baade (1996) concluded that there was no evidence that professional sports franchises or facilities had a positive impact on real per capita income or employment in these two industry classifications that include sports franchises.

Baade and Sanderson (1997) examined the employment created by sports facilities. The authors used data on employment from the Amusements and Recreation, and Commercial Sports Industry classifications of the Standard Industrial Classification for ten cities and their states covering the years 1958 through 1993. They estimated separate regressions for each city, with the dependent variable either the city's share of state employment in the Amusements and Recreation or the city's share of state employment in Commercial Sports. They found very little effect of newly constructed stadiums or from having additional professional teams on employment shares. When new stadiums were significant, their effect

was to reduce the city's share of employment. An additional team statistically significantly raised the city's share in several cases, and reduced it significantly in one case. Thirteen of twenty coefficients for the number of teams were not statistically significant. Baade and Sanderson sum up their results by saying, "In general, the results of this study do not support a positive correlation between professional sports and job creation" (112).

Hudson (1999) examined the economic impact of professional sports on urban employment in 17 metropolitan areas over a twenty year period. This study used both the number of professional sports franchises in the metropolitan area and indicator variables for the presence of MLB, NFL, NBA and NHL franchises, as well as a variety of explanatory variables reflecting local wages, income, energy prices, population and taxes. None of the sports franchise variables were statistically significant, indicating that professional sports teams had no effect on employment in this sample of cities.

Coates and Humphreys (1999) examined the impact of professional sports on the level and growth rate of per capita income for all 37 metropolitan areas that had an NFL, MLB or NBA franchise over the period 1967-1994. This study included a vector of variables reflecting the "sports environment" in these metropolitan areas that included franchise indicator variables, new facility indicator variables, variables identifying the first ten years that a new franchise or facility was in a metropolitan area, stadium and arena capacities, and variables identifying periods after franchises left cities. The models contained metropolitan area fixed effects, a lagged dependent variable, and local population. Although few of the variables in the sports environment vector were individually significant, an F-test on the entire vector indicated that the variables were jointly significant, and the average effect on the level of real per capita income was negative. The sports environment vector was not significant in the regression that used the growth rate of real per capita income as the dependent variable. Coates and Humphreys (1999) concluded that professional sports had no positive effect on metropolitan area real per capita income and may have a negative effect.

Coates and Humphreys (2001) used sports strikes as a natural experiment to test for an economic impact of professional sports on the level of income per capita in urban areas.[5] The paper used the vector of "sports environment variables" from Coates and Humphreys (1999) and augmented this with indicator variables for five work stoppages in the NFL and MLB during the sample period. Work stoppages in professional sports leagues are useful for analyzing the economic impact of professional sports franchises because they represent periods when there are no sporting events to draw outside visitors to a city, the primary driver of economic impact in promotional economic impact studies, and they are

5 Sociologist John F. Zipp, (1996) also examined the impact of the Major League Baseball strike in 1994.

unexpected, infrequent events. Coates and Humphreys found that real income per capita in metropolitan areas did not fall during work stoppages in professional sports leagues, supporting the emerging consensus in the literature that professional sports has no tangible economic impact on local economies.

Coates and Humphreys (2002) used a second natural experiment, playoff appearances by franchises, to measure the economic impact of professional sports on real per capita income in metropolitan areas. The sports variables used were the vector of "sports environment" variables used in their earlier studies (Coates and Humphreys 1999, 2001), augmented with indicator variables for various postseason appearances in Major League Baseball, the National Football League, and the National Basketball Association. The results indicate that real per capita income in metropolitan areas that are host to postseason games is identical to real per capita income in metropolitan areas that are not host to postseason games, disputing the idea in promotional economic studies that postseason games are an important source of economic impact. The results did suggest that the metropolitan area that is home to the Super Bowl winner had higher real per capita income in the following year than in other metropolitan areas, but this cannot be attributable to direct economic impact because the Super Bowl is played at a neutral site.[6] Hosting the Super Bowl also had no effect on real per capita income in the host metropolitan area.

Coates and Humphreys (2003) used the same approach as in their earlier work, but used the analysis to explain wages and employment in two sectors of the economy that are closely linked to activities in stadiums and arenas: the services and retail sectors. The services sector includes both hotels and amusements and recreation as sub-sectors, while the retail sector includes eating and drinking establishments. By looking at employment and earnings in these sectors rather than in the metropolitan area, their analysis is focused where sports-led development advocates contend much of the impact will be. Coates and Humphreys' (2003) evidence suggests that positive effects in earnings per employee in one sector, Amusements and Recreation, are counterbalanced by negative effects on both earnings and employment in other sectors. Their evidence also suggests that professional sports reduce real per capita income in cities both because of substitution effects, where private expenditures are switched between sectors of the economy but are not increased, and in the creation of relatively low paying jobs.

Gius and Johnson (2001) also examined the effect of professional sports teams on per capita income in metropolitan areas. Gius and Johnson (2001) estimated the effect of sports franchises on the determination of per capita income using data from all cities with population over 25,000 that were included in the 1988 and 1994 City and County Data Books. The 1988 City and County Data

6 Matheson (2005) finds no effect on the victorious city from the Super Bowl. Davis and End (forthcoming) reach the opposite conclusion.

Books included calendar year 1985 data for 951 such cities; the 1994 City and County Data Books included calendar year 1990 data for 1,083 such cities. The paper used two sports indicator variables: an indicator variable for cities with one professional sports team (NFL, NBA, MLB and NHL) and an indicator for cities with two or more professional sports teams. Neither of the sports indicator variables was statistically significant at the 5% level or better, suggesting that real per capita income was not higher in cities of population over 25,000 with professional sports teams.

Miller (2002) examined the effects of two professional sports facility construction projects on employment in the construction industry in St. Louis, MO. Miller's empirical models controlled for factors that affect employment in the construction industry and accounted for the effects of wages on employment in construction. The sports variables used were indicator variables for the specific quarters in which the Kiel Center and TransWorld Dome were being built. The results indicate that the construction of these two sports facilities had no statistically significant effect on employment in the construction industry. This result rebuts the claim in promotional economic impact studies that the urban economy will experience significant increases in employment as a result of the construction of a sports facility.

Lertwachara and Cochran (2007) used an event study methodology to assess the impact of a new franchise, enticed into a city via subsidies, on the local economy. They look for a difference in the city or regional economy before and after expansion or relocation of a new franchise into the city. Their evidence includes new teams from each of the four major US professional sports leagues, MLB, NFL, NBA, and NHL. Their results are consistent with those in the literature and specifically with the findings of Coates and Humphreys (1999) that new teams "have an adverse impact on local per capita income for U.S. markets in both the short and long run" (244).

Rosentraub (1997) asked the question of whether stadiums and arenas can "reverse or slow the suburbanization trends so apparent in numerous urban areas" (180). Rosentraub identified two approaches or rationales for building stadiums downtown, revitalization and the creation of an export industry. Indianapolis followed an explicit, and aggressive, policy of developing an export-based sports program, while other cities followed the revitalization strategy. Rosentraub concludes, "In contrast to cities that did not build downtown sports facilities, the experience of cities with these assets is not encouraging" (205). He goes on to say that "this first view of the potential of downtown facilities to invigorate CBDs suggests that great caution should be used before spending substantial amounts of the public's resources on this tool for redesigning urban space" (206).

Two authors have concluded that sports and franchises are beneficial in

terms of income and job creation for the cities. Nelson[7] (2001) examined the effects of professional sports teams on the share of state personal per capita income attributable to metropolitan areas. Nelson (2001) estimated a reduced form model of the determination of the share of state personal income per capita that was accounted for by specific metropolitan areas. The paper used data from 43 metropolitan areas over the period 1969-1994. The empirical model contained indicator variables for both the number of professional sports franchises and facilities, as well as the location of the facilities relative to the Central Business District. The paper concludes that the share of real state personal income attributable to the cities in the sample was larger for cities with two or more sports facilities and teams located in the Central Business District relative to cities with one professional sports team, but that the share of real state personal income attributable to the cities in the sample was smaller for cities with two or more sports facilities and teams located in the suburbs relative to cities with one professional sports team.[8]

Similarly, Santo[9] (2005) examined the economic impact of professional sports on the local economy. He posited that:

> Theoretically, a retro-style ballpark in a downtown or retail setting is likely to attract visitors from a wider area than its more utilitarian suburban counterpart, and is likely to induce longer stays and greater ancillary spending. If so, it is plausible that the new generation of sports facilities would have more favorable economic impacts than their predecessors. (Santo 2005, 180)

Santo utilized methods identical to Baade and Dye (1988) but extended the data through 2004 and dropped all data prior to 1984. His regressions explained variation in income, or the city's income as a share of regional income, using population, a time trend, and variables that indicated years following the construction of either a football only or baseball only stadium. Positive and significant coefficients on the stadium variables were interpreted as evidence of the importance of context.

While both Nelson and Santo conclude that downtown stadiums have beneficial impacts on their cities, few economists are convinced by their results. In both instances, demonstrating that a downtown stadium raises the share of state or region income that accrues downtown may be evidence that a sports facility redistributes income away from the rest of a state or region and concentrates it in the downtown area of a major city. Those who find no economic impact

7 We do not know Nelson's discipline. He is a Fellow of the American Institute of Certified Planners and a Professor of Urban Affairs and Planning.

8 Wassmer (2001) suggests Nelson's results may indicate a healthy central city that helps build a healthy metropolitan area.

9 Santo's Ph. D. is in Urban Studies and he is employed in a Department of City and Regional Planning.

of franchises and stadiums in the local economy argue that one explanation for those results is the redistribution of spending and income. Both Santo (2005) and Austrian and Rosentraub[10] (2002) indicate that pure redistribution effects from stadiums and arenas are, or should be, acceptable from a public policy perspective if the stadium or arena induces or enhances redevelopment of an area that needs redevelopment. They suggest, in fact, that the emphasis on improvements in general economic well-being from stadiums and arenas should be replaced by this redistributive focus despite the unclear welfare implications.

In addition, these studies suffer from methodological problems. First, identifying "downtown" and "suburban" sports facilities involves a great deal of researcher discretion, as the US Bureau of Economic Analysis ceased identifying the Central Business District in US cities in the early 1980s. Second, Coates and Humphreys (1999) pointed out the econometric problems inherent in transforming the dependent variable, rather than using additional control variables, to account for the presence of unobservable factors in reduced form models of local income or employment determination.

Recently, economists have turned to more disaggregated data on local tax revenues and hotel occupancy rates in their search for evidence that professional sports franchises and facilities generate positive economic benefits. Lavioe and Rodriguez (2005) examined monthly hotel occupancy rates in eight Canadian cities over the period 1990-1999 using univariate time series analytic techniques. Their paper contains weak evidence that hotel occupancy rates were lower during the 1994 NHL lockout, but this result is undermined by an estimated positive impact on hotel occupancy associated with the departure of an NHL franchise and a finding of no impact on hotel occupancy rates associated with the arrival of two new franchises in this group of Canadian cities.

Mega Events

Much of the emphasis in the early work on the effects of stadiums and arenas on local economies was on their job and income creation effects, with the intent of assessing the value of subsidies for those facilities. A closely linked body of research addresses the impact of large, infrequent, events on the local economy. This mega-events literature attempts to measure the benefits to communities that host events such as the National Football League's Super Bowl, Major League Baseball and National Basketball Association All-Star Games, National Collegiate Athletic Association basketball's regional tournaments and the Final Four, and even the Olympic Games. The linkage of the stadium literature to this mega-event literature is two-fold. First, it has become common for the professional sports leagues to offer to cities the hosting of one of the premier events as an

10 Austrian holds a Ph. D. in economics. Both Austrian and Rosentraub work primarily in Schools of Urban Affairs or Public Policy.

inducement to build a new facility for the local team. Second, promoters of these events tout the beneficial economic impact they will have on the host cities.

Porter (1999) may have been the first academic economist to carefully examine the effects of Super Bowls on local communities. His evidence, based on an analysis of Super Bowls in Florida and Arizona, indicated that hosting the Super Bowl had no detectable impact on the taxable sales of the host city. As previously mentioned, Coates and Humphreys (2002) also found no effect of hosting a Super Bowl on the level of real per capita personal income in a metropolitan area. Baade, Baumann, and Matheson (2008) examined taxable sales in Miami, Tampa, and Orlando, Florida for the effects of hosting Super Bowls and All-Star Games, and having local teams participate in the World Series or other league championships. The only event to have a consistent effect on taxable sales during their time period, 1980 through 2005, was Hurricane Andrew. The sporting mega-events had little effect on taxable sales, but on average may have reduced the sales.

Hotchkiss, Moore, and Zobay (2003) found that hosting the 1996 Summer Olympics boosted employment in Georgia counties where Olympic activities were held and in those counties close to the events. They conclude that evidence of wage increases is too weak to draw reliable inferences. Their estimates are, however, somewhat sensitive to the choice of when the Olympic effects begin. For example, do those effects begin as soon as Atlanta is announced as the host (September 1990), when the Olympics are held (August 1996), some time in between the announcement and the event, or do they begin only after the event? Their results are strongest if the Olympic effects are dated from 1994 for employment and 1995 for wages. Madden (2006) used a computable general equilibrium model to analyze the impact of the 2000 Summer Olympics in Sydney, Australia on the entire Australian economy. He finds that there may be a modest beneficial impact for the state hosting the games, which may come at the expense of the other states. Leeds (2007) examined the impact on Colorado ski resorts of the Salt Lake City Winter Olympics. His argument was that skiers whose access to Utah's ski resorts was blocked by the Olympic Games may have substituted trips to Colorado's nearby resorts. His evidence is consistent with some degree of substitution, suggesting that when one jurisdiction hosts a mega-event, that other jurisdictions may benefit from the displaced visits. In other words, the net gain to a host community may be smaller than is typically assumed. Porter and Fletcher (2008) studied the impact of the Salt Lake City Olympics. They found that, relative to the same time period in non-Olympic years, neither hotel occupancy rates nor the number of arriving air passengers exhibited any increase. Only the room rental rates at local area hotels were higher for the period of the Olympic Games.

Research also has turned to the examination of influences on tax collections. Coates (2007) analyzed monthly sales tax data for Houston, Texas over a period of about 15 years that included both the 2004 Super Bowl and the 2004

MLB All-Star Game. The results indicated that the Super Bowl did raise sales tax revenues in Houston, by about $4 million, but that the MLB All-Star Game did not. Coates and Depken (2006) expanded the analysis to all the jurisdictions in Texas that hosted professional sports, football, basketball, baseball, hockey, soccer and auto-racing, Division I college football, and several professional and collegiate mega-events. Their analysis also finds a large sales tax revenue impact of the 2004 Super Bowl, but events like individual regular season games and even playoff and championship series games have modest or even negative effects on sales tax collections.

Conclusion

We offer an argument against sports subsidies based on economic intuition, survey evidence that a majority of economists believe that sports subsidies are unwarranted, and a review of the existing literature on the economic impact of professional sports. Although the intuitive argument and survey evidence do not deny the possibility of certain local economic benefits from sports subsidies, the empirical findings also strongly reject sports subsidies on the grounds of a lack of economic benefits. The large and growing peer-reviewed economics literature on the economic impacts of stadiums, arenas, sports franchises, and sport mega-events has consistently found no substantial evidence of increased jobs, incomes, or tax revenues for a community associated with any of these things. Focusing our attention on research done by economists, as opposed to that of scholars from public policy or urban development and planning departments, we find near unanimity in the conclusion that stadiums, arenas and sports franchises have no consistent, positive impact on jobs, income, and tax revenues. If professional sports franchises and facilities do not have any important positive economic impact in the local economy, then subsidies for the construction and operation of these facilities are even more difficult to justify.

We have not discussed the growing literature that attempts to quantify supposed external or "intangible" benefits. For example, if a franchise provides a community with non-rival and non-excludable (public goods) consumption benefits, the value of those benefits may be reflected in local income or employment.[11] However, it is likely to show up in willingness to pay for homes and rents in the community in the same way that other amenities and disamenities are capitalized into fixed asset prices and rents. Consequently, researchers have turned to hedonic methods to determine the value of sports franchises to a community. In a similar vein, an active research agenda using contingent valuation methods to

11 However, Coates and Humphreys (1999) say one explanation for their finding of negative effects of the sports environment on personal incomes is that lower incomes are a compensating differential for access to professional sports.

analyze sports teams and stadium projects has recently emerged. In these studies, individuals are surveyed about their willingness to pay for hypothetical situations, such as purchase of a franchise or stadium renovation, and the survey answers used to estimate willingness to pay. Finally, researchers have estimated the consumer surplus from game attendance to assess the benefits of a stadium or arena. All these approaches address important dimensions of the benefits a community may attain from a sports franchise, or a new stadium or arena. These studies may shed light on the issue, but again, in our judgment, economic intuition provides a strong rationale that any supposed local external benefits would not justify sports subsidies.

We have seen that economists in general, as represented by Whaples's survey (2006), oppose sports subsidies. Economists reach the nearly unanimous conclusion that "tangible" economic benefits generated by professional sports facilities and franchises are very small; clearly far smaller than stadium advocates suggest and smaller than the size of the subsidies. The fact that sports subsidies continue to be granted, despite the overwhelming preponderance of evidence that no tangible economic benefits are generated by these heavily subsidized professional sports facilities, remains a puzzle.

