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Comments

The Soviet Economic Decline Revisited,
Brendan K. Beare 135-144

Reply to Beare,
William Easterly and Stanley Fischer 145-147

The Diluted Economics of Casinos and Crime:
A Rejoinder to Grinols and Mustard's Reply,
Douglas M. Walker 148-155

Connecting Casinos and Crime:
More Corrections of Walker
Earl L. Grinols and David B. Mustard 156-162

Smoking in Restaurants: Rejoinder to Alamar and Glantz
David R. Henderson 163-168

Externalities in the Workplace: A Response to a Rejoinder
to Response to a Response to a Paper,
Benjamin C. Alamar and Stanton A. Glantz 169-173

Investigating the Apparatus
Symposium: Gender and Economics

Reaching the Top?
On Gender Balance in the Economics Profession,
Christina Jonung and Ann-Charlotte Ståhlberg 174-192

On Gender Balance in the Economics Profession,
Ann Mari May 193-198

Mr. Max and the Substantial Errors of Manly Economics,
Deirdre N. McCloskey 199-203

Diversity in Tastes, Values, and Preferences:
Comment on Jonung and Ståhlberg,
Catherine Hakim 204-218

Preferences Underlying Women's Choices
in Academic Economics,
John A. Johnson 219-226

What is the Right Number of Women?
Hints and Puzzles from Cognitive Ability Research,
Garett Jones 227-239

Christina Jonung and Ann-Charlotte Ståhlberg will reply in the September 2008 issue.

Character Issues

Honestly, Who Else Would Fund Such Research?
Reflections of a Non-Smoking Scholar,
Michael L. Marlow 240-268

Entire May 2008 Issue (1.8 MB) 135-268

Econ Journal Watch,
Volume 5, Number 2,
May 2008, pp 135-144.

The Soviet Economic Decline Revisited

Brendan K. Beare[1]

A comment on: William Easterly and Stanley Fischer, "The Soviet Economic Decline," *World Bank Economic Review* 9(3), September 1995: 341-371.

Abstract

In their paper "The Soviet Economic Decline", published in the *World Bank Economic Review* in 1995, William Easterly and Stanley Fischer study the decline of the Soviet economy during the period 1950-1987. The authors begin by showing that, conditional on standard growth determinants such as national investment and human capital accumulation, Soviet per capita economic growth was the worst in the world, and worsening, 1960-1987. This is despite the fact that during the 1950s Soviet growth per capita was significantly above the world average. The purpose of Easterly and Fischer's study is to explain this poor economic performance. Two candidate explanations—that the Soviet economy was overly burdened by excessive military spending, and that central planning stymied the effectiveness of spending on research and development—are briefly considered, but Easterly and Fischer find that neither provides a plausible explanation for the extreme nature of the Soviet experience.

Instead, Easterly and Fischer focus on an explanation for the Soviet growth slowdown known as the extensive growth hypothesis, or low elasticity of substitution hypothesis. Extensive growth refers to growth that is driven primarily by input accumulation rather than productivity growth. As discussed by Easterly and Fischer, the decline in Soviet economic growth after the 1950s was accompanied by a substantial increase in the national investment rate, which more than doubled between 1950 and

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The first version of this comment was written while the author was a graduate student in the Department of Economics at Yale University. I thank Timothy Guinnane, Valery Lazarev, Peter Phillips, an *EJW* reader, and an anonymous referee for their comments, and Yale University and the Cowles Foundation for financial support. The opinions expressed here are my own.

1987. Similar increases in the investment rate were experienced in a number of newly industrializing East Asian economies, including Japan and Korea. Whereas extensive growth via capital accumulation led to rapid economic growth in much of East Asia, the rising investment rate in the Soviet economy was accompanied by a declining rate of growth. The extensive growth hypothesis, proposed earlier by Weitzman (1970), posits that this decline was due to sharply diminishing returns to capital brought about by a low elasticity of substitution between capital and labor. Easterly and Fischer argue that the elasticity of substitution was indeed much lower in the Soviet economy than in the newly industrializing East Asian economies, and suggest that the difference may be fundamentally related to the contrasting nature of planned and market economies.