Rent-seeking generates powerful incentives for people like professional sports team owners and professional athletes to divert public money into their pockets. Elected officials are especially susceptible to flattery from professional athletes, and these officials are also keenly aware of the political value of keeping the local team in town regardless of the underlying cost-benefit calculus. These explanations, along with simple collective foolishness when it comes to matters of the heart like sports, have considerable explanatory power. Moreover, the implicit and explicit anti-trust protection extended to North American professional sports leagues probably contributes to the ability of team owners to extract subsidies from local governments. At any rate, we seem to have reached the classic paradox in which economists reach a conclusion but are unable to make economic wisdom decisive in public policy decisions.

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About the Authors

Dennis Coates is a Professor of Economics at UMBC. He is on the editorial boards of several journals including *Journal of Sport Management*, *International Journal of Sport Finance,* and *Journal of Sports Economics,* for which he is the Book Review Editor. His work has appeared in a variety of academic journals including the *Journal of Policy Analysis and Management*, the *Journal of Sports Economics*, the *Southern Economic Journal*, and *Regional Science and Urban Economics*. His work on stadiums has been discussed in the *Washington Post*, *Business Week*,

and *Sports Illustrated*. He is a frequent guest on television and radio programs concerning stadiums and professional sports and was elected to be President of the North American Association of Sports Economists.

Brad Humphreys is an associate professor in the Department of Economics at the University of Alberta where he holds the chair in the economics of gaming. He is a co-editor of *Contemporary Economic Policy* and associate editor of the *International Journal of Sport Finance*. He serves on the editorial board of the *Journal of Sports Economics* as well as several sport management journals. In April 2007 he testified before the United States Congress on the economic impact of professional sports franchises and facilities.

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Econ Journal Watch,
Volume 5, Number 3,
September 2008, pp 316-348.

Intellectual Tyranny of the Status Quo

Colleagues, Where Is the Market Failure? Economists on the FDA

Daniel B. Klein[1]

Abstract

According to the Food and Drug Administration (FDA), some 20 cents of every consumer dollar purchases products that come under the purview of the FDA. It regulates the safety of America's food supply and cosmetics, the safety and effectiveness of pharmaceuticals and medical devices, and the claims that can be made about these and other products.

This article treats three FDA-administered restrictions:

- The permitting of new drugs and devices
- The control of manufacturer speech
- The imposition of prescription requirements.

This article works from a point of view holding that there is no market-failure rationale for these three interventions. The implications of that view are very much at odds with the common and official cultural attitudes about the matter. This article is a cultural analysis of the economic discourse on the issues. It explores how economists approach and discuss an enormous, entrenched apparatus that basic economic reasoning properly condemns as a bane to humanity.

Survey evidence strongly suggests that the modal economist is somewhat supportive of the extant regulations, but this paper focuses on a narrow subset of economists: those who express in print judgments on such matters—a group sometimes referred to here as FDA-expressive economists. This paper works exclusively from such published statements. Many economists have expressed

1 Professor of Economics, George Mason University, Fairfax, Virginia 22030
I am very grateful to Michael F. Cannon for his significant contributions to the knowledge accumulated here. For valuable comments on earlier drafts, I thank Niclas Berggren, Jason Briggeman, Randall Holcombe, Alex Tabarrok, and three anonymous *EJW* referees.

judgments concerning the three controls. In almost all cases, they support liberalization, often dramatic. Appendix 1 contains bulky compendia evincing the pro-liberalization judgments. The confounding cases are treated in the text.

The first-order finding—that economists judge very preponderantly for liberalization—is just a preliminary to the main business of the paper. This paper is chiefly concerned with considerations of the second-order. Do economists agree either that economists or that fundamental economic reasoning favor liberalization of the restrictions? In fact, in either variation, they *do not* agree. That is, while the first-order investigation finds that economists' judgments very preponderantly favor liberalization, the second-order investigation finds that some economists deny that liberalization is strongly supported by "economics" –interpreted either as economists' judgment or as economic reasoning as such.

I suggest that taboos surround the issue, particularly taboos against the critical examination of fundamentals. I contend that there is no market-failure rationale for the restrictions. Many FDA-expressive economists exhibit a sort of intellectual schizophrenia. In their heart of hearts, they seem to agree that there is no respectable market-failure rationale. I explore the rhetoric of their writings and the political sociology surrounding such research.

FDA-expressive economists often employ a rhetoric of quantitative cost-benefit analysis. Rhetoric that suggests that a quantitative argument is necessary to arrive at judgment bypasses the question of whether the policies have any respectable market-failure rationale. In bypassing such a fundamental question, the economist effectively presupposes that there is a respectable rationale out there somewhere, and that, therefore, quantitative analysis is necessary to determine whether benefits outweigh costs. I suggest that such bypassing is economic misfeasance.

When market-oriented economists suggest that a quantitative argument is the most serious—or even the only serious—basis for challenging the policies, they are implicitly surrendering the basis that I, for one, consider the most important: Thinking through whether there is any respectable market-failure rationale. I specify several reasons why quantitative argument is very valuable, but I nonetheless maintain that in this case it is not necessary to judge against the observed policies. Because I believe that such scrutiny finds no respectable market-failure rationale, I see fault in economists' insisting that quantitative arguments are necessary.

I then move on to some thoughts on the nature of the quantitative argumentation. I contend that the quantitative argumentation we see on the issue would be best presented as *a fortiori* argumentation, rather than net-benefit calculation. I suggest that *net-benefit calculation* is chimerical. The empirical investigations do not really undertake net-benefit calculations. I suggest that they should not pretend to do so, and that they should not aspire to do so.

I explore why leading experts favorable to liberalization might be reticent

to examine and discuss fundamental presuppositions, or to express their views openly. One point I make is that if an economist confronts fundamentals and lets on that he sees no respectable market-failure rationale, he then implies that any quantitative investigation is merely illustrative, or merely helps in quantifying the net-costs of the policy. That is, any such quantitative investigation is merely illustrating something that is learned from scrutiny of fundamentals: that water runs downhill—and perhaps giving a better idea of how much water, or how fast. But that it runs downhill was—in this case—decided by thinking through fundamentals. I suggest that openly acknowledging such implications might insult the vanity of some of the players and communities involved—both of some of the quantitative researchers and of some officials and others enmeshed in conventional (or establishment) political culture.

Economists' Judgments In Favor of Liberalization

"Economist." I count someone as an economist if he or she has a graduate degree in economics, has been a college-level economics professor, or has had a job with the title "economist." Qualification details are reported in the Excel file of Appendix 2 (link). Every person listed in Table 1, 2, and 3 counts as an economist. As this paper is about the judgments of economists only, many non-economists who have published judgments on the issues have simply been passed over.

Judgment Tabulations. Table 1 shows the result of an extensive search for economists' judgments about FDA permitting of new drugs and medical devices. The search is one I've been making for about 12 years, during which time I've assiduously collected judgments by economists on the matter. I admit that the search lacks any systematic method. But bias in the search can be exposed and corrected. Below I propose an open-source method for doing so.

Every cell in Table 1 is pro-liberalization. The cells vary by degree of liberalization and definiteness of the judgment. Every economist in Tables 1, 2, or 3 is quoted in Appendix 1 (link). For each cell, there is a corresponding set of quotations. The compendium strives for sufficiency in economists' judgments, not in each's expressions of such. That is, for each listed economist the relevant compendium of quotations in Appendix 1 contains samples that are sufficient to establish his judgment, but does not aim to cover all of his expressions of that judgment.

Table 1 shows 35 economists favoring liberalization. Surely, the table is quite incomplete. But the argument is one of sufficiency. Omissions that would matter would be ones that oppose liberalization. The few confounding judgments that I have found are collected and addressed below.

Table 1: FDA Permitting of New Drugs and/or Devices: Economists' judgments about liberalizing, by definiteness and minimum degree of liberalization advocated.

	Definite	Fairly Definite	Mild
Significant liberalization supported	Gary Becker Noel Campbell Milton Friedman Dale Gieringer David R. Henderson Robert Higgs Randall Holcombe Daniel Klein Sam Peltzman Paul Rubin Russell Sobel Alex Tabarrok Robert Tollison Kip Viscusi Michael Ward Walter Williams	Henry Grabowski Charles Hooper F.M. Scherer John Vernon	
Liberalization supported, but *not explicitly significant* liberalization	J. Howard Beales Ernst Berndt John Calfee Tomas Philipson Eric Sun Murray Weidenbaum	Ronald Hansen Joseph Harrington David Schwartzman Meir Statman Peter Temin Steven Wiggins	Charles Phelps David Dranove David Meltzer
	Total: **35** Economists judge in favor of liberalization in the permitting of new drugs and/or devices.		

Documentation: See quotation compendia of Appendix 1 (link).

The next two tables follow the same scheme. Table 2 shows the results for FDA permitting of manufacturer speech about their products, in advertising, labeling, packaging, or promotion. It shows 12 economists favoring liberalization.

Table 2: FDA Speech Restrictions: Economists' judgments about liberalizing, by definiteness and minimum degree of liberalization advocated.

	Definite	**Fairly Definite**	**Mild**
Significant liberalization supported	John Calfee Milton Friedman David R. Henderson Alison Keith Daniel Klein Keith Leffler Paul Rubin Russell Sobel Alex Tabarrok		
Liberalization supported, but *not explicitly significant* liberalization			Ernst Berndt Davina Ling Margaret Kyle
	Total: **12** Economists judge in favor of liberalization in manufacturer speech.		

Documentation: See quotation compendia of Appendix 1 (link).

Suppose a drug has been permitted by the FDA. Prescription requirements say that retailers cannot sell the drug without the buyer presenting a doctor's prescription. The FDA decides whether the drug will be prescription-only or "over the counter." Table 3 shows the results about FDA imposition of prescription requirements. It shows 8 economists favoring liberalization.

Table 3: FDA Prescription Requirements: Economists' judgments about liberalizing, by definiteness and minimum degree of liberalization advocated.

	Definite	Fairly Definite	Mild
Significant liberalization supported	Milton Friedman Daniel Klein Russell Sobel Alex Tabarrok	F.M. Scherer	
Liberalization supported, but *not explicitly significant* liberalization	Kathleen Johnson Shirley Svorny	Sam Peltzman	
	Total: **8** Economists judge in favor of liberalization in prescription requirements.		

Documentation: See quotation compendia of Appendix 1 (link).

Apparent Anti-Liberalization Judgments

I have assiduously searched for and collected scholarly judgments suggesting an anti-liberalization view. Here we examine cases of economists.

Paul Krugman. In a *New York Times* column of March 22, 2000, Krugman wrote about genetically modified foods and favored liberalization on that matter. But he set up the point with remarks on the dietary supplements industry. He said dietary supplements "are known to pose big health risks—but nobody knows how big, because lobbying by the industry has blocked effective regulation, testing and even reporting. . . . There is extensive evidence that dietary supplements can, if misused, be quite dangerous. [Omitted here is a brief alarmist quotation from a *Washington Post* survey.] But a 1994 law specifically exempts supplements from almost all federal regulation, including the need to report adverse effects."

That's it. Krugman seems to be favoring FDA regulation of dietary supplements, but doesn't say whether "effective regulation" would look like traditional FDA controls. Again, Krugman was setting up his objections to illiberal attitudes and policies on genetically modified foods, which is the main point of the column.

A second item comes from Krugman, again in the *New York Times* (May 21, 2007). It is about food safety, not drugs, but it contains three sentences that refer to FDA drug controls:

> Who's responsible for the new fear of eating? Some blame globalization; some blame food-producing corporations; some blame the Bush administration. But I blame Milton Friedman. …
>
> The economic case for having the government enforce rules on food safety seems overwhelming. Consumers have no way of knowing whether the food they eat is contaminated, and in this case what you don't know can hurt or even kill you. But there are some people who refuse to accept that case, because it's ideologically inconvenient.
>
> That's why I blame the food safety crisis on Milton Friedman, who called for the abolition of both the food and the drug sides of the F.D.A. What would protect the public from dangerous or ineffective drugs? 'It's in the self-interest of pharmaceutical companies not to have these bad things,' he insisted in a 1999 interview. He would presumably have applied the same logic to food safety (as he did to airline safety): regardless of circumstances, you can always trust the private sector to police itself.
>
> O.K., I'm not saying that Mr. Friedman directly caused tainted spinach and poisonous peanut butter. But he did help to make our food less safe, by legitimizing what the historian Rick Perlstein calls 'E. coli conservatives': ideologues who won't accept even the most compelling case for government regulation. (Krugman 2007)

Jerome Rothenberg. In 1993, Rothenberg published a review essay of Aaron Wildavsky's highly libertarian book, *Searching for Safety* (1988). The review essay is a broad discussion of safety issues; it is not specifically on the FDA. In the article (180) he describes himself as "considerably less optimistic" than Wildavsky and clearly expresses judgments in support of restrictions:

> Perceptibly safer versions of a commodity, or commodities that can protect users against predictable hazards, will be profitable and hence

likely to be produced through competitive pressures. Even some forms of precautionary information--for example, safety ratings on consumer goods—will be generated by the market. But these will be inadequate where product performance is hard to monitor by users, where hazards are not widely or accurately perceived, or where people do not realize that they are uninformed. (Rothenberg 1993, 166)

Market processes do provide a partial corrective to consumer ignorance where hazard identification is relatively straightforward. When consumers suspect they are dangerously uninformed about private commodities, they may be willing to purchase product information as though it were a private commodity itself. Entrepreneurs may then profit from selling such information, their success depending on the perceived relevance and accuracy of the information they provide. *Consumer Reports* and *The Medical Letter* are notable examples of such success. But such instances are in fact rare. The more likely sources of such precautionary information are governmental product safety programs, scientific studies, and journalistic exposés. (171-72)

The market's myriad decentralized actions do not themselves ensure adequate safety. Centralized controls of various sorts are needed. These have been instituted in the form of regulations, constraints, information programs, licensing and certification. (172)

But if one does not know how to recognize 'beneficial' new drugs dependably without screening, and if the purpose of screening, with all its delays, is to point the way to devising new drugs that are *more* beneficial, then introducing 'beneficial' new drugs without screening has a possibly serious indirect opportunity cost—namely, the delayed development and introduction of even more beneficial drugs—in addition to the direct cost of unnecessarily high side effects.

The net weight of both sets of direct and indirect costs is not easy to gauge empirically, so Wildavsky makes his case with examples rather than solid quantitative evidence. How pervasive are well-publicized cases of apparently unwarranted delay? We certainly do not have quantitative estimates of the damage that has been directly prevented by screening, and indirectly by the incentives screening creates. More extensive, tightly analyzed data are required before we can make a trustworthy judgment about the issue. Wildavsky's anecdotal evidence is insufficient. (175-76)

Rothenberg never identifies a market-failure rationale for the interventions. Citing only works by Peter Temin and Victor Fuchs, he demonstrates little acquaintance with economic research on the FDA. He shows no acquaintance with the myriad ways that private practices and institutions certify and assure medical products, such as a doctors' prescription, nor that off-label medicine functions in a realm of efficacy assurances largely disconnected from FDA efficacy certifications. He never mentions that most drugs are prescription-only. Also, the essay does not note that voluntary practices –which of course include "scientific studies" and "journalistic exposés"—will be distorted by the banned-till-permitted system with a monopoly permitter.

Patricia Danzon and **Eric Keuffel**. Danzon is one of the most prolific economic researchers of the pharmaceutical industry, focusing especially on issues of pricing, price controls, liability, insurance, and drug development. In work with a doctoral student in Health Care Systems Eric Keuffel, she has recently expressed views that must be counted as opposing significant liberalization of the issues examined here:

> Thus in our view, the case remains strong for a regulatory agency such as the FDA to establish minimum standards of safety, efficacy and quality as a condition of market access. However, the optimal integration of post-launch data with the prelaunch [randomized controlled trials] data remains an important issue to be resolved. (Danzon and Keuffel 2007, 28)

> There is a strong argument that structuring and interpreting such data analysis is a public good that is best delivered by an expert regulatory agency. The existence of regulatory systems to perform these functions and control market access in all industrialized and most developing countries is strong evidence for consensus opinion on this basic proposition. (82)

Open-Source Scholarship: Test the Foregoing Analysis

Again, the only apparent anti-liberalization statements that I have found are those of Krugman, Rothenberg, and Danzon and Keuffel. Perhaps I have been biased in my search or presentation. The present journal invites communications that provide other apparently anti-liberalization statements by economists, as well as communications that contend that any of the judgments tabulated in Tables 1, 2, and 3 and compiled in Appendix 1 have been taken out of context, misrepre-

sented, or wrongly categorized. All such communications will be gathered up and shared in a future issue of the journal. As for additional pro-liberalization statements by economists, those are welcome as well.[2]

Denials of Conclusion

John Calfee (2000, 25-31) has explained that if a firm develops a good new drug and it gets permitted, the potential benefits will go unrealized if patients and doctors never recognize it. Calfee further points out that confidence in future recognition is crucial, in the first instance, to creating the new drug.

Similarly, if an economist develops a good piece of economic discourse, the potential benefits will go unrealized if others never recognize it. If the cultural ecology of economics fails to recognize good economic discourse, benefits will go unrealized and, in the first instance, the development of good discourse will be discouraged. One factor inhibiting the realization of potential benefits is silence. Another is denial.