The model of the Soviet economy used by Easterly and Fischer is a constant elasticity of substitution (CES) growth equation:

$$\ln Y_t = \delta_0 + \delta_1 t_{50-59} + \delta_2 t_{60-69} + \delta_3 t_{70-79} + \delta_4 t_{80-87} + \frac{\gamma}{\gamma - 1} \ln\left[\alpha K_t^{(\gamma-1)/\gamma} + (1-\alpha) L_t^{(\gamma-1)/\gamma}\right] \tag{1}$$

This is labeled as equation 6 in Easterly and Fischer's paper (with ln L_t subtracted from both sides of the equation). In each period t, Y_t denotes output, K_t denotes capital, and L_t denotes labor. t_{50-59} is equal to the product of t and a dummy variable that is equal to one in periods corresponding to the years 1950 to 1959, and equal to zero in other periods. The other trend variables are defined similarly. The parameters $\alpha, \gamma \in (0,1)$ correspond to the share of capital in production and the elasticity of substitution between capital and labor. The log of technology, assumed by Easterly and Fischer to be Hicks-neutral, is equal to the sum of the first five terms on the right hand side of (1). Thus, δ_1 through δ_4 represent decade-specific rates of technical change. The purpose of the decade-specific trend functions is to allow for a modicum of flexibility in the evolution of technological progress over the sample period.

Easterly and Fischer seek to appraise the extensive growth hypothesis by empirically estimating (1) using nonlinear least squares. In a CES model of economic growth, an elasticity of substitution below one implies decreasing returns to capital accumulation. Thus, an estimate of γ significantly (in both the statistical and the economic sense) below one would provide support for the extensive growth hypothesis. Relatively constant estimates of δ_1 through δ_4 would provide further support for the extensive growth hypothesis, by showing that the bulk of the decline in Soviet growth can be explained by decreasing returns to capital accumulation. An estimate of γ close to one, and declining estimates of δ_1 through δ_4, would provide evidence against the extensive growth hypothesis.

Empirical estimation of (1) is complicated by the relatively poor quality of data for the Soviet economy during the period in question. Easterly and Fischer consider several annual datasets spliced together by Gomulka and Schaffer (1991) from various sources.[2] Three main sources are involved: a dataset constructed by the U.S. Central Intelligence Agency (CIA), a dataset constructed by the Russian economist Khanin, and the official Soviet statistics.[3] Easterly and Fischer report estimates of (1) using the CIA data at the total economy and industrial sector levels. They also discuss briefly the results obtained using the other datasets.

The results of Easterly and Fischer's analysis using the CIA total economy data are displayed in the column labeled *EF* in Table 1. These estimates are taken from Table 6 in their paper. Using the original data and the equation they specified,[4] I attempted to derive the same estimates. My own estimates are displayed in the column labeled *EF** in Table 1. The figures in columns *EF* and *EF** are similar, but not identical. Without the precise details of the numerical optimization routine used by Easterly and Fischer, or access to the code used to generate their results, it is difficult to know exactly why the figures differ. Note that the *R*-squared values in columns *EF* and *EF** are extremely close to one another. See McCullough and Vinod (2003) for a discussion of numerical discrepancies arising from differences between econometric software packages and optimization settings. The figures in column *EF** were obtained by performing a grid search over the nonlinear parameters γ and α, running from 0.01 to 0.99 in increments of 0.01, and estimating the other parameters by linear regression conditional on γ and α at each point in the grid. This approach was used to avoid the sensitivity of results to the choice of starting values in more sophisticated optimization routines, a problem which appears to be substantial in this instance.