The first-order question has been: Do economists' judgments constitute a consensus on reforming the FDA? That question, I maintain, is answered quite decisively in the affirmative, and the consensus is for liberalization.

Now we turn to two related second-order questions:

> *Second-order question A*: Do economists agree that economists reach a conclusion in favor liberalization of the restrictions in question?
>
> *Second-order question B*: Do economists agree that there are no respectable market-failure rationales for the restrictions?

If all FDA-expressive economists agreed that there is no respectable market-failure rationale, then they would presumably agree that economists favor liberalization.[3] Hence, the two questions are related. Both A and B are worthy of investigation, and they could be investigated separately. I find that at the second-order, in either version of the question, economists do not agree. As I think that some economists are getting it wrong in either version, my treatment tends to combine the two questions, rather than take pains to keep them separate.

Some economists seem to suggest that economists do reach a conclusion on FDA policy. For example, in their well-known textbook, *Economics of Regulation*

2 Send communications to dklein@gmu.edu. Please provide full quotations, complete citation information, and information about the economist qualifications of the quoted authors.

3 I must write "presumably" because, even if all FDA-expressive economists agreed that there is no respectable market-failure rationale, they might not all agree that such economists favor liberalization, either because some of those economists falsify their views on the matter, or because some of them either misread or misrepresent the record of expressed views.

and *Antitrust*, Viscusi, Harrington, and Vernon (2005) write: "Although a few critics have charged that the FDA has been too lax, the consensus in the economics literature is that the FDA has placed too great an emphasis on Type II errors" (795). In other words, the consensus favors liberalization.

Others, however, deny that economists reach a conclusion. Here we present statements by economists suggesting that economic research has not arrived at any definite judgment in favor of liberalization. Some of the economists treated here are ones who have expressed judgments in favor of liberalization and are listed in Tables 1, 2, and/or 3—to wit, Ernst Berndt, David Dranove, David Meltzer, Tomas Philipson, and Eric Sun. There is no inconsistency in saying (a) liberalization is desirable, and (b) economic reasoning and research does not decisively favor liberalization. I think (b) is wrong, but it is not contradicted by (a).

An economist may maintain that income taxes should be lower and that economic analysis does not decisively support that conclusion. Analogously, Adam Smith (1790, 175, 327) distinguished between grammar-like rules, that are "precise and accurate," and aesthetic-type judgments, that are inherently "loose, vague, and indeterminate." In judging a matter of the latter type, for example, in ranking movies by quality, we might all put *Back to the Future* over *Bride of Chucky*, but not insist that our judgment rides a force of grammar—which flatly declares that "He goes" is proper and "He go" is not.

Perhaps it would be useful to extend Smith's distinction to anticipate some of what follows in the present article. We have seen that most FDA-expressive economists favor liberalization. But I will criticize those who suggest that there is no strong grammar behind that conclusion. I will suggest, first, that there is a "grammatical" force behind the conclusion, and, second, that to treat the matter as looser than it really is, or to overstate the role of empirical artfulness in judging the policy issue, is to fail to apply political-economy grammar when it ought to be applied,[4] and hence to fail to strengthen the norm within economics of following such grammar when it applies.[5]

4 The poltical-economy grammar that I invoke is a liberal grammar in the sense that it puts aside certain aspects of welfare, in particular, the gratification of bents that would favor the restrictions for reasons that one might frame as identity or cultural externalities. In fact, many people favor the restrictions because, as they might say, the regulations affirm community or social responsibility, restrain greed, or restrain or subvert capitalistic or neoliberal culture. Such aspects play a subterranean role in the setting and subtext of some of the discourse about FDA policy, but, at least among economists, such aspects are never explicitly recognized as helping to justify the restrictions. Until those aspects are put squarely onto the table I think it is appropriate to invoke a political-economy grammar that puts them aside. When they are put openly onto the table, the conversation becomes less grammar-like and more art-like.

5 By the way, the "political-economy grammar" to which I allude would be Quinean (and, I would argue, Smithian and Coasean) in the worldly nature of its presumptive authority, the warrant residing neither in "deduction" nor "induction" (Quine 1961). In particular, I do not see the presumptive authority of the grammar as deriving from axioms or "first principles"

William S. Comanor. The AEA's outreach and review organs, the *Journal of Economic Literature* and the *Journal of Economic Perspectives* are crucial nodes in the ecology of economic culture, and hence of the political culture at large. On the FDA topic, the record of those two journals has been disturbing. Only two pieces on the topic have ever appeared. The first was in *Journal of Economic Literature* in 1986, entitled "The Political Economy of the Pharmaceutical Industry," by William S. Comanor. He addresses the then extant work on the suppression effect, delays, the international drug lag, and drug promotion. He makes some loosely pro-liberalization remarks, such as: "There is wide acceptance of the general depressing effects of regulation—in the political arena as well as among economists. And studies of pharmaceutical regulation have, for the most part, followed that trend" (Comanor 1986, 1210-11). His review suggests, however, that the suppression effects have been overstated. He concludes his piece with the following sentences:

> Although the existing economic literature on the pharmaceutical industry has provided a wealth of detail regarding its structure and performance, it has not supplied the research findings needed to permit accurate judgments on the critical issues for public policy. Perhaps this is due to the ways in which this literature has interacted with the continued political debates. There remain important tasks to be done. (Comanor 1986, 1214-15)

David Dranove and David Meltzer. These authors provide useful evidence that more important drugs are developed and approved more quickly. At the conclusion of their study they say: "Of course delays in approval also have their costs, and appropriate policies must weigh the costs and benefits of accelerated testing and approval on a case-by-case basis depending on the drug's specific risks, benefits, and difficulty of testing" (Dranove and Meltzer 1994, 422), seemingly rejecting—or at least failing to affirm—the idea that economists should have a strong presumption against the withholding of permission, regardless of a drug's risks, benefits, and difficulty of testing.

Berndt, Ernst R., Adrian H. B. Gottschalk, Tomas Philipson and **Matthew W. Strobeck**. These authors write:

> A central tradeoff facing the FDA involves balancing its two goals—protecting public health by assuring the safety and efficacy of new drugs, and advancing the public health by helping to secure and speed access to new innovations. Although little quantitative evidence has been produced on this central tradeoff, some observ-

(just as English grammar does not find its presumptive authority from first principles).

> ers have argued that the FDA is not taking enough time evaluating new drugs and biologics, while others have argued that the agency is taking too long in doing so. Little empirical evidence has been put forward to make the case that the FDA is too slow or too fast in its drug approval process, *partly due to significant difficulties in measuring the costs and benefits of greater speed.* (Berndt et al 2005, 1; italics added).

Elsewhere, the same authors say:

> [S]urprisingly, very little quantitative empirical evidence has been put forward to evaluate the degree to which the speed and safety tradeoff facing the FDA is being resolved efficiently. More generally, *there seems to be no suggested quantitative methodology or framework for assessing the economic efficiency of the central speed-safety tradeoff of the agency.* (Philipson et al 2005, 3; italics added)

Philipson and **Eric Sun** writing in the *Journal of Economic Perspectives* say that speed of new product approval was underprovided prior to the reform of 1992 and add: "although more analysis would be needed to see *whether additional gains in speed at the expense of drug safety might be worthwhile*" (Philipson and Sun 2008b, 99; italics added).

Patricia Danzon and **Eric Keuffel** (2007) write that "the only significant attempt to weigh both the benefits and costs of the 1962 Amendments is Peltzman's (1973) study" (22).

Regarding direct-to-consumer advertising (DTCA), they write:

> [D]rawing welfare conclusions from the empirical evidence is particularly problematic. The economic/marketing literature generally views advertising that expands aggregate category sales as more likely to be informative, and hence welfare-enhancing, whereas advertising that simply changes market shares without affecting aggregate use is more likely to be wasteful … However, in the case of heavily insured pharmaceuticals, for which consumers pay only a small fraction of the cost out-of-pocket, it is possible that even category-expanding effects could reflect unnecessary use (and/or unnecessarily costly use), even though such purchases are well-informed and rational for individual consumers, given their insurance coverage. (Danzon and Keuffel, 2007, 76)[6]

6 I don't think the subsidization rationale for restrictions on direct-to-consumer advertising withstand scrutiny, but I refrain from digressing on the matter.

> The existing evidence on effects of DTCA is mixed, with quite strong evidence for category expansion and weaker evidence for improved compliance and product specific benefits. *Effects on patient outcomes and on competition and overall costs have not been measured. Thus several of the components of a full welfare analysis remain to be developed.* (Danzon and Keuffel 2007, 86; italics added)

Anonymous *Journal of Economic Literature* referee. In late 2001, I submitted to the *Journal of Economic Literature* a proposal to write a literature review on the FDA that would organize the studies and evidence by natural-experiment comparisons and would suggest that the various analyses point to liberalization. In using this material here, I am probably uncollegial, but it is important to see the reasons actually given at the crucial moments at the crucial nodes of the economic culture, and unseemliness would seem to be the only way to bring the evidence to light.

One of the two referees wrote:

> I do, however, have a number of problems with the outline, some serious, I believe. The most fundamental is the one of *measuring welfare or consumer surplus in this context. In particular, I think most of us would agree that standard measures, like integrating under demand curves, are problematic here, due to the informational asymmetries and the agency issues.* Furthermore, there are obvious econometric problems with looking for changes in morbidity and mortality arising from changes in regulation regime. This paper is mainly a literature review, so you might think it's unreasonable for me to suggest that you tackle this question. However, *your thesis rests entirely on it* and *most of the literature you review is agnostic on the question of welfare* (for the reasons I mentioned above). How, for instance, would a finding that some regulation had decreased the rate of drug innovation or reduced use of a drug be interpreted to support your thesis of 'overregulation' without a discussion of welfare? (Anonymous *JEL* referee 2002; italics added)

The referee seems to be saying that to arrive any firm judgment about the restrictions we need a net-benefit calculation, and we haven't yet figured out how to measure welfare. It seems that the then-editor of the *JEL* John McMillan reasoned along similar lines. In his cover letter rejecting the proposal he wrote:

> Both [referees] say they are predisposed to agree with your position on the FDA (one says this in the report, the other in a cover letter

> to me). But both say the evidence, as it currently exists, doesn't stand up to scrutiny. Both say, also, that there are methodological problems that will have to be addressed before the literature gets to be something more than advocacy. That being the case, it is premature to consider running a *JEL* article on this topic. (McMillan 2002)

Jerome Rothenberg. Above I quoted from Rothenberg to show that he seems to oppose liberalization. The material quoted also contains remarks to the effect that "anecdotal evidence is insufficient" and that policy judment must await "solid quantitative evidence" (Rothenberg 1993, 176).

There Is No Market-Failure Rationale

I contend that the longstanding banned-till-permitted policies[7] have no market-failure rationale. If that contention is correct, the implications are intriguing and far reaching, particularly as regards the economic literature on the FDA. Now I offer a brief case for the contention.

Uncertainty engulfs us especially in matters of health and treatment. Some might say that such uncertainty makes us child-like. But the "child" metaphor holds water only if there is a "parent" counterpart. A rationale for the observed restrictions would need to assert, at least somewhat plausibly, that what amounts to FDA veto power somehow corrects systematic erring in the face of such grave uncertainty. But no grounds are ever offered for any such systematic erring, much less for the corrective effect of FDA veto power.

Litigation weighs heavily on product safety and medical treatment. More importantly, people have demands for ex ante assurance of quality and safety, and those demands give rise to supplies.[8] Reputation is but one form of assurance, and it suffers by product recalls (Jarrell and Peltzman 1985). Assurance and litigation do not work perfectly. But there is no theory contending that they err systematically AND that the longstanding policies constitute any plausible correction to such erring. A defense of government intervention must open with a rationale and proceed to a reasonably well-rounded case—a case that recognizes important costs and the imperfections of the alternative arrangement. As regards the longstanding policies, we do not find even a coherent opening rationale, much less a well-rounded case.

Consider some of the articulations of market-failure rationales found in the literature. Danzon and Keuffel (2007, 11) articulate the arguments for proof-of-efficacy:

7 I use this phrasing because Congress, not the FDA, imposed the bans. The FDA decides whether to give or withhold permissions. To be sure, it can give permissions more freely, but it can not repeal the bans.

8 On the demand and supply of assurance see Klein (2002).

> The presumption underlying the requirement for proof of efficacy was that imperfect and possibly asymmetric information prevented physicians and consumers from making accurate evaluations, leading to wasted expenditures on ineffective drugs and excessive product differentiation that undermined price competition.
>
> More generally, they write:
>
> The rationale for heavy regulation of pharmaceuticals is not intrinsic natural monopoly, since any market power enjoyed by individual products derives ultimately from government-granted patents. Rather, regulation of market access, manufacturing and promotion arise because product efficacy and safety can be critical to patient health but are not immediately observable. Evaluating safety and efficacy as a condition of market access and monitoring manufacturing quality and promotion accuracy over the product life-cycle are public goods that can in theory be efficiently provided by an expert agency such as the Food and Drug Administration (FDA) (Danzon and Keuffel 2007, 3; see also 5-6, 11).

Thus, Danzon and Keuffel say that that knowledge has certain public goods properties. But the public-goods point in no way justifies the restrictions we see; it could only justify government subsidization of knowledge production.

As for uncertainty and the hazard of medical mistreatment, these in themselves provide no market-failure rationale. Danzon and Keuffel provide no mention of systematic erring in the face of such uncertainty. Without an even superficially plausible case of systematic erring that is somehow corrected by the policies, there is nothing here making for justification. Surely the restrictions do prevent some medical mistreatments. But a ban on driving automobiles would prevent some injuries. Market failure can be adduced in the first case no more than it can be adduced in the second. Market failure can be adduced from the presence of risk and uncertainty no more than from the presence of scarcity. Stigler (1961) showed us how to see information as costly, and Demsetz (1969) justly argued that such scarcity in and of itself does not imply failure.

A market-failure rationale requires a plausible story about how government, with its special abilities, might improve matters. It is not legitimate to look at the situation, notice risk, ignorance, and uncertainty, and declare "failure." That ignorance, risk, and uncertainty, as well as folly and presumptuous, plague political and governmental affairs leads us into other epistemic arguments against government intervention, per Smith and Hayek. If "failure" has any meaning at all, it is as an idea in comparative institutions.

Furthermore, in assessing permitting and promotion restrictions, Danzon and Keuffel scarcely acknowledge that most sensitive drugs are prescription-only, and hence uncertainty arguments for pre-market approval and speech restrictions implicitly declare that doctors' access to wisdom discerned by FDA officials is so poor that that deficiency is not redeemed by their obviously far superior knowledge of the local situation. Not only is the FDA assumed to have some special ability in evaluating safety and efficacy (and speech by manufactures), it is assumed that the wisdom discerned by those officials cannot be imparted and entrusted to the medical professionals who actually know something about the patient. Thus, presuppositions of FDA specialness are implicit throughout, but scarcely ever explicit.

Maybe FDA evaluation is a public good, but that would not justify the observed restrictions. The government is special, notably in its exclusive power of institutionalized coercion. That specialness makes for a coherent rationale for taxpayer subsidization of basic research and other public goods. But the policies under discussion are not of such nature.

Danzon and Keuffel also suggest (pp. 28, 82) that a voluntary and competitive field in the assuring of quality and safety and the creation of associated standards would perform less well than a situation in which a governmental agency had privileges and powers over such matters. But they give no grounds for such a view. A very good case can be made for the opposite conclusion, that is, that quality and safety assurances develop best within processes that are voluntary and competitive. Not only does a monopoly government certifier run the risk of producing bad, simplistic, or too few standards, the awesome power inherent in the current system damages integrity throughout the scientific and certification process. In the face of awesome power, people are especially reluctant to be candid about doubts and weaknesses. If, instead, the market were free and the processes were voluntary, the fears associated with candor would be much reduced—the certifier might withhold its seal of approval, but it could not withhold freedom.

Another summary of arguments for banned-till-permitted policy for new drugs is provided by Ronald Hansen:

> The principal benefit claimed is the elimination of unsafe and ineffective drugs thereby reducing the harmful effects no patients either directly from unsafe drugs or indirectly from delaying proper therapy as the result of using an ineffective drug. Control over the claims that companies could make for their products would reduce the need for physicians to verify independently the claims made for products that they prescribed, thus reducing information costs. To the extent that the administration of the regulations discouraged me-too research, research would be directed to more innovative projects. (Hansen 2000, 274)

Hansen's presentation leads us to believe that he himself does not think much of the arguments. Clearly, they in no way suggest that the observed policies correct systematic erring.

If we go down a checklist of market-failure rationales— adverse selection, externalities, natural monopoly, equity, etc.—we do not find one that can be invoked for the policies. Robert Higgs (1994; 1995a, 7-9) examines the official rationales and finds them empty.[9] Likewise, Russell Sobel (2002, 464-65) challenges the presupposition of market failure.

Consider the following from F.M. Scherer, an economist not known to be a free-market stalwart:

> An information market failure may need correction. But why doesn't the regulator merely require appropriate testing and disclosure of test data, letting physicians decide from the data whether the drug is safe and efficacious? If there is an argument for regulation of *whether* new drugs may be marketed, it must lie in a further information market failure—e.g., from the possibility that most physicians are too busy to make well-informed independent decisions. (Scherer 2000, 1315)

Most sensitive drugs would normally be prescription-only. Opposing liberalization in permitting and speech must see comparative failure, not only in voluntary assurance and litigation, but in the profession that writes prescriptions.[10]

If uncertainty sometimes makes us child-like in the face of illness and choice of treatment, who is the parent? The presuppositions surrounding the FDA might stem from an unexamined precept that there must be some kind of parent out there, a yearning for some kind of validator, as well as from the cultural convention of finding the validator in government (Buchanan 2005). If so, it is the responsibility of economists to explain that government is no less child-like than the patient's medical professionals, and that arrangements that effectively give veto power to child-like officials devoid of local knowledge only make a bad situation worse.

Meanwhile, if phantom validation and the like are the actual impetus of the observed policies, then such facets ought be brought out onto the table, presum-

9 I do not agree with the following by Higgs: "By simply denying me the option to consume X, you have definitely made me worse off , because you have removed my most preferred object of choice from the set of alternatives open to me" (1994, 6). But I think the point—and similar "Austrian" claims in his argument—can be suitably altered or omitted, such that the basic contention of no market-failure rationale holds.

10 Incidentally, Philipson and Sun (2008b) discuss the inefficiency of duplication by liability and FDA approval, but, oddly, they make no mention of the yet third layer of control in prescription requirements.

ably as some kind of cultural public good, and defended as such. Until such time, I will regard them as off the table, as is customary in economics, and persist in invoking the political-economy grammar that then applies.

The Intellectual Tyranny of the Status Quo: The Political Sociology of Economic Discourse about the FDA

Peter Temin—again, no free-market stalwart—examined the history of political attitudes surrounding these issues. He writes of new presuppositions spearheaded by officials and then adopted as public doctrine, a shift that lacked critical thought:

> The shift from assuming a capable consumer to assuming an incompetent consumer was made within the FDA within six months of the Federal Food, Drug, and Cosmetics Acts' passage [in 1938]. Not only was the shift in assumptions not controversial, the method by which it was accomplished occasioned no comment as well. The decisions of the FDA were ratified by the courts and enacted into statute by the Congress. Neither branch of the government undertook to question the FDA's assumptions. (Temin 1979, 104)

The public doctrine tacitly attributes specialness not merely to government, but to *American* government. Counterparts to the FDA function in other countries. The FDA could adopt a standing policy that drugs permitted in Europe, Canada, Japan, Australia, etc. automatically become permitted in the US. Why doesn't the FDA adopt such a policy? Is it because a drug that is safe and effective in Australia, Canada, or France may not be safe and effective in the United States? Of course not. The presupposed specialness is not, in fact, special to the American government. Most players of the economic cultural elite—such as the AEA editors and officers, the most prestigious health economists, etc.—do not welcome candid discussion of this point. They observe the standard public doctrine. They observe the taboos of officialdom, academia, and "the policy community." Were the elites to defy the taboos, we would see clearly that the presupposed specialness is, not only not special to American government, but altogether baseless. There is no helpful specialness in this matter—there is no market-failure rationale. The public doctrine is public superstition.

Some know better but play it strategically. Partly, the problem is a Catch-22 embedded in a cultural prisoner's dilemma: If a "player" were to defy the taboos, he might disqualify himself from "playerhood," possibly to the detriment of social welfare. He may engage in noble lying so as not to make the best the enemy of the good.

But the words people publish spell paradox. There are good reasons for us to expect that some, if not most, of the power players—and hence some, if not

most, of the experts—will tend to believe in the goodness of bad policies.[11] If the counterparts in 15 other countries could also permit for use in the US, the FDA would have to compete in drug review and would lose prestige, power, influence, and funding. The whole structure would unravel. Politicians would have to face-up to a colossal mistake, and explain it somehow to voters. Other sacrosanct institutions share the same lack of rationale. Grand superstitions would be challenged and taboos shattered. The great powers—including the two parties who vie for power and pander to voters who systematically under-appreciate liberalism—could scarcely tolerate such implications. They participate in open communication only with those who take care to avoid any such talk. Timur Kuran (1995) explains how the unsaid becomes the unthought and then the unthinkable. The economics profession is supposed to stand up against the collective foolishness of officialdom and society at large. Instead, it has stooped so low that some of its liberal grammar has become unspeakable, even unthinkable.

The presupposition of FDA specialness is political superstition buffeted by power structures. It has given rise to taboos that gull most of the ordinary citizens and cow the regulatees—the pharmaceutical industry—and many of the researchers who interact with the central players. The researchers often depend on the FDA for data access, institutional expertise, visibility, and prestige. Also, the researchers often depend on the regulatees again for data access, institutional expertise, visibility, and funding, if only indirectly. For example, industry provides most of the funding for the Tufts Center for the Study of Drug Development and significant funding for the American Enterprise Institute and the George Stigler Center for the Study of the State and the Economy at the University of Chicago. All told, it is very plausible that we have a polite social network that tends to cut out those who would scrutinize "progressive" presuppositions ensconced long ago and subsequently made ever more permanent and politically sacrosanct. Even an economist in the network who personally doubted the establishment presuppositions would likely suppress such thoughts because of his interdependencies with others who would either take offense or simply doubt whether he can be relied upon as an effective and respectable ally.

I have often pondered whether "big pharma" would make less profit if permitting restrictions were significantly relaxed.[12] There are arguments pointing in both directions. However, one should not assume that all of the people inside big pharma would favor company profits, which might be diminished by liberalization, over social well-being, which would be enhanced by liberalization. They are part of medicine and humanity, and hence consuls of the impartial spectator, and they must see as clearly as anyone the tremendous social downside of restrictions.

11 I have offered a general treatment of why power players will tend to believe in the goodness of the bad policies that they administer or are expert in; see Klein (1998).

12 As for the speech and prescription restrictions, it seems fairly clear that relaxation would enhance their profits.

But even if inclined toward significant liberalization of any of the restrictions, they would still be extremely constrained in voicing such views or bringing their expertise to bear in the policy debate. The diffidence would stem not only from prudence—slaves are never wise to criticize the whipping-master—but from the realization that anything they say is likely to be scandalized as pure greed, particularly by journalists, leftist pundits, opposition politicians, and so-called consumer advocates. Big pharma is a big player in the funding of social research on the FDA, and in the provision of data and institutional expertise, but there is little reason to think that they would fund, openly assist, or themselves voice challenges to the basic presuppositions challenged here.

The important questions are cultural, and the answers must be sociological and psychological. I am not suggesting that economists have been bought off by the drug industry. On the contrary, I am pointing to an explanation of how the problem can persist without conspiracy or venality. At one level, the problem might be sheer status-quo bias, as would exist within any society, even a freer society. But why can't status-quo bias in our present context be more substantially overcome by the power of enlightenment? At that deeper level, I suggest that the problem is a symptom of—and cause of—undue cultural statism—among the electorate, among the power players, among the experts, and among academics. Unenlightenment may feed unenlightenment, particularly when institutionalized coercion enters the contest, as it does here in a big way. Getting it right on the FDA is but part of a much larger set of views.

Our sociology must also delve into the psyche of the researcher. Now we return to the words of the expert economists, especially those who tepidly favor liberalization but preach a kind of agnosticism. Consider this: If there is a market-failure rationale for the observed policies, then we need researchers to explore whether they are warranted and, if so, how they might be fine tuned. But if the observed policies lack a market-failure rationale, then we know—do we not?—that their costs are not fully redeemed. The quantitative research comes to look like complicated demonstrations that water runs downhill. Such awareness might threaten the researcher's selfhood. It is good that scholars demonstrate that the accelerations following 1992 improved welfare, etc., but in a sense they are simply showing evidence on an issue that was settled as soon as we saw that there was no market failure. Thus, many of the leading figures, though favorable to liberalization, might be quite antipathetic to any critical examination of basic presuppositions.

Must Quantification Precede Economic Judgment?

Quantitative empirical work on the effects of the observed restrictions is extremely valuable. It is good to confirm basic tenets with responsible empirics.

Second, the basic message is made more convincing. Third, in quantifying effects we delve into and learn about institutional and practical affairs; we learn to refine the tenets, their application, and our understanding of their applicability. Fourth, empirics improve our sense of the magnitude of the effects. And other reasons exist for admiring and rewarding good quantitative empirical research.

But some economists' rhetoric concerning quantitative research gives me pause. I see two problems. The first, treated in this section, is that some of the rhetoric tends to surrender, slight and undermine the power of non-quantitative argumentation on the issue.

I quoted economists who deny that FDA-expressive economists come to a conclusion. Some of those authors seem to suggest that economics cannot come to a conclusion without quantitative analysis of the effects of the policy. Philipson, Berndt, and their coauthors show little regard for the theoretical arguments for liberalization, and proceed as though the only relevant discourse is quantitative evidence. They claim that "[l]ittle empirical evidence has been put forward to make the case that the FDA is too slow," and that "there seems to be no suggested quantitative methodology or framework for assessing the economic efficiency of the central speed-safety tradeoff." With such remarks they have slighted and dismissed many economic studies rooted in a theoretical argument for liberalization. Philipson and Sun (2008b) suggest we need further empirical analysis to judge "whether additional gains in speed at the expense of drug safety might be worthwhile" (99). Danzon and Keuffel (2007, 86) write: "Effects on patient outcomes and on competition and overall costs have not been measured. Thus several of the components of a full welfare analysis remain to be developed."

Suppose an economist is asked: Which policy is better, import quotas on sugar or freedom to import sugar? In judging the matter, does the economist require quantitative evidence specific to the policy? Does a competent econometrician need to do a quantitative study before we can come to judgment? Certainly not. Quantitative material of such nature is not necessary to arrive at a judgment on the sugar program. Political-economy judgment is complex, but it has been developed—within, by the way, a broad enterprise that *does* depend on empirics of wide array—so as to make part of it is rather simple (or grammar-like), and that part suffices here: *If the situation apparently involves no significant, systemic market imperfection, we side in favor of allowing voluntary action.*

Many of the works cited in the first cells of the Tables 1, 2, and 3, and quoted in Appendix 1, question the presupposition of market failure. For example, in his empirical study of speech restrictions, Leffler (1981) writes, "restrictions on pharmaceutical promotion appear to risk large losses in consumer welfare *for the promise of unproven and perhaps nonexistent gains*" (74; italics added). That is, he establishes the broken eggs and implicitly asks, *Where are the omelets?* Again, Higgs (1994; 1995a, 7-9) and Sobel (2002, 464-65) directly dispute the market-failure presupposition. But many of the establishment authors quietly elide

this fundamental argumentation. Most neither assert the existence of a market-failure rationale, nor do they assent that none exists.[13] But isn't that where the conversation should start?

Consider research on the lessons of off-label practices. Building on suggestions by Beales (1996) and others, Alexander Tabarrok (2000) developed an analysis that asked: What can we learn about the need for efficacy requirements from the pervasive experience of off-label medicine, which has no FDA efficacy certification? The thrust of Tabarrok's argument is that off-label seems to work quite well and so why not drop efficacy requirements entirely? The voluntary assurances for off-label treatment, such as listing in professional medical compendia, are illustrated with evidence of various kinds. To explore the matter further, Tabarrok and I (Klein and Tabarrok 2008) searched for justifications by interviewing those in the trenches. We constructed an online questionnaire that asked doctors about off-label issues. The findings showed that virtually all doctors opposed the idea of imposing efficacy requirements on off-label uses. The survey challenged doctors on the matter of consistency: If one opposes efficacy requirements on off-label uses, shouldn't he also oppose them on initial (on-label) uses? In effect, the doctors were asked: What is the market-failure argument that is decisive for initial uses but not for off-label uses? We collated the responses, breaking them down into a number of arguments. Those arguments were then critically examined. Although we uncovered limitations to the consistency argument, we found nothing resembling a respectable market-failure rationale for initial efficacy requirements.

Such research suggests that off-label practices speak to efficacy requirements. But the learning is not a net-benefit calculation. It is about presuppositions. Such is how much of the critical literature has worked—what do real experiences in other times, other places, other industries, and other realms of medicine tell us? The suggestion, very often, is either that there is no real market imperfection to correct, or that any market imperfection that might exist would call—if it called for any government action at all—for corrections different than the established policies.

The establishment authors sustain faulty presumptions, and correspondingly, they have a faulty idea of where the burden of proof lies and what it entails (Lewin 2007). It is useful to distinguish two vying attitudes:

1. **The liberal attitude** starts with the presumption that free markets work tolerably well, and places the burden of proof on intervention, beginning with a market-failure rationale.

2. **The establishment attitude** starts with the presumption that the

13 As we've seen, there is an exception in Danzon and Keuffel (2007).

> status quo reflects some kind of collective wisdom, and hence places the burden of proof on those who would change it. That burden often takes the form of a demand for a quantitative, empirical demonstration that the change will improve social welfare.

Alas, the latter has largely displaced the former. Had such the establishment attitude prevailed in previous centuries, Adam Smith would have been obliged to remain professionally reticent on mercantilism, Jeremy Bentham on usury, and John Stuart Mill on slavery and women's rights. None of them pretended to resolve the issue by recourse to quantification of the effects of the debated policies. Rhetoric that suggests that such quantification is necessary to arrive at judgment bypasses the question of whether the policies lack any respectable market-failure rationale. Of course, there were individuals who offered rationales for slavery, coverture, and usury restrictions.[14] But the perniciousness of such policies must be understood in terms of general principles, and must be defeated in such terms. When an economist bypasses the fundamental questions, he effectively presupposes that there is a respectable rationale out there somewhere. The presupposition becomes conventional behavior and conventional thought.

The Chimera of Net-Benefit Calculation

Now, the second problem: Some of the remarks surveyed earlier insist not merely on quantitative evidence about effects, but more specifically on a complete, if rough, calculation of net benefits (benefits minus costs). They suggest that policy judgments are mere "advocacy" until we have a net-benefit calculation. Moreover, some pretend to or aspire to net-benefit calculation.

Philipson et al (2005) study the review-time acceleration following 1992. The authors tout their study as the real thing. The gist is that, because the acceleration expedited permission of mutually advantageous exchanges, the acceleration was beneficial. Even under extreme assumptions, any health losses resulting from the reckless permitting of unsafe drugs weren't nearly enough to offset the gains.

To fit things into a net-benefit calculation, they do things like multiplying a life-year by a supposed value of life. Again, I appreciate the merits of being quantitative. At the same time, the drive to fit the matter into an encompassing calculation can lead to the omission of factors that are difficult to quantify. For example, without ever acknowledging it, Philipson et al (2005) left out entirely the effect that faster review times have on drug development! The authors have also left out the all moral, ethical, cultural, and political consequences. These

14 Here we should acknowledge that, paradoxically enough, Smith offered and endorsed rationales for the usury laws that were the status quo in his times.

effects pertain to the sources of moral approval set out by Adam Smith in *The Theory of Moral Sentiments* (1790, 326-27). The first three of those sources have to do with the character of social affairs, in particular how norms of propriety affect how sentiments interact. Some of us would be willing to pay for a more liberal culture in these matters, apart from any effects on health and wealth more narrowly construed. Surely many suffering patients are willing to pay simply for the feeling of not being denied the liberty to decide their own affairs.

But besides getting the net-benefit calculation wrong, there is a more fundamental problem: The rhetoric of net-benefit calculation is phony, and that phoniness can be highly damaging. In reading Philipson et al 2005—whose approach and basic results are recapitulated by Philipson and Sun (2008b) in the *Journal of Economic Perspectives*—we are struck by the many ways in which the authors have simplified the problem.[15] Most of the simplifications tilt the calculation—sometimes sharply and implausibly—*against* finding net benefits from the acceleration. What the authors are really doing is mounting an argument *a fortiori* in favor of the acceleration. An *a fortiori* argument endeavors to make a claim persuasive by making an even stronger claim persuasive—I will show that I can lift 50 pounds by showing that I can lift this bench here which weighs *at least* 50 pounds. That is, Philipson et al are tacitly arguing that the accelerations were beneficial on net in the following manner: *Even when we tip the calculation heavily against the acceleration*, we still find that it was beneficial on net.

In making an empirical argument, trying to quantify all significant effects is usually vain. Instead, we construct the stronger claim by simplifications that make that claim easier to represent and to judge. In *The Rhetoric of Economics* (1985, 115-30), Deirdre McCloskey explains how Robert Fogel argued that the impact of the railroads in the development of the U.S. economy was much smaller than commonly supposed. Fogel estimated not the social savings of railroads but upper bounds on the savings, showed the bounds were small, and concluded that the social savings were small.

I suspect that Philipson and his various economist coauthors, in their heart of hearts, believe that the FDA is much too restrictive—I suspect that, if cornered, they would be inclined to agree that there is no market-failure rationale, and that they would admit the profound implications of that. Between the lines, they argue *a fortiori*. In that respect their work is really like the many other works that cite less technical or merely exemplary evidence of costs and argue that, *since there is no market failure*, the costs of the restrictions are not fully redeemed.

That form of argument would be fine, but it ought to be presented as such. If, instead, an *a fortiori* argument is presented as a net-benefit calculation,

15 Some of the simplifications are summarized at Philipson et al 2005, 31-33; and at Philipson and Sun 2008b, 99.

as though judgment hinges on whether they come up with a positive or negative answer, then the research might mislead.