The estimates in column *EF* appear to provide striking support for the extensive growth hypothesis. The estimated elasticity of substitution is 0.37; well below one. Moreover, the estimated standard error for this parameter is only 0.04. The capital share parameter is estimated at 0.96, with a standard error of only 0.01. This estimate seems extremely high, even in view of the intensive capital accumulation in the Soviet economy during the sample period. The rates of technical change are less precisely estimated than the other parameters, but it is notable that they do not appear to decline over the sample period. The same qualitative statements hold true for our own estimates reported in column *EF**.

2 The data are downloadable from the World Bank website: www.worldbank.org.

3 The CIA data are at the level of the whole Soviet economy, and at the level of the Soviet industrial sector. Khanin's data are at the level of the material sector – roughly speaking, that part of the economy that produces material goods – while the official statistics are at the material and industrial sector levels.

4 All computations in this comment were carried out using Ox version 5.00. Ox is free for academic use and may be downloaded from www.doornik.com. The .ox files used to generate the results in Tables 1 and 2, and suitably formatted data files, may be downloaded from www.nuff.ox.ac.uk/users/beare/soviet.

Table 1: Parameter Estimates for the Soviet Production Function, 1950-1987

Parameter (or statistic)	Estimate (or value)				
	EF	*EF**	*CIA*	*KHA*	*OFF*
Elasticity of substitution (γ)	0.37	0.38	0.75	0.99	0.04
	(0.04)	(0.05)	(0.59)	(2.20)	(0.02)
Capital share (α)	0.96	0.96	0.53	0.82	0.82
	(0.01)	(0.02)	(0.53)	(0.19)	(0.15)
% rate of technical change, 1950-59 ($100\ \delta_1$)	1.09	0.99	2.90	2.47	0.76
	(0.32)	(0.33)	(1.14)	(0.38)	(0.21)
% rate of technical change, 1960-69 ($100\delta_2$)	1.10	0.95	1.35	-0.34	5.63
	(0.35)	(0.43)	(0.67)	(0.51)	(0.15)
% rate of technical change, 1970-79 ($100\delta_3$)	1.16	1.08	0.61	-0.48	4.18
	(0.36)	(0.43)	(0.96)	(0.44)	(0.10)
% rate of technical change, 1980-87 ($100\delta_4$)	1.09	0.97	-0.14	-0.02	2.60
	(0.34)	(0.40)	(1.08)	(0.12)	(0.15)
R^2	0.9987	0.9988	0.9983	0.9990	0.9995
Durbin-Watson statistic	1.9228	1.9183	1.5715	1.4647	1.4334

Note: Estimated standard errors in parentheses. *EF*: Easterly-Fischer estimates based on CIA total economy data, equation (1); *EF**: Author's estimates based on CIA total economy data, equation (1); *CIA*: Author's estimates based on CIA total economy data, equation (2); *KHA*: Author's estimates based on Khanin material sector data, equation (2); *OFF*: Author's estimates based on official material sector data, equation (2).

The results obtained by Easterly and Fischer using the other datasets are mixed. Specifically, Easterly and Fischer claim to find support for the extensive growth hypothesis in the industry-level CIA data and in the material sector and industry-level official data, but no support in the Khanin data. The results obtained using the official data and the Khanin data do not appear in the version of their paper published in the *World Bank Economic Review*, but they may be found in two earlier working papers (1994a, 1994b).

Unfortunately, the results of Easterly and Fischer's study are faulty because their equation is faulty. By allowing the slope coefficients δ_1 through δ_4 to vary across decades while holding the intercept δ_0 fixed, Easterly and Fischer allow changes in the rate of growth of technology to become confused with changes in the level of technology. In particular, any decline in the rate of growth of technology between decades must necessarily be accompanied by a sudden decline in the level of technology.

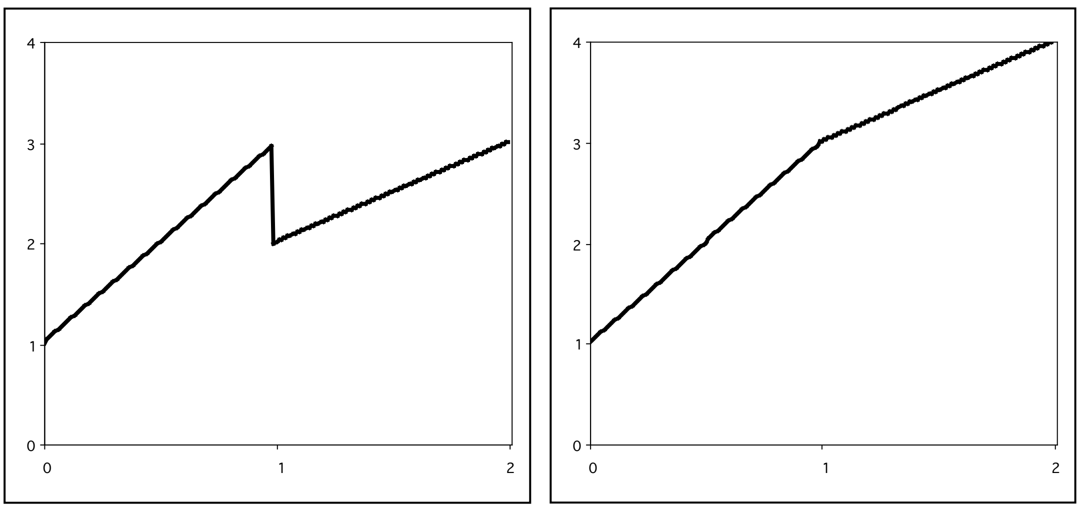
Figure 1 illustrates the problem. In panel (a), we have a graph of the equation

$$y = \begin{cases} 1 + 2x & x < 1 \\ 1 + x & x \geq 1, \end{cases}$$

while in panel (b), we have

$$y = \begin{cases} 1 + 2x & x < 1 \\ 2 + x & x \geq 1. \end{cases}$$

Figure 1: Two Piecewise Linear Functions



(a) (b)

The function graphed in (a) drops discontinuously when the slope decreases, whereas the function graphed in (b) does not. The example is banal, but it makes the point: If one wants to write down an equation for a continuous piecewise linear trend, one needs to allow the intercept term to vary along with the slope term. Easterly and Fischer's specification of the log-technology process is of the kind shown in panel (a). This is a serious deficiency because, if the data does not support a sudden decline in the level of technology at the end of each decade,[5] then estimated parameters will be less likely to reflect a decline in the rate of growth of technology, even if such a decline does match the data well. It seems safe to assume that this was not Easterly and Fischer's intention.

A more appropriate specification for the production function, presumably

5 Using the CIA total economy data, a Wald test of the hypothesis that the intercept term in equation (1) is constant, versus the alternative hypothesis of an intercept that may vary between decades, yields a p-value of less than 0.01.

consistent with the intended model of Easterly and Fischer, is

$$\ln Y_t = \delta_0 + \delta_1 t D_{50-87} + (\delta_2 - \delta_1)(t-10)D_{60-87} + (\delta_3 - \delta_2)(t-20)D_{70-87} + (\delta_4 - \delta_3)(t-30)D_{80-87} + \frac{\gamma}{\gamma - 1}\ln\left[\alpha K_t^{(\gamma-1)/\gamma} + (1-\alpha)L_t^{(\gamma-1)/\gamma}\right] \tag{2}$$

Here, $D_{50\text{-}59}$ represents a dummy variable that is equal to one during 1950-59 and zero during other years. The other dummy variables are also defined in the obvious way. This specification allows for a technology process with a rate of growth that varies across decades, but without inducing a sudden change in level. In other words, it is of the kind shown in panel (b) of Figure 1. Estimating equation (2) with the data used by Easterly and Fischer gives results very different from those obtained by estimating equation (1). These results are presented in the columns labeled *CIA*, *KHA* and *OFF* in Table 1. The three columns refer respectively to the CIA total economy data, the Khanin material sector data, and the official material sector data. Again, the estimates are obtained by employing a grid search over the nonlinear parameters γ and α.