First, readers might take the calculation at face value and say, "Oh look, FDA restrictions are not that big a deal, our leaders have more important problems to worry about." In fact, the range of net-benefits that Philipson et al come up with for the accelerations following the 1992 reform are unimpressive. Political attention and political will are scarce. One might well react to the finding by saying we have larger fish to fry. The problem would be avoided if Philipson et al made plain that they were not estimating the net benefits of the accelerations but a lower bound, and aiming to show merely that it was smaller "than the true but unmeasurable amount" (McCloskey 1985, 115).

More importantly, when an *a fortiori* argument is presented as a net-benefit calculation, it will quite possibly legitimize wrongheaded notions.

Suppose I am recovering from an injury and my doctors decide that I will need an additional invasive procedure unless I can lift 50 pounds. The only object readily available for lifting is a bench. We sense that the bench weighs at least 50 pounds. I struggle and just manage to lift the bench, so the assurance is provided, *a fortiori*. Now, if someone were to say, "OK, let's say the bench weighs 50 pounds; Dan struggled to lift it, so clearly Dan can only lift about 50 pounds." The danger here is that perhaps the bench weighs much more than 50 pounds, perhaps 100 pounds. Say that, upon my lifting the bench, the doctors record my ability to lift as 50 pounds. Were the cutpoint for the invasive procedure subsequently raised to 70 pounds and my records reviewed in light of that change, I would then be called in to undergo the procedure—erroneously.

It is not plausible that my doctors and I would be so dysfunctional, but the political process is highly dysfunctional. Accordingly, with Philipson et al's calculation, some major parts of the calculation will necessarily be vague and speculative. Opponents can take most of the calculation at face value—after all, its authors have presented it as a complete calculation of costs and benefits. But opponents can then contest certain parts, particularly vague and speculative parts, revise them, and then proclaim that the sign of the calculation reverses. Thus, claiming to do a net-benefit calculation when one is really doing an *a fortiori* argument sets oneself up for refutation.

In general, calls to police government policy with net-benefit calculations—such as Hahn and Tetlock (2008, 73)—can backfire, because very often a net-benefit calculation is beyond the constraints of time, costs, and credibility. Rather, as Ronald Coase (1982) writes in "How Should Economists Choose?," positions, very often, are arrived at by economic reasoning "based on assumptions about human nature so basic that they are difficult to question" (24, 25), and then argumentation deploys "measurements of an effect" (25)—not *all* effects—in an *a fortiori* manner. Empirical measurements of effects are valuable, but the pretense of doing a net-benefit calculation is often chimerical.

Whether they admit it or not, economists typically do not withhold judgment prior to calculation. I noted that the great economists who criticized mercantilism, usury restrictions, slavery, and the subjection of women did not pretended to resolve the issue by recourse to quantification. Even less did they pretend to do a net-benefit calculation. The two problems we have visited—the abandonment of market-failure framing and the pretense of basing judgment on net-benefit calculation—stem from professional norms against critically challenging established policy, norms against acknowledging the viability of condemning many major established interventions on the basis of the grammar of liberal economics.

WE CAN DO BETTER

This article has taken the economic literature on the three FDA-administered interventions as a case study in how statist political culture degrades academic economic discourse. While this article's middle and later parts have been critical of the economic culture, the first part showed that—save Paul Krugman and a few others— the degradations have not gone so far as to embolden economists to judge against liberalization. Rather, much good economic sense survives the degradations. The first part of the paper showed that FDA-expressive economists preponderantly favor liberalization of the three restrictions.

How far should liberalization go? Proposals include international reciprocity, creating a competitive field of certifiers certified by the FDA (Miller 2000), dropping efficacy requirements, "split-label" reforms and such for speech, dropping prescription requirements, —all the way to abolition. The idea is to move some distance from banned-till-permitted to allowed-till-forbidden. For example, the FDA allows dietary supplements, but, after a concern about a product in the store arises, decides whether to forbid it. Much of food control works similarly, as does the Consumer Products Safety Commission. In *Searching for Safety*, Aaron Wildavsky (1988) called it "resilience," and favored it over restrictive "anticipation." Pure resilience would be just the court system. My view tends towards pure resilience (and a better court system), because I see large costs to restrictive "anticipation" and believe that the heavy lifting in assurance is done, and will be done, by voluntary practices and institutions. The voluntary processes are self-correcting, while restrictive systems do not exhibit that virtue with anything like the same agility, diversity, and sensitiveness—and humaneness, decency, and equity. Hence the great costs that many studies have substantiated.

The matter is urgent, the stakes very high. A leading figure, Sam Peltzman, speaking in 2005 of the proof-of-efficacy requirement imposed in 1962, said:

> I concluded that the proof-of-efficacy requirement was a public health disaster, promoting much more sickness and death than it prevented. Nothing I have seen since has moved me to change that conclusion—the disaster is ongoing.

He goes on to say that because of biases, "The carnage from this regulation, I regret to assure you, will continue for a long time" (Peltzman, 2005, 15-16).

Such pessimism is hard to escape. A 2003 survey showed that most economists are *supportive* of FDA controls.[16] If we are to escape Peltzman's pessimistic forecast, it must be by way of better, bolder, braver leadership at the crucial nodes in the ecology of economic culture. Economists ought to know that vital economists reach a conclusion on the FDA, and, further, that there is not and never was a market-failure rationale. Economists must face up to economics, even if sacred cows must be slaughtered. Our central calling is to correct attitudes that are gravely mistaken. We can do much better. What's needed is fuller embrace of the by-and-large Smithian verities that sustain liberal presumptions and a corresponding readiness to oppose established policies and the establishment-mindedness that surrounds them.

Appendices

Appendix 1: Compendia of judgments to substantiate the listings of economists in Tables 1, 2, and 3. Link.

Appendix 2: Substantiation that the individuals listed in Tables 1, 2, and 3 or treated in the "Apparent Anti-Liberalization" section meet at least one of the criteria required to be counted as an "economist." Link.

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NOTE: A large portion of these items are cited only in Appendix 1.

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ABOUT THE AUTHOR

Daniel Klein is professor of economics at George Mason University, associate fellow of the Ratio Institute, Stockholm, and chief editor of *Econ Journal Watch*. He has long challenged "consumer protection" rationales for government intervention, notably in the collection he edited *Reputation: Studies in the Voluntary Elicitation of Good Conduct* (University of Michigan Press, 1997) and his concise essay entitled, "The Demand for and Supply of Assurance" (link). He has published work critical of the medical interventions administered by the Food and Drug Administration in *Knowledge, Technology and Policy* and, with his colleague Alex Tabarrok, in *American Journal of Economics and Sociology* and *Regulation*. With Tabarrok he is coauthor of the extensive website of research and scholarly argumentation on those interventions, FDAReview.org.

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Econ Journal Watch,
Volume 5, Number 3,
September 2008, pp 349-351.

ECONOMICS IN PRACTICE: FOLLOW UP

The Curtailment of Critical Commentary in Australian Economics

BRIAN DOLLERY,[1] JOEL BYRNES[2] AND GALIA AKIMOVA[3]

FOLLOW UP TO: COELHO, PHILIP R.P., FREDERICK DE WORKEN-ELEY, AND JAMES E. MCCLURE. 2005. DECLINE IN COMMENTARY, 1963–2004. *ECON JOURNAL WATCH* 2(2): 355–363. LINK.

ABSTRACT

COMPARATIVELY FEW SCHOLARS HAVE EXPLORED TRENDS IN THE NATURE OF papers published in economics journals. These include Laband and Piette (1994), Laband, Tollison and Karakan (2002), Ellison (2002), Coelho and McClure (2005), Coelho, De Worken-Eley and McClure (2005), Coelho and McClure (2006), and Whaples (2006).

Perhaps the most important contribution in this genre is Coelho, De Worken-Eley and McClure (2005), who found that "the space devoted to critical commentary has declined sharply at top economic journals" (355). They investigated the *American Economic Review*, *Economic Journal*, *Journal of Political Economy*, *Quarterly Journal of Economics* and the *Review of Economics and Statistics* from 1963 through 2004.

In Dollery, Byrnes and Akimova (2007), we employed their method for all Australian economics journals from 1962 to 2005. Figure 1 shows the results we obtained.

We concluded that "from the mid-1970s through to around the early to mid-1980s our sample of Australian economic journals was relatively more amenable to publishing 'critical comment' pertaining to the articles previously published in those journals (measured as either a proportion of total articles or as a percentage of total pages)." However, "it is striking how the occurrence of critical comment

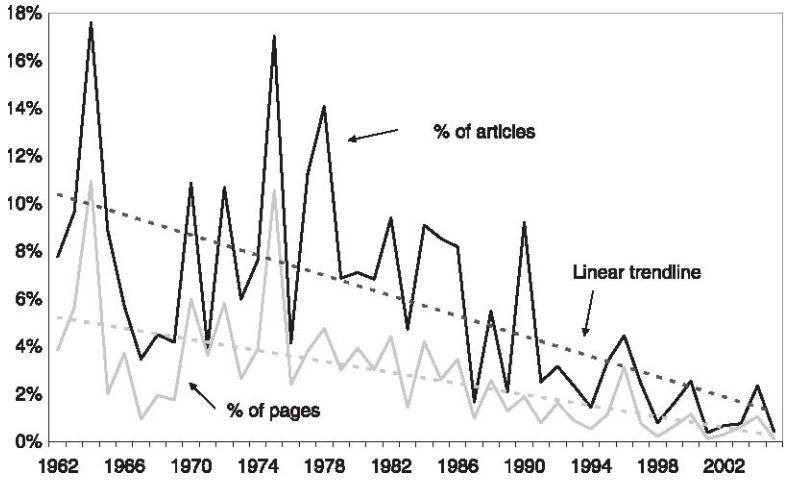
1 Professor of Economics and Director, Centre for Local Government, University of New England, Armidale, New South Wales, Australia, NSW 2351

2 Research Consultant, KPMG, Melbourne, Australia, VIC 3000

3 Research Assistant, School of Business, Economics, and Public Policy, University of New England, Armidale, New South Wales, Australia, NSW 2351

throughout the 1990s has diminished, almost to the point that it now seems to be a 'lost art'." We observed that "this finding concurs closely with the results obtained by Coelho, De Worken-Eley and McClure (2005) for their more limited sample of five top-ranked economics journals" (Dollery, Byrnes and Akimova 2007, 301-302).

Figure 1. Percentage of Critical Commentary Articles as Proportion of Total Number of Articles in Australian Economics Journals, 1962 to 2005



Source: Dollery, Byrnes and Akimova (2007, 302, Figure 1).

Our finding for Australia suggests that the trend shown by Coelho, De Worken-Eley and McClure (2005) may also hold for economics journals generally. Is the trend the same in other disciplines?

Finally, possible reasons for the curtailment of critical commentary are explored and discussed, especially by Coelho and McClure (2006) and Whaples (2006).

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About the Authors

Brian Dollery is Professor of Economics and Director of the Centre for Local Government at the University of New England in Armidale, New South Wales, Australia. His main research focus falls in local public finance and the economics of Australian local government. Brian has written and co-authored numerous books and scholarly papers, including *Australian Local Government Economics* (2006) and *Reform and Leadership in the Public Sector* (2007). His email is bdollery@une.edu.au.

Dr Joel Byrnes is a research consultant at KPMG in Melbourne and former Deputy Director of the Centre for Local Government at the University of New England in Armidale, New South Wales, Australia. His main research interests reside in the economics of urban water and Australian water policy as well as the economics of Australian local government. His email is joelbyrnes@kpmg.com.au.

Galia Akimova is a research assistant at the School of Business, Economics and Public Policy and a member of the Centre for Local Government at the University of New England, Armidale, New South Wales, Australia. Her email is gakimova@une.edu.au.

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Econ Journal Watch,
Volume 5, Number 3,
September 2008, pp 352-357.

CHARACTER ISSUES: CLASSIC REPRINTS

The Present State of Economic Science

GLENN E. HOOVER[1]

REPRINTED WITH PERMISSION FROM *SOCIAL FORCES*, VOLUME 5, ISSUE 1, SEPTEMBER 1926: 57-60[2]

ABSTRACT

THERE IS PROBABLY NO SCIENCE WHICH HAS MADE SO LITTLE PROGRESS IN ITS APplication as the science of economics. The most elementary principles are as hotly disputed today as ever except where despair or boredom has supplanted prejudice and passion. The British electorate fights over "protection," as exemplified by the Safeguarding of Industries Act, with the same weapons their grand-fathers used in the days of Cobden and Bright. In the United States, the Republican party, favoring protection for reasons that were better expressed by Alexander Hamilton, governs alternately with the Democratic party which favors free trade—perhaps theoretically, but certainly more confusedly and timorously than did Thomas Jefferson.

The debates and arguments continue, from the country store to the street corner and on to the halls of Congress, everywhere speeches, but only the speakers are new. The speeches themselves, at best, are the same as of old, at their worst they but show the progress we have made in fatuity. If Adam Smith, Ricardo and the younger Mill returned to earth they would see no evidence that they had ever written on the subject of international trade.

The monetary theories of Ford and Edison are accepted with the same credulity and enthusiasm as was the "free silver" heresy of the eloquent Mr. Bryan or the "make money ridiculous" theory of the efficient Mr. Lenin. Charlie Chaplin,

1 For information about Glenn E. Hoover, see About the Author at the end of this document.

2 From *Social Forces*, Volume 5, no. 1. Copyright © 1926 by the University of North Carolina Press. Used by Permission of the publisher. www.uncpress.unc.edu. Here a few minor apparent typos have been corrected.

the Sage of Hollywood, adds to his popularity with the masses by joining in the hue and cry against Wall Street, the Gold Barons and the Federal Reserve Board. The solution of the versatile Charlie is the elimination of the gold standard. He would have the government issue paper money "representing" production, just as Mr. Edison would have money issued "representing" the falls of the Tennessee River at Muscle Shoals. Mr. Ford's *Dearborn Independent*, not to be outdone in vision, advocates the abolition of all interest, "a tax that few ancient tyrants would have dared impose." This is surely the most appealing reform since Jack Cade advocated hanging all the lawyers. Compared with it, we economists must reluctantly admit we have nothing to offer.

The belief that our protective tariff policy makes possible the comparatively high wage scale in the United States was never more widely held than at present. Labor leaders, industrialists, even the farmers, vie with each other in their advocacy of high tariffs. The bankers, being more intelligent in economic matters, do not give their unanimous support. For the so-called "international bankers" of New York, the crude protectionist arguments are as transparent as a window pane. They are beginning to murmur at our governmental stupidity.

The Economic Bulletin of the Chase National Bank, August, 1925, assures us as follows:

> There is no mystery about the high wage scales in America. These high wage scales are not begotten by the tariff, nor are they dependent upon the tariff. They grow out of the high efficiency of labor per individual.

Housewives and others somewhat distressed by the high costs of living should note well the following passage from the same bulletin:

> Finally, everybody is hurt by the tariff as a consumer. Everybody in the United States pays more for many commodities than it would be necessary to pay if there were not tariffs on these commodities. This extra payment by the consumers constitutes the price which the country pays for maintaining in present volume certain industries for which the country is not so well adapted comparatively as it is for other industries. It constitutes the subsidy which the country supplies to certain industries to enable them to bid away labor and capital from other industries which could use the labor and capital better if there were no tariffs.

For such un-American heresies as the foregoing, the Chase National Bank is leaped upon by the weekly *American Protectionist* in its issue of September 4, 1925. The *American Economist* [published by the American Protective Tariff League] is an

organ of enlightenment "devoted to the protection of American labor and industries," as we are frankly advised on its title page. It accuses the bank's economist "of indulging in so many sophistries that, in the space we have, we cannot point out all of his errors." No communist could be more suspicious of the bank's motives in publishing the bulletin. It asks triumphantly:

> Where does the money come from to pay such extraneous expenses? What is back of it all? What is the purpose of the publication?

When the protected employers make this attack on the economic theories of Chase National Bank, they can be assured of the support of the leaders of organized labor. The very title of the article in the bulletin, "A World Afraid of Production" is enough to infuriate any group which subscribes to the "make work" theory, limitation of output, five-day week, hostility to machinery, etc. Oddly enough, the workers and those whom they choose to call their "masters" cannot always be united on a plan to increase production and lower prices to the consumer; but any scheme which they think will raise or maintain high prices for their particular product, finds them sticking together like burglars. The "wage-slaves" and their "masters" unite in supporting the tariff as the indispensable prop of the "American standard of living." And yet, Professor Taussig of Harvard, the greatest tariff expert, perhaps, in this or any other country, says of their arguments:

> None put forward in favor of protection are more specious and widely held, none are more fallacious (*Principles of Economics*, 3d. ed., V. I, p. 513).

One explanation of the relative backward state of economics, is that it is applied, in all matters of general concern, not by experts, but by the erratic man in the street, the uninitiated, leaders of trade unions, employers' associations, and, God save the mark, members of Congress. The failure is collosal; it inspires; but it probably is no greater than would be the failure of chemistry if it were "applied" by the same individuals who apply the science of economics.