Let us first compare the results in columns *EF* and *CIA*, as these columns correspond to the estimation of equations (1) and (2) respectively, using the same data. The most obvious difference between the two sets of results is that the estimated standard errors are far larger in the latter. The estimated standard error on the elasticity of substitution using equation (2) is so large that we can say almost nothing about the magnitude of this parameter with any confidence. The estimates for δ_1 through δ_4 are more interesting, and suggest a monotone decline in the rate of technical change. The Wald statistic corresponding to the hypothesis $\delta_1 = \delta_2 = \delta_3 = \delta_4$ has a *p*-value of 0.03, and so the hypothesis of a constant rate of technical change can be rejected at the 5% significance level.

The Durbin-Watson statistic corresponding to the results in column *CIA* of Table 1 is 1.57. This number is neither high enough nor low enough for us to be clear about whether serial correlation in the residuals presents a serious problem. Column *CIA* in Table 2 presents the results that were obtained when the production function was re-estimated with a correction for first order autocorrelation in the residuals, again using the CIA total economy data. Estimation for this column, and for the other columns of Table 2, was performed using a grid search over γ, α, and the autocorrelation coefficient. The results in column *CIA* are generally similar to those obtained without correcting for autocorrelation, but the estimated standard errors are even larger. None of the estimated parameters are statistically significant. The Wald statistic corresponding to the hypothesis $\delta_1 = \delta_2 = \delta_3 = \delta_4$ now has a *p*-value of 0.096. In sum, the data tells us almost nothing other than that the rate of technical change appears to be declining over the sample period.

Table 2: Parameter Estimates for the Soviet Production Function, 1950-1987, Using a First-order Correction for Autocorrelation

Parameter or statistic	Estimate or value		
	CIA	*KHA*	*OFF*
Elasticity of substitution (γ)	0.55	0.99	0.04
	(0.65)	(2.78)	(0.02)
Capital share (α)	0.75	0.84	0.85
	(0.78)	(0.22)	(0.16)
% rate of technical change, 1950-59 ($100\delta_1$)	2.52	2.26	0.48
	(1.64)	(0.46)	(0.32)
% rate of technical change, 1960-69 ($100\delta_2$)	1.48	-0.40	5.61
	(1.08)	(0.59)	(0.18)
% rate of technical change, 1970-79 ($100\delta_3$)	0.97	-0.53	4.18
	(1.69)	(0.51)	(0.13)
% rate of technical change, 1980-87 ($100\delta_4$)	0.27	0.02	2.61
	(1.63)	(0.15)	(0.18)
Autocorrelation coefficient	0.23	0.18	0.25
	(0.19)	(0.24)	(0.17)
R^2	0.9982	0.9989	0.9995
Durbin-Watson statistic	1.9413	1.7705	2.0741

Note: Estimated standard errors in parentheses. *CIA*, *KHA*, *OFF* defined as in Table 1.

Next, consider the results obtained when we estimate equation (2) using the Khanin material sector data. These results are given in column *KHA* of Table 1 when we do not correct for autocorrelation, and column *KHA* of Table 2 when we do. In Table 1, the estimated elasticity of substitution is 0.99, with a standard error of 2.20. This is for a parameter that is expected to be between zero and one. When we correct for autocorrelation—the Durbin-Watson statistic is again ambiguous—the estimated elasticity remains at 0.99, and the standard error increases to 2.78. Essentially, given the model we are estimating, Khanin's data tell us close to nothing about the elasticity of substitution. They do tell us that the rate of technical change appears to be significantly higher in the 1950s than in subsequent decades. The null hypothesis of a constant rate of technical change can be rejected using a Wald test at any reasonable significance level, whether or not we correct for autocorrelation.