The non-social sciences on the contrary, are applied by experts and therefore in their application, they tend to incorporate the best thought and the latest experience that can be found anywhere in the world. It is not necessary that the average voter be converted to some improvement in iron-making before it is put in operation almost simultaneously in Sweden and Alabama, Pittsburg and Essen.

Moreover the advance of the non-social sciences is steady. They do not turn back on their tracks and repeat their stages. We are not confronted alternately with the railroads and ox-cars as systems of transportation, whereas it is just that sort of thing that happens in the application of economics to our social problems.

There the movement is not so much circular or spiral as it is a see-saw, teeter-board affair. We first exert every effort to induce a period of rising prices, which we associate with increased production, business optimism and "boom" times. (All this, of course, is but another way of saying we are decreasing the value of money.) We then become appalled by the perfection of our handiwork, and, with equal effort, retrace our steps, with the object of increasing the value of money, lowering prices, launching attacks (mostly verbal), against that perennial monster the H.C. of L. [a popular abbreviation at the time for the high cost of living], until we are back where we started ready to go again. It has been said that the only thing we learn from history is that we learn nothing from it and this is particularly true of the average man in the field of economics.

The loss to society by reason of our failure to utilize the best scientific thought in the solution of our economic problems is incalcuable. It is probable that a scientific application of sound economic theory offers by far the greatest prospect of increasing the social income. The most evident proof of waste and inefficiency at present are found in the following phenomena:

1. Failure to utilize to the fullest the advantages of division of labor and large scale production, by our prohibition of imports and exports and protective customs duties.
2. Involuntary unemployment, chronic in some of the industrialized countries and intermittent in all the rest of them.
3. A monetary system that is inadequate to provide us with a stable standard of value for payments over a long period of time. This lack of a monetary unit, comparatively stable in value, is in large part the cause of our industrial crises and the resultant unemployment.
4. The toleration if not the encouragement of monopolies among both capitalists and laborers, which restrict the free movement of labor and capital to the industries where they are most needed from the point of view of society as a whole.

The enormity of the losses indicated above leave the average man strangely unmoved. Even when assured by economists that some or all of these evils are not inevitable features of the social order, but could be eliminated with intelligence and good will, he remains cold. Such things are important perhaps, but they are everybody's business and must be neglected accordingly. Besides it is difficult to visualize the losses due let us say to a defective monetary standard, and anything that cannot be visualized is difficult to comprehend for the second generation of cinema folk. If the eruptions of Vesuvius drive fewer Italian famers from their plots of ground on the mountain side, it attracts the attention of the world and yet, all the losses caused by Vesuvius since the beginning of time is probably insignificant compared to the loss suffered by Great Britain each week that her million unemployed stand idle.

In accounting for the backwardness of any social science such as economics, we must realize too that they have their peculiar and inherent difficulties. Social phenomena are so complex that it is quite impossible to isolate causes. One cannot put a modern society into a test tube or a closed room and, excluding every other factor, give it a dose of "free silver," "deflation" or "protection" and observe the results. Social experiments can never be repeated under exactly the same conditions, hence it is not surprising that fools learn nothing and wise men little form the accumulated experiences of mankind. If the application of economic science were turned over by us to experts with the same gracious abandon with which we have abdicated in favor of the chemist, the surgeon or the engineer, our progress would still be limited by these inherent difficulties.

We do not wish however to hold the economists quite blameless for the backwardness of their science. On the contrary we think their responsibility is large. The fundamental difficulty is that most university economists are little interested in the advancement of their science in the sense of working for a more general acceptance of its truths. They have a professional interest in advancing themselves in the science, they prefer research to teaching, they purify, qualify and rarefy definitions and doctrine until they are accused with some justice, of indulging in the sterilities of medieval scholasticism.

Their professional journals are spotted if not filled with articles whose authors may well pride themselves both on the high quality and uselessness of their research. They are usually dull and without exception, very meagerly read. No average man, however keenly interested in a vital economic problem, would think of looking there for light. The economists are too far ahead to give him the help he needs. [The public] are quite out of touch with the people whose help is indispensable if public economic problems are to receive a rational and scientific solution.

There is a school of economics, now in high voyage, which stresses what it calls practical economics, as opposed to the scholasticism of the older theorists. But, as so often happens, a commendable revolt has gone off in the wrong direction. This school is practical, not in the sense that it deals with actual economic problems of public importance, but that it gives such a view of the actual operation of economic society, that students look upon it as a valuable preparation for their business career. Naturally enough, this school is most highly developed in the United States, where the courses in economics are often given in the departments or schools of business, and consist in large part of such subjects as industrial management, accounting, foreign trade, traffic management, advertising, and other subjects calculated to train young men to participate advantageously (to themselves) in the business world. This, of course, is economics in the spirit of the trade school or business college. Such instruction has no more to do with the consideration or solution of social problems than has instruction in a barber college.

We should like to conclude optimistically, but as we do not now see either the

men nor the movement which might popularize economic truths in our time, we shall content ourselves with pointing out the direction whence they may come. We must wait for some man of ability and sound judgment, capable of crusading for economic truth with the same fervor that Marx, Bryan, Samuel Gompers, Lenin and the American Protective League have displayed in their advancement of economic darkness. It is doubtful if he will come from the *professorial* class; they lack the apostolic fervor. We need a Turgot, a Cobden or a Henry George. Why is it that Truth may not secure the same zealous devotion that Error so frequently inspires?

About the Author

Glenn E. Hoover (1887-1961) was a professor of economics at Mills College in California and author of *Twentieth Century Economic Thought* (1950). A JSTOR search shows that he published ten articles in *American Journal of Economics and Sociology*, three in *Social Forces*, one in *Political Science Quarterly*, and one in *American Economic Review*. Readers interested in the present article are encouraged to consult his 1943 article "The Failure of the Social Sciences" in the *American Journal of Economics and Sociology*. (Photo Credit: *Mills Quarterly*/Alumnae Association of Mills College)

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Econ Journal Watch,
Volume 5, Number 3,
September 2008, pp 358-372.

Toward a Public and Professional Identity for Our Economics

Daniel B. Klein[1]

Abstract

The demand and supply of identity works rather differently than the demand and supply of dinner. You can enjoy Greek food tonight and Thai tomorrow. You can enjoy a meal in isolation. Identity is different. Because choice is socially embedded, identity suffuses most any meaningful preference scheme or utility function. If you are shopping for identity, what is it in you that constitutes the basis for the choice?

Many of our failure theories apply to the demand and supply of identity. Heck, not only do societies and institutions suffer identity lock-in, individuals do. There's no reason to reject the suggestion that there are opportunities for better identity options. In one's formative years one might come to a menu option, a meal that lasts a lifetime, that doesn't really agree with him.

And there's always scope for reconfiguring the menu. An identity works in relation to a scheme of options. For many, "Republican" means not Democrat.

But as Yogi Berra said, "If you don't know where you're going, you might end up somewhere else." Where you're going sometimes turns out to be a destination in a configuration other than you figured.

Here I endeavor some identity entrepreneurship. I sense a latent demand for a new identity option for economists. I would associate the new identity with, among others, Adam Smith, Edwin Cannan, Friedrich Hayek, Milton Friedman, Ronald Coase, and James Buchanan—the "SCHFCB" identity. Well, that is but one name that won't do.

1 Professor of Economics, George Mason University, Fairfax, Virginia 22030
This article and the associated questionnaire grew particularly from discussions I had with Russell Roberts, as well as the Econlib article that he helped me with (link). Parts of this article replay passages of that article. For valuable comments I thank Niclas Berggren, Jason Briggeman, Kirk Dameron, Dan Johansson, Nils Karlson, and Pedro Romero.

I tentatively call it "Smithian." Perhaps ten percent of economists in the United States share a broadly Smithian character. Should we identify and declare it? Should we try to cultivate an identity that functions in both the professional and the public cultures?

Oh, let's give it a try. Even if the probability of success is small, the potential upside is great indeed—imagine, a classical-liberal economist identity that functions in the public culture!—and the costs of our trying are low.

Here, the entrepreneurship is cultural, and hence collaborative in an open-ended way. We need to make sure that we are, or could be, a community—of a networked sort. We need to discover what the latent community thinks, or would think if it were a community. How would you characterize "we"? Who all is "we"? What kind of economics does this "we" represent?

If we build it, maybe others will come. But what exactly is "it"? And, what would it mean for others to "come"? What activities would actualize the identity and associated community?

A process of co-determination and mutual adjustment might be advanced by open discussion. This article consists of an essay followed by a questionnaire to form the basis of further discussion. I will invite a set of individuals to complete the questionnaire. All completed questionnaires will be non-anonymous and made available online.

The questionnaire is not a survey. Rather, it is more like a set of interviews, to elicit interest and discover what each identified individual thinks. The following individuals will be invited to complete the questionnaire:

- Individuals who are members of the Institute of Economic Affairs' academic advisory council or honorary fellows
- Individuals who have served as president of the Mont Pelerin Society
- Individuals who have served as president or vice-president of the Association for Private Enterprise Education (APEE)
- Individuals who have served as president or vice-president of the Society for the Development of Austrian Economics (SDAE)
- Individuals who have served as president or vice-president of the Public Choice Society
- Individuals who served as president of the European Public Choice Society
- Individuals tenured in the George Mason University Department of Economics
- Individuals who are research or associate fellows or members of the scientific advisory board of the Ratio Institute (Stockholm)
- Individuals listed on the *Econ Journal Watch* masthead (including editors, directors, and Advisory Council members)
- Individuals who have received the Nobel prize in economics
- Individuals who have received the John Bates Clark Medal from the American Economics Association

- Individuals who have served as president of the American Economics Association, Royal Economics Society, European Economics Association, Canadian Economics Association, Southern Economics Association, Western Economic Association, or Eastern Economics Association

Character and Its Heterogeneity within Economics

Adam Smith taught that people need to define and judge themselves as beings that transcend the bestial. An economist has purposes qua economist, purposes related to serving goals, causes, values, and ideas. According to Aristotle, "Character is that which reveals moral purpose, exposing the class of things a man chooses or avoids."[2]

Although economists share some basic ideas and formulations, they often differ somewhat in terms of the important things that Aristotle associated with character. Economists differ in basic purposes, in some of their basic formulations, in modes of discourse, in presumptions about what is good for society, in kinds of audiences or discourse to seek or avoid, and in associated policy judgments. Character types can themselves be subjected to evaluation. In *The Theory of Moral Sentiments*, Adam Smith wrote: "The characters of men … may be fitted either to promote or to disturb the happiness both of the individual and of the society."

If asked, most economists would probably say they serve society by serving science. To serve science, they follow the customs, standards, and practices of academic economics. Doing "normal science" is keeping your nose to the professional grindstone, in the faith that doing well academically is doing good. Perhaps the dominant characteristic of the normal economist is a tendency to look to the profession—its top journals, its most cited individuals—to determine what kinds of activities and purposes are meritorious. The "normal" character in economics feels considerable deference, if not loyalty, to the top of the economics pyramid.

The practical morals of "normal" practices become adopted and internalized without much critical scrutiny. Thinking of their purpose as merely "scientific," uncritical economists are in fact assuming and adopting the particular substantive judgments implicit in normal practices. Perhaps most economists see themselves as character-free. But once normal economics is subjected to critical scrutiny, we find norms.

For example, we often see deference to top economics, and among top economists we often see a focus on the policy and institutional status-quo. Specifically, top economists tend to carry a presumption of the status quo, such that challenges to the status quo bear the burden of proof. If you think far outside the status quo, or make explicit ideas like the distinction between voluntary and

2 Here I omit citations and references. Write to me (dklein@gmu.edu) for any you may desire.

coercive action, normal science might dismiss you as an "ideologue," "advocate," or purveyor of "normative" economics.

Another example of tacit norms in normal economics is a certain double-standard. Sometimes it seems that free enterprise is deemed to fail if it works less than perfectly, and government activism is deemed to succeed if it works at all.

Yet another example is that by flattening human beings down to machines and flattening knowledge down to information, some of the moral, behavioral, and epistemic facets of the problem, facets that might be important, are eclipsed or misrepresented.

Still other examples could be offered, but the point is that there is no "character-free" economist. The notion of being without character is nonsensical. The discipline is populated by economists of different character types.

The Smithian Economist: A Few Characteristics

One of the broad and venerable characters is that of Adam Smith, Edwin Cannan, Friedrich Hayek, Milton Friedman, Ronald Coase and James Buchanan, among others. Surely, there are some tensions within this set, but more important commonalities predominate. Each of us would paint the portrait a bit differently, of course, and there's no urge to converge on a single rendering.

And I have no great urge to propose a particular name. In previous discourse I suggested the cumbersome "spontaneous-order economics," and then "Smith-Hayek economics." It might be useful to proceed with a meaningless term that is an obvious placeholder—such as "Placeholder." But, instead, here I proceed by calling the composite character "Smithian." Using a meaningful candidate term advances the exploration of the advantages and disadvantages to consider. Also, Smith looms large in all this, so getting some focus on his significance seems worthwhile. But the suggestion of "Smithian" remains tentative, half placeholder.

Here I offer six characteristics to indicate the portrait in my mind:

1. a tendency to employ, and even make explicit, the distinction between voluntary and coercive action in formulating many basic economic issues, categories, principles, and arguments;
2. an appreciation that knowledge is not merely information, but also interpretation and judgment, and as such is highly particular to the individual and the moment; it is essential for humans to err, in the sense that they kick themselves for having interpreted or judged badly; asymmetry marks not merely information but interpretation; the "common knowledge" assumption typically does not hold;
3. a sense that economics must be relevant and serve social purposes, and that such service necessarily entails engagement with non-economists, notably laypeople and policy-makers; a sense that our academic institu-

tions might fail such that doing well academically does not always align to doing good;

4. a sensibility that economic reality is incredible—in the sense that attempts to depict or explain economic reality are generally not credible; this sensibility eschews efforts to paint a picture of the economy, to know the economy, beyond some by-and-large generalities;
5. a sober, non-romantic view of government—since economic reality is scarcely knowable, we should be wary of those who pretend to manipulate it beneficially; moreover, coercive arrangements generally self-correct much less effectively and reliably than do voluntary processes;
6. a presumption in favor of liberty, not the status quo; the burden of proof should be on those who favor restriction or intervention, even when it is the status quo.

These Smithian characteristics are not particularly salient in most economists today. Some of the Smithian characteristics even go against the grain of normal economics. We might advance the Smithian characteristics by building an effective "we."

IDENTITY: CHARACTER IDENTIFIED, AFFIRMED, PROCLAIMED

Those of like mind or kindred spirit share our purposes. With them we have fraternal feeling. They are kin in character.

In pre-Civil War America, proponents of the immediate emancipation of all slaves identified themselves as "abolitionist." Not all abolitionists agreed on every aspect of the struggle. But the overarching identity of "abolitionist" facilitated discovery, mobilization, and cooperation. And indeed it cut both ways, as their opponents, too, called them abolitionists, and sometimes used violence against them. "Abolitionist" was an identity, as in what one shows when asked for "ID." Someone's name is a label, but it is also an identity functioning in society.

Not all of those who identified with abolitionism presented themselves as "all on fire" like William Lloyd Garrison. We do not always wear on our sleeve all important elements of our identity—such as our religious convictions. In economics, one might identify to one extent or another with Marxism, Keynesianism, Austrianism, or feminism, but remain low-key about it. The present endeavor is not about bringing people "out of the closet." Rather, the idea is to coordinate on a new classical-liberal identity, even if, for some, it functions primarily in closets.

Even if the group only meets in the closet, even if others do not know *who* exactly belongs to the network and spirit, the wider culture nonetheless identifies the name of the group and associates it with the group's characteristics. But this will be the case only if there are *some* who proclaim the identity, *some* William Lloyd Garrisons. Thus, the rest of the abolitionists may identify with Garrison and abo-

litionism, even if they remain low-key about it.

The *American Heritage Dictionary*'s first two definitions of *identity* are:

□ "The collective aspect of the set of characteristics by which a thing is definitively recognizable or known"

□ "The set of behavioral or personal characteristics by which an individual is recognizable as a member of a group."

Identity reduces the transaction costs of finding and cooperating with kin, yielding fruits in identification, recruitment, mobilization, and organization. It forms a basis for teamwork and shared sentiment. It develops purposes, clarifies messages, and emboldens the spirit. It answers people's quest for meaning.

At the same time, identity can turn into groupthink, and, if it seizes coercive power, can perpetrate oppression. That's why Adam Smith thought that laissez-faire ought to prevail in the church sector. We need free cultural institutions. A mix of identities creates criticism, debate, and competition.

Lost in the Shuffle

Economists who favor liberalization are routinely caricatured as exponents of flattening human beings down to machines—"economic man"—and flattening social affairs down to blackboard diagrams and mathematical models. Their policy views are said to stem from a faith in perfect competition. These slurs and monkey-shines are regurgitated by crass economists and are regularly aided and abetted by the left-leaning press. –As though *The Theory of Moral Sentiments* and *The Wealth of Nations* had never been written. As though Hayek, Friedman, Coase, Buchanan, Armen Alchian, Vernon Smith, Thomas Sowell, Deirdre McCloskey, etc. have never existed. As though we don't exist.

The Smithian kin of economics have a problem. Even if that character can be fairly well drawn, it does not have a suitable identity. There is a Smithian character, shared by thousands. But there is not today a functioning Smithian identity. If we had a functioning identity, we would be less fringy within economics at large, and we would cultivate our own cultural niche and occupy the center of that subdomain.

Sometimes such an economist will call himself a "free-market economist." Some might say "Austrian." Some will simply say "economist." None of these work well as an identity for the character favored here.

"Free-market economist" is misleading. First, it is easily misunderstood as the insistence that all markets should being absolutely free—something Smith explicitly rejected, as do most Smithians. Second, it would seem to signify any economist who favors free markets, regardless of other aspects of his character. Although every Smithian economist tends to favor freer markets, not every free-market economist shares the Smithian character. Enthusiastic young libertarians

often cherish simple formulae that need to be overcome, or judiciously weakened, to mature into the "squishy" Smithian character. And, further down the path of life, a mature economist who never did relevant or meaningful research, and instead only practiced and affirmed arid applications of certain scholastic modes of discourse, and deprecated criticism of normal science, would not be a Smithian no matter how strongly he favored free markets. These reasons speak also against "libertarian economist" and "classical liberal economist." Yet another problem with such names is that, while the Smithian character allows for outspokenness, it is just too pushy to announce political opinions in the name.

Remarks on the Austrian Identity

The Austrian identity often appeals to young economists with the vision and courage to challenge conventional thinking and the status quo and to defy taboos against taking liberty seriously—hearts and minds that we wish to attract to a Smithian identity. Because Austrianism is a significant and incumbent competitor to Smithianism, some rivalrous marketing may be in order.

The coherence of the Austrian identity, and that name, really stands or falls with infatuation with Ludwig von Mises and his distinctive "praxeological" approach to economics, which conceives of economics in terms of a priori axioms, logical deduction, and categorical conclusions that are apodictically true.

Hayek did not share that approach. He never fashioned himself as a protégé of Mises, and he never promulgated an "Austrian" identity. He wrote generally against rationalistic individualism and in particular he remarked about Mises' undue rationalism. In a way, Mises is a type of the "man of system" described in *The Theory of Moral Sentiments*, as Mises' deductions seem, for example, to neglect that the human "chess pieces" might prefer, even "really really" prefer, systems of coercion—if only as statist means of creating identity focal points—and choose to mobilize so as to impose systems of coercion on themselves and others.

There is much that Hayek shared with Mises—specifically in money and trade-cycle theory and the calculation debate, and generally in classical-liberal vision and motivation—but those elements are not things that fundamentally distinguish the thought of Mises from the thought of a great many other liberal economists who did not hail from Austria. Hayek borrowed a lot from Mises, but he also borrowed from, for example, Hume, Smith, Thornton, Mill, Spencer, Wicksell, Polanyi, and especially LSE colleagues.

When we set aside Mises' methodological distinctiveness and forgive his crankiness, Mises comes to be seen—like Menger or Böhm-Bawerk—as but one great liberal economist in a line that extends back long before 1871 and that ranges far outside of Austria.

Although I oppose the identity built around Misesian economics, I certainly

would have the Smithian tent be broad enough to include Mises and Austrians of any sort. Indeed, I regard Mises as an epic, heroic figure, the person who, more than any other single person, bridged classical liberalism and modern libertarianism. Murray Rothbard, too, I regard as a great and epic figure, and Israel Kirzner, in my view, deserves a Nobel prize. But I still say that it is time to rethink all that they have to offer within a more Smithian mode.

The essential Austrian impetus, represented by Mises, Rothbard, and Kirzner—despite any disavowals—is to claim a scientific foundation for laissez-faire economics. That impetus is misguided. By contrast, the Smithian attitude, in this respect exemplified by Hayek, eschews foundationalist ambitions. It appreciates some by-and-large political-economy verities that allow for and help to justify a presumption of liberty, but, mainly, assumes a posture that is critical of the scientific pretension of interventionist economics.

The wing of Austrianism more associated with Kirzner (as opposed to Rothbard) has attempted to homogenize Mises and Hayek, and externally trades chiefly on Hayek, but the homogenization is illegitimate. The economics of Hayek is, at bottom, closer to that of Smith, Cannan, and Coase than that of Mises. In as much as the character of Austrianism is worthy (that is, Hayekian and hence Smithian), there is no sense in calling it "Austrian" (might as well call it "Scottish"), and in as much as it is distinctive and reasonably identified as "Austrian" (that is, Misesian), the character is not worthy, particularly in light of the availability of the Smithian character.

My Austrian colleague Peter Boettke has suggested that what makes Austrian economics distinctive is its appreciation of knowledge's richness. That virtue is profound in Hayek and plays a vital role in Kirzner and latter-day Austrians, but it is not particularly strong in Menger, Bohm-Bawerk, Mises, or Rothbard. It is probably at least as strong in Smith as it is in Hayek's Austrian predecessors. Further, the Mises-Rothbard axiom about people always acting to better their own interests, at least ex ante, unduly attributes a rationalism to human conduct, and goes against deeper wisdom about knowledge and human nature. Another problem with Boettke's attempt to justify the Austrian identity is that appreciation of knowledge's richness is by no means original with or even all that special to Hayek. For example, there's Michael Polanyi, arguably Hayek's equal on knowledge's richness. And there's Thomas Schelling, who explained that focalness is a matter of context and interpretation, not necessarily inherent in the logic or information of the situation. When Schelling says that there is no way to prove that a joke is bound to be funny, he nails the essential and pervasive nature of asymmetric interpretation as well as anyone. No one, so far as I know, has made a suitable study, but over the course of centuries many philosophers and social scientists—including Smith, Bentham, Kierkegaard, Schopenhauer, Spencer, the pragmatists—have shown appreciation for knowledge's richness in ways that Boettke would have to call "Austrian." A final shortcoming of Boettke's view is that appreciation of

knowledge's richness, though a great virtue in an economist, simply does not make for any kind of field or research program. It does not make any sense to distinguish a body of research as "Austrian economics" because those studies appreciate knowledge's richness.

Again, the wing of Austrianism associated especially with Kirzner and Boettke externally trades on Hayek. But what really makes for the Austrian identity, even for that wing, is infatuation with Mises and his disciples Rothbard and Kirzner. I urge young classical-liberal economists to discover whether Hayek is really closer to Smith than to Mises, and to think carefully before identifying too strongly with Mises-Rothbard-Kirzner economics.

KEEPING OUR SIGHTS ON THE PUBLIC CULTURE

The development of an identity—functioning in both the professional and public cultures—would require a name. "Smithian" is merely tentative. Smith's works and messages are unwieldy and highly debatable. Further, it is probably never a good idea to build on the identities of individuals (the success of "Christian" notwithstanding). It isn't all in Smith, of course, and there's plenty to dispute and to disagree with. Finally, it might be good for the name to use an exotic term that we can infuse with the meaning we wish it to have.

I do not have any great suggestions. I like "spontaneous order," but "spontaneous order economics" is unwieldy, both in its length and its resistance to abridgement—would we speak of "spontaneous orderists"?, of "spontaneous orderism"?

Here the main issue is the need of identity. If that is something Smithians agree on, they can later focus on the name.

Whatever the name, what would have to follow are adoption, endorsement, and institutionalization. We would want to make use of the name in naming journals, associations, programs, and so on. One tactic would be to slap it onto existing institutions. For example, the present journal could become: *Econ Journal Watch: A Smithian Journal of Economic Criticism*, and later just the *Smithian Journal of Economic Criticism*.

Here the Austrians are exemplary. Murray Rothbard and Israel Kirzner had visions of an Austrian identity, and their followers have carried out those visions with journals, book series, and associations called "Austrian." If that identity is now moribund, it is not from any failure in marketing. For a few decades Austrianism had cornered an important part of the market for young economists who sought an identity that would satisfy libertarian sensibilities, scientific self-image, and disenchantment with formalism.

Other experiences may be instructive. "Public choice," "Chicago school," and "free-market" have functioned as identities, but none have sustained a coherent character and place in the professional and public cultures. One thing that rec-

ommends "Smithian" is the breadth of the frame that it implies about the context of the discourse in which we are engaged. Adam Smith is probably liberalism's best all-around representative. If "Smithian" is a bit woolly, it is also durable.

Another thing I like about "Smithian" is that it would travel reasonably well. Truth be told, Smithian economists often have more affinity with Smithian types in other fields than they do with "normal" economists.

We must keep our sights on the public culture. "Smithian" is something that journalists, authors, teachers, bloggers, mothers, fathers, clerics, business people, public officials, and community leaders could, in principle, relate to:

> — "Oh, yes, Adam Smith, the invisible-hand guy, the Scottish enlightenment, yes, yes! Didn't he see people as inherently morally reflective and sociable? Wasn't he also opposed to slavery and imperialism? He was sort of an egalitarian, right? Didn't he influence the American Founders? Didn't he call it 'natural liberty'? But he acknowledged exceptions and thought exceptions should be regarded as exceptions, right? I think he's onto something with that 'impartial spectator' stuff, but I'm not quite sure I get it …"

Exploring Adam Smith would be a vehicle for developing an identity in economics and society at large.

Embracing Heterogeneity—While Not Overdoing It

As economists, we all know the desire to address young students or sympathetic readers with the authority of economic science. In teaching introductory economics, we are reluctant to admit the extent to which economists differ. One reason that chemistry is so authoritative is that chemists agree on the things taught in Chemistry 101. Economists would like their auditors to ascribe to them a similar kind of authoritativeness. To acknowledge important differences among economists is to invite doubt about what any particular economist says, and possibly about the way he teaches the course.

Discourse is contextual and affords some wiggle room. The point here is that Smithian economists face great challenges that call for the embrace of at least some heterogeneity. Advanced economics students know about differences. They know that Nobel laureates disagree profoundly about important things. As for the general public, they know that professors disagree. Journalists instinctively look for opposing voices.

Think of all the classrooms and public forums that you are *not* party to, and which are led by economists of other sorts. They too downplay heterogeneity and claim for themselves the authority of a unified economic science. In their hands,

the presupposition of character homogeneity might be dangerous, and it is weakened to the extent that others accentuate heterogeneity.

Embracing heterogeneity has the virtue of being open and honest about the differences. If you tell your students that your judgments on certain matters are not those of all economists, they might appreciate it. Admitting heterogeneity allows one to express one's judgments more freely and fully, to really characterize a penetrating and powerful way of seeing things.

Seeing differences need not sunder all common ground among economists. All economists can share a familiarity with core topics and agree on many basics, such as the need to think through the individual's incentives as she understands the situation. That point of view leads directly into ideas of scarcity and trade-offs. Some such characteristics will continue to span all economists. The vision is for the Smithian economist to function in the profession and the public culture as a recognized and accepted type of economist.

WHAT SHOULD YOUNG SMITHIAN ECONOMISTS DO?

Again, Smithian economics appreciates some by-and-large political-economy verities that allow and help to justify a presumption of liberty, but, mainly, assumes a posture that is knowingly critical of interventionist economics. Smithian economists can make academic careers from pursuing research projects, using whatever methods make sense, that illustrate or refine the by-and-large verities—particularly about the relative fecundity and agreeable creativity of liberal policy—and that criticize misguided policies and illuminate their unintended consequences. Also, they may explore, develop, and refine the limits of and exceptions to the by-and-large verities—for example, asking, When is coercion our friend? Doing so will develop the large themes and messages we have to offer the culture in general—themes and messages that, as I see it, are represented more exquisitely by Adam Smith than by any other thinker. There is plenty of scope for doing Smithian research in many if not most of the normal fields of economics. Many young economists are doing just that. The challenge is to build a Smithian identity among them.

FROM CHARACTER TO IDENTITY

Many a Smithian economist thinks of him- or herself as simply "economist." But to develop Smithianism we must draw distinctions and stir controversy *within* economics. Most of those who are recognized as "economist" are at considerable variance with the Smithian character. Few maintain Smith's presumption of liberty. It is doubtful that most even subscribe to Smith's conception of liberty—instead gulled by taboos against taking liberty seriously. As for the judging of re-

search questions, modes of discourse, and audiences to address, few clearly exhibit Smithian attitudes.

In the United States, thousands of economists fit a broad Smithian character. Hundreds are members of the Association of Private Enterprise Education, the Public Choice Society, the Southern Economics Association, and the Society for the Development of Austrian Economics.

To function in both the professional and public cultures, Smithianism needs to go from character to identity. That would depend on admitting and embracing some degree of character heterogeneity within economics. Only if heterogeneity is recognized does it become possible to achieve widespread recognition of a Smithian character. As a functioning identity, the contest between it and alternative characters would be more meaningful and productive.

Questionnaire on Building a New Identity within Economics

[Economists invited to respond to the questionnaire will be contacted individually. Here is a draft of the material and questionnaire that will be sent to each.]

Dear Leading Economist,

The spirit of the questionnaire is exploratory—and more an interview than a survey. Feel free to enter discursive remarks at any point.

The matters treated by the questionnaire are rather philosophical. Your reflection is greatly appreciated.

The questionnaire has been constructed so that one can respond to it without reading the opening essay. However, you are encouraged to read the opening essay, and welcome to refer back to it. But that is not the expectation.

It's OK to keep your remarks brief and to leave individual questions unanswered.

The entirety of your responses will be included in a compendium online and announced at *Econ Journal Watch*.

This is an interview. We will not accept anonymous responses—your identity will be given with your responses.

The cultural context of the questionnaire is primarily the United States and secondarily the Anglosphere. If you wish to make the context more specific or respond in regard to some other context, please specify the countries or regions you have in mind.

I thank you in advance for your attention and participation.

Respectfully, Daniel Klein

QUESTIONNAIRE

1. Kindly provide your name:
2. One might think of a character-type of economist that is well represented by the following five economists: Adam Smith, Friedrich Hayek, Milton Friedman, Ronald Coase, and James Buchanan. For that character type, which five *additional* economists would you include in a top-ten list of representatives of that character type? We welcome remarks about each—and feel free to express reservations about the "fit" of any of the five posited so as to better delineate the character type you see as relevant here.
3. List the chief characteristics of such a character type. Elaboration is welcome. (You may wish to refer to the six numbered characteristics in the opening essay—for example by indicating what you would omit, add, or change.)

Now, it will be useful to have a term to denote the character type represented by your answers to questions nos. 2 and 3. That character is the type you see in the set of Smith, Hayek, Friedman, Coase, and Buchanan. (Notice that the initials of that set of five economists are SHFCB.) This questionnaire concerns *that* character type—as you see it, *not* as portrayed in the opening essay. Accordingly, let's call it "your-SHFCB."

We proceed in the expectation that there is a fair amount of overlap between the characters that people see in the SHFCB set.

4. Would you agree that your-SHFCB is not well identified today within the professional culture of academic economics?

☐	☐	☐	☐	☐	☐	☐
Strongly agree	Agree	Somewhat agree	Neutral	Somewhat disagree	Disagree	Strongly disagree

5. Would you agree that your-SHFCB is not well identified today within the public culture?

☐	☐	☐	☐	☐	☐	☐
Strongly agree	Agree	Somewhat agree	Neutral	Somewhat disagree	Disagree	Strongly disagree

6. Would you agree that your-SHFCB *stands in need of better identification*, within the professional culture, the public culture, or both?

□	□	□	□	□	□	□
Strongly agree	Agree	Somewhat agree	Neutral	Somewhat disagree	Disagree	Strongly disagree

7. Would it be beneficial for your-SHFCB to become an identity that functions in the professional and public culture? (We would welcome elaboration as to why or why not.)
8. Would you consider yourself as one who is of the your-SHFCB character type?

□	□	□	□	□	□	□
Definitely yes	Yes	Somewhat yes	Neutral	Somewhat no	No	Definitely no

Comments (We welcome remarks as to why or why not.):

9. How might you characterize yourself as an economist? Is there any particular "type" of economics you identify with? (Feel free to give multiple identifiers.)
10. List one or two character types other than your-SHFCB, and, for each, explain how it differs from your-SHFCB. (Be as brief or discursive as you like.)
11. Is an economics of a your-SHFCB type *viable* as an accepted identity (assuming it had an effective name) *within academic economics*?
12. Is an economics of a your-SHFCB type *viable* as an accepted identity (assuming it had an effective name) *within the public culture*?
13. An effective name of the identity would be important. Rate the following names for an economics of a your-SHFCB type. Comments welcome.

A. "Smithian economics"

□	□	□	□	□	□
Excellent	Good	OK	Weak	No good	Don't know

Comments:

B. "Smith-Hayek economics"

□	□	□	□	□	□
Excellent	Good	OK	Weak	No good	Don't know

Comments:

C. "Hayekian economics"

☐	☐	☐	☐	☐	☐
Excellent	Good	OK	Weak	No good	Don't know

Comments:

D. "Spontaneous order economics"

☐	☐	☐	☐	☐	☐
Excellent	Good	OK	Weak	No good	Don't know

Comments:

E. "Liberal economics"

☐	☐	☐	☐	☐	☐
Excellent	Good	OK	Weak	No good	Don't know

Comments:

F. "Classical liberal economics"

☐	☐	☐	☐	☐	☐
Excellent	Good	OK	Weak	No good	Don't know

Comments:

G. "Free-market economics"

☐	☐	☐	☐	☐	☐
Excellent	Good	OK	Weak	No good	Don't know

Comments:

14. Is there some other name you'd suggest for a your-SHFCB identity? If so, please do, and explain why.

Thank you again for your attention and participation.

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Econ Journal Watch,
Volume 5, Number 3,
September 2008, pp 373-379.

Uncovering the Costs of the Iraq War

Fred E. Foldvary[1]

Abstract

A key benefit of studying economics is to better see the reality beneath appearances. Two simple examples are often taught in economics courses. One is the difference between accounting profit and economic profit, the real gain being the economic profit, which subtracts from revenue not just the explicit costs recorded by accountants but the implicit opportunity costs of the owner's labor and asset yields. A second example is the difference between nominal and real interest rates, the latter being adjusted for inflation. Note that the adjective "economic" represents reality, in contrast to possibly deceptive reports and accounts.

Although we can dispute questions about the costs and benefits of war with respect to the moral, political, and security consequences, our viewpoints should be informed by the real economic costs. In their excellent book on the war in Iraq, Joseph Stiglitz and Linda Bilmes have done what economists are supposed to do: bring to light the economic reality. Regardless of one's assessment of the big questions, they provide us with the data and analysis of the war's real costs.

The three trillion dollars stated in the title is the cost only to the United States, excluding Iraq and the rest of the world. (Impacts abroad are discussed in a separate chapter.) The three trillion dollars figure calculates the direct spending on the war in Iraq (how much money has been appropriated for the war in Iraq) plus the indirect costs, starting with the 2003 invasion. The U.S. government's reported cost is the superficial appearance. Stiglitz and Bilmes go beyond that number to also reveal the implicit costs, including liabilities that will bear on future accounts.

The estimated liabilities of the Iraq war depends on the expected future course of the conflict. The authors provide two estimates, a "realistic-moderate"

1 Lecturer in Economics, Santa Clara University. Santa Clara, CA 95053.

case and a "best" case for a "rosy scenario for the wind-down of the war" (131). The explicit realistic-moderate budgetary costs calculated by the authors total $2.655 trillion, with the best case cost of $1.754 trillion (57). The realistic-moderate total cost of caring for veterans is put at $717 billion, with the best case at $422. For the costs of lost lives, the authors use the "value of a statistical life" of $7.2 million (95), which puts the additional social economic costs at $415 billion realistic-moderate and $295 best case. The total budgetary plus social costs are thus $2.016 trillion best case and $3.095 realistic-moderate (112). The total macroeconomic cost is $1.9 trillion realistic-moderate and $263 billion best case, which includes the oil price impact of $800 billion realistic-moderate and $263 best case. The total budgetary, social, and macroeconomic costs come to $4.995 trillion realistic-moderate and $2.279 trillion best case (130), not including interest on the extra debt.

The authors note that even the direct cost of the war is difficult to calculate. During past conflicts, the Pentagon usually established a separate account to keep track of operation funds. However, no such distinct account exists for the war in Iraq. As the authors state, "War and baseline funds are mixed in the same accounts" (9).

That the authors had to dig into the details of the federal budget to extract the data points to another serious issue, the sloppy accounting practices of the federal government. The government's accounting is so shoddy, say Stiglitz and Bilmes, that the Securities and Exchange Commission would prosecute any private firm with such a mess. The federal government uses cash accounting, which by ignoring liabilities, makes the costs appear to be much lower than they are. (However, the federal government does publish the *Financial Report of the United States Government*, showing its financial position using accrual accounting.) Even worse, the government has not made it easy to obtain figures; the uncovering of some of the data required the use of the Freedom of Information Act by veterans' organizations.

Another factor distracting the public away from understanding the direct cost of the war is that the military operations have been almost entirely funded via a series of "emergency" supplemental spending bills totaling in the hundreds of billions. This budget gimmick makes it possible to avoid painful budget choices since "emergency spending" is exempt from the budget caps designed to set an upper limit on government spending.

The professional budget staff in Congress is therefore unable to do its usual thorough review of the numbers, and there has been little Congressional oversight, since emergency spending takes place mainly outside of the regular budget process. Congress is not blameless in this process, as it has used the war to attach special and local interest spending to war bills with minimal scrutiny, despite the legal requirement to separate war spending from regular defense appropriations. The corruption is spread throughout the government.

Stiglitz and Bilmes show that many billions of dollars have been misdirected and wasted. The waste itself increased the costs and exacerbated hardships of the troops and of the Iraqi people. An example they provide is the $18.4 billion that Congress approved for the reconstruction of Iraq. Rather than being spent for the intended purpose, the money was held up as the Department of Defense squabbled with Congress on how to allocate it, and then when it was spent a year later, because of escalated violence, the funds were spent on security instead. The administration sought to keep accounting costs low at the beginning of the war, which not only postponed the payments to the future, but made the ultimate costs much greater.

The authors contend that another source of waste is the contracting out of many of the tasks. Contracting can be cost effective if it is done by comparative bidding—though cost effectiveness might not be a good if the endeavor isn't. At any rate, the Bush administration, in a hurry to conduct the operations, has often used "sole-source bidding." With cost-plus contracts, the incentive is to incur costs. Even if this was unavoidable at the beginning of the war, justification is lacking for the multi-year contracts that have been awarded.

The authors note various opportunity costs of the war in Iraq, where the funds could have been better utilized. The trillions spent in Iraq could have been spent to reduce the federal debt, provide tax cuts, provide domestic government services, or enhance efforts in Afghanistan, where it might have prevented the resurgence of the Taliban. With the lack of honest and proper accounting, the incompetence, the political games, and opportunity costs, Stiglitz and Bilmes state that "For students of 'government failure,' the Iraq war is a case study" (xix).

Once Stiglitz and Bilmes add up all these costs, they end up with a much higher number than the one generally reported by the administration. The authors contend that as a total real amount, the Iraq war has already cost the US economy more than any other war except for World War II. (The authors, however, do not calculate the costs as a fraction of GDP.) They report the cost of World War II in 2007 dollars at $5 trillion, so, as it continues, the absolute costs (rather than relative to GDP) of the Iraq war could surpass World War II. They report the cost per troop (soldier) for the Iraq war at four times the $100,000 cost per troop of World War II (which, one should add, could be justified as substituting technologically advanced capital goods and human capital for troop numbers).

With respect to public policy and public choice, the authors observe that typical Americans have not personally felt or observed the huge costs of the war. The personal costs at present are concentrated in the military and its contractors, and their families. But since the federal budget was already in deficit when the war began, the extra costs to the budget have been borrowed, much from abroad. The financial costs are potential liabilities on taxpayers (and possibly on the holders of treasury bonds if greater inflation reduces the real debt), but it has not yet resulted in a loss of personal treasure except in higher prices.

The costs of war in terms of resources always impact people living in the present day. The guns of war have a present-day opportunity cost of fewer civilian goods produced. As we teach our students in the production possibilities graph, more guns implies less butter. The opportunity cost includes less domestic investment in capital goods and technology, which then reduces future growth and living standards. But the typical person does not observe these costs, as they are hidden in higher prices and would-have-been life improvements, as resources get pulled away from civilian production by the government's bidding higher prices to hire the labor and materials, or as government orders members of the national guard to vacate their civilian jobs and shift over to military service, which then increases costs as these workers get replaced.

Nevertheless, the financial costs of a war are shifted to future generations when the funds are borrowed. This raises the issue of the best policy for financing wars: by taxes or debt? In their proposals for reform, Stiglitz and Bilmes favor taxation, echoing the thought of Adam Smith in the *Wealth of Nations*. Smith wrote that the citizens are much less likely to approve a war if it is financed by taxes rather than by debt: "when war comes," governments are unwilling "to increase their revenue in proportion to the increases in their expence. They are unwilling, for fear of offending the people... who would soon be disgusted with the war... The facility of borrowing delivers them from the embarrassment which this fear ... would otherwise occasion" (Smith 1776, 919). Moreover, "Were the expence of war to be defrayed always by a revenue raised within the year, the taxes ... would last no longer than the war... Wars would in general be more speedily concluded and less wantonly undertaken" (925-6).

Better knowledge of the costs is vital to the public's attitude towards the war. The cost per U.S. household is over $100 per month for the Iraq war (35). Even if they are not being taxed to pay for the war, if the public knew the costs, opposition would be substantially greater. And if they were asked whether they would pay $100 per month to pay for it, a massive rejection of the war would not be surprising. Stiglitz and Bilmes have thus not just engaged in an academic exercise but have provided the public the means to better judge the costs and benefits of the war.

An economic analysis of the war has to consider counterfactual alternatives. What would likely have happened had the U.S. government not invaded Iraq? The authors consider the case of continued enforcement of the no-fly zones and other pre-war measures, but one should also consider that the pre-war trade restrictions on Iraq were weakening, the oil-for-food program was being exploited by the Iraqi regime for its own gains, and possibly the Iraqi regime would have restored its oil revenues and weapons programs and continued to promote attacks on Israel. Moreover, Iraq is no longer a military threat to its neighbors. The authors might have done more counterfactual analysis, as there may well have been alternatives to war for dealing with these issues.

Stiglitz and Bilmes claim that the war in Iraq has contributed to the rise in the price of oil. They note that the price of oil began to surge just as the war began, and rose beyond the range indicated by futures markets. Their estimate for the price increase is $5 to $10 per barrel, which was dwarfed by the increase in the price in 2008, after their book was written. Most likely the price of oil would have gone up substantially even in the absence of the Iraq war, but indeed by reducing production in Iraq and with the demand for fuel by the military in Iraq, the war in Iraq did indeed contribute to the price increase.

Costs need to be compared to benefits, and some pro-war voices have argued that the previous Iraqi regime would have become a greater threat to its neighbors. Stiglitz and Bilmes argue that the Iraq war "has not increased stability and security in the Middle East. It has not reduced the threat of terrorism" (128). They also note a global opposition to U.S. unilateralism, and the sentiment by many Muslims that the United States is acting against Islam. While one can dispute such arguments, the pro-war case also needs better grounding, as it often seems to presume that the benefits are infinite, worth any cost.

Stiglitz and Bilmes discuss various costs and issues for exiting Iraq, but do not mention the possibility of a plebiscite. If most Iraqis wish American forces to leave, a plebiscite would provide the U.S. government with political cover for an exit. If the vote were in favor of U.S. troops staying for the time being, it could blunt the Iraqi rebel attacks. Perhaps both the opponents and supporters of the war avoid talk of a plebiscite because the results could go against them.

There is also a useful chapter on "Learning from Our Mistakes: Reforms for the Future." The first proposed reform is that wars not be funded through emergency supplemental appropriations after the beginning of the war. They should have gone further and proposed this as a Constitutional amendment, rather than a statute which could be more easily amended later.

As one would expect, some of their proposals seek better accounting methods. On the fiscal side, Stiglitz and Bilmes's reform #9 is in accord with Adam Smith, proposing that the costs of a war lasting more than one year should be borne by the current taxpayers. However, they say this should be with "a war surtax" (197). But an increase in marginal income tax rates would cause a greater than proportional increase in the excess burden of the tax, increasing the economic cost by significantly more than the tax cost. Given a war expense, why not at least mention, as Smith did, the tax on land value or ground rent, which has little or no deadweight loss? (Perhaps they should read my "Plea to Public Economists" which appeared in this journal in 2005).

A further strength of the book is its appendices, which treat the evolving Department of Defense web sites as well as their methodologies. The book has copious notes and an index.

Stiglitz and Bilmes have done a superb job in documenting and analyzing the economic costs of the Iraq war. The book would have been even better if they

had included the literature on the costs of war, particularly the classical liberal thought that opposed war. For example, John Denson's *The Costs of War: America's Pyrrhic Victories* examines the costs as well as the misleading rationales for America's past wars. The authors could also have broadened the work by placing the war in Iraq in the broader context of American foreign policy.

Some past statements on war resonate today in the context of the past cold war and the current war on terror as well the shooting wars in Iraq and Afghanistan. In discussing the anti-colonial thought of John Mill, Edmund Silberner (1972, 44) states, "Nowhere, perhaps, has fear produced as much harm as in the domain of national security. Bad governments easily persuade their frightened peoples that they can never enjoy enough security."

The classic pithy connection between war and state power was made by Randolph Bourne, an opponent of America's entry into World War I, who was arrested for his stance. A poem by John Dos Passos (1932, 106) has Bourne's ghost crying out, "*War is the health of the state*" (italics in the original), which Bourne had written in an unpublished manuscript called *The State*. This recognition, that war enhances state power, was recognized by James Mill. Silberner (1972, 44-5), discussing Mill, states, "nothing increases more than war the volume of that part of the national wealth which is subject to the authority of the state." (See also Higgs (1987) for the historical inducement to greater power by war).

In classical economic thought, free trade was put forth as the antithesis of colonies and wars to extend and protect colonial territory. Henry George (1886) wrote that free trade has been "the extinguisher of war, the eradicator of prejudice, the diffuser of knowledge" (52).

Adam Smith (1776, 617) thought Great Britain would benefit from granting the American colonies their freedom: "Great Britain would not only be immediately freed from the whole annual expence of the peace establishment of the colonies, but might settle with them such a treaty of commerce as would effectually secure to her a free trade, more advantageous to the great body of the people, though less so to the merchants, than the monopoly which she at present enjoys." The War in Iraq is better understood in the context of this anti-empire thought, as this war can be seen as a continuation of an imperial policy, with overseas wars of choice going back to the Spanish-American War of 1898. In this journal, Christopher Coyne and Steve Davies (2007, 11-15) have offered a 20-point overview of the common public bads of empire, nation building, and the like.

But even absent this context, we can salute Stiglitz and Bilmes for doing the good economic work of taking us beyond the superficial accounts of the war, and thereby helping people better understand the colossal costs of this war.

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About the Author

Fred Foldvary teaches economics at Santa Clara University in California. His books include *Soul of Liberty*, *Beyond Neoclassical Economics*, *Public Goods and Private Communities*, *Dictionary of Free-Market Economics*, and (co-edited) *The Half-Life of Policy Rationales: How New Technology Affects Old Policy Issues*. Foldvary's main areas of research include public economics, social ethics, real estate economics, and private communities. His website is www.foldvary.net; his email is ffoldvary@scu.edu.

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Econ Journal Watch,
Volume 5, Number 3,
September 2008, pp 380-381.

CORRESPONDENCE

WHERE THERE'S SMOKE...

Editors,

I enjoyed the excellent and insightful discussion "Honestly, Who Else Would Fund Such Research? Reflections of a Non-smoking Scholar," by Michael Marlow, in the May 2008 issue of *Econ Journal Watch* (link). I learned a lot from what the author had to say in that article.

Through my membership and participation in the eco-spiritual community, (Children of Gaia Iseum, Fellowship of Isis), as well as my involvement with informal social and political activism networks such as The Anti-Fascist League, I contributed to a social atmosphere in which unfairly judging a researcher's integrity on the basis of their source of funding has become commonplace. Yes, I was a true believer, for nearly thirty years, that corporate funding necessarily creates bias favorable to the funder. I didn't have to actually read corporately-funded studies to justify denouncing them as frauds and their authors as corrupt. I "magically" knew this must be the case. I behaved like an ignoramus with regard to this matter and I deeply regret having engaged in pre-judgment and unfairly maligning academics involved in such research.

I am not generally disposed to pre-judgment, but I sincerely believed that there was essentially unlimited access to genuinely "neutral" sources of funding. I had been led to believe this was the case, by the writings of leaders in the fields of deep ecology and consumer protection. Therefore, I concluded that researchers working for corporations must be motivated by nothing but greed.

It has only been within the past few years, as a result of intensive research I've conducted, that I've woken up to the reality that genuinely "neutral" sources of research funding are almost entirely mythical. The reality is that government and non-profit funding is severely limited in some areas and is itself controlled by people who have social, political and public policy agendas of their own. I have finally realized that, if an academic wishes to pursue a hypothesis that is unlikely to support the agendas of government agency or non-profit funders she may find herself with no alternative but to seek or accept corporate funding, that the very fact of her having received corporate funding will be employed by people like I used to be, to dismiss her and her work without even reading it.

I had always believed that government agencies and non-profit organizations involved in public health promotion, social justice, and consumer protection were involved in uncovering and denouncing biased corporate-funded research because they wanted ALL research to be genuinely unbiased. I never imagined, until recently, that some of them were eliminating competition for calculated control over public and policy-maker's thinking. And then I discovered Social Marketing and "health communications"—was my naive bubble burst!

Rather than clearing the field of deceptive, manipulative misrepresentation, some of these agencies and groups were actively copying the very same techniques and technologies employed by industry, for their own purposes. And this included and continues to include commissioning research studies that have very little to do with advancing scientific knowledge and everything to do with calculated propaganda! So much for "the public interest." I don't trust any of these agencies or groups anymore, (but I do actually read the studies, now, since I can no longer trust anything that anyone else tells me about them).

Another person who has addressed these issues with uncommon integrity is A. Thomas McLellan. His article "Implicit Demand Characteristics in Research Funding Sources—It's Not Just Some Sources," published in *Addiction*, also upsets taboos by arguing that institutionalized bias exists on all sides of research funding (McLellan 2007). He is one of a very select few other writers to have confronted these issues with the kind of honesty and candor found in Marlow's article.

Respectfully,

Roy Harrold
Edmonton, AB, Canada
edmontonguys@gmail.com

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