Finally, consider the results obtained when we estimate equation (2) using the official material sector data. These results are given in column *OFF* of Tables 1 and 2, depending again on whether we include a correction for serial correlation. The effect of correcting for serial correlation is minor: in both cases, the estimated elasticity of substitution is 0.04, with a standard error of only 0.02. The estimated rate of technical change is low in the 1950's, increasing sharply in the 1960's, and then declining over the remainder of the sample. A constant rate of technical change can easily be rejected at any reasonable significance level. The extremely low estimate of the elasticity of substitution using the official material sector data is surprising, especially in view of the results obtained using the other data sets. It is worth bearing in mind that, as noted by Easterly and Fischer, the official Soviet data for this sample period are widely believed to be misreported.

I have not reported estimation results based on the CIA or official data at the industry level because they suffer from an acute autocorrelation problem: with both datasets, the Durbin-Watson statistic is less than 0.52. Further analysis suggests that the residual process for equation (2) using the industry-level datasets may lack mean reversion.

The results we have reported in the *CIA*, *KHA* and *OFF* columns of Table 1 and Table 2 reveal very little about the nature of the Soviet economy during the sample period. There is evidence that the rate of technical change was declining over the sample period, but we can say almost nothing about the elasticity of substitution or capital share parameters. The standard errors, and the discrepancies between data sets, are simply too large. This should not be surprising. Even if we ignore the poor quality of available data, estimation of a nonlinear model such as (2) using only thirty-eight observations, with a trend term that changes slope every ten periods, would seem to be extremely ambitious.

The apparent support provided to the extensive growth hypothesis by the results reported by Easterly and Fischer is clearly nothing more than an artifact of their inappropriate trend specification. What is surprising is that the defect in Easterly and Fischer's analysis has not been noted previously. It has been more than a decade since the paper in question appeared in the *World Bank Economic Review*. As of January 2008, a search of the Social Sciences Citation Index yields 41 citations of the paper (including the working paper versions). None of those 41 papers note the error. Many refer specifically to Easterly and Fischer's claim that the elasticity of substitution between capital and labor in the Soviet economy was significantly below one.

The simple error we have identified in Easterly and Fischer's paper brings to mind McCullough's (2007) discussion of the need for economics journals to ensure that published empirical studies are replicable; see also McCullough, McGeary and Harrison (2006). McCullough argues that the authors of published empirical studies should be required by journals to deposit data and code files into journal archives. Presumably, this practice should help to ensure against inaccuracies in published work. Yet, as noted earlier, the data used by Easterly and Fischer are publically available on the World Bank's website. While code has not been made available, this seems relatively harmless given the straightforward and familiar nature of the econometric techniques employed. The point illustrated in Figure 1 is trivial; the problem with Easterly and Fischer's trend specification would be obvious to anyone attempting to reproduce their results, or even just taking a careful look at equation (6) in their paper. It would seem that, despite the public accessibility of Easterly and Fischer's data, no researcher has felt the need to take a closer look at the striking empirical results reported in their study.

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About the Author

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Go to reply by Easterly and Fischer

Go to May 2008 Table of Contents with links to articles

Go to Archive of **Comments** Section

Econ Journal Watch,
Volume 5, Number 2,
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Reply to Brendan Beare

William Easterly[1] and Stanley Fischer[2]

A Reply To: Brendan K. Beare, "The Soviet Economic Decline Revisited," *Econ Journal Watch* 5(2), May 2008: 135-144. Link

Abstract

We are grateful to Beare (2008) for the valuable service of re-checking our old results (Easterly and Fischer 1995). Beare's main point is correct: we made a careless error in constraining the intercept of the time trend to be constant while allowing trend coefficients to vary across decades. The consequence of this constant intercept assumption was not that our estimated equation produced crazy jumps at the end of each decade, as Beare's Figure 1 might be taken to imply. It was instead that we biased the trend coefficients to be equal across decades (as Beare recognizes soon after Figure 1). So we gave biased support to our story that the slowdown in Soviet growth was *not* because of slowing TFP growth but rather was because of the extensive growth strategy of reliance on capital accumulation with a sharply falling rate of return to capital caused by a very low elasticity of substitution between capital and labor.

Of course, while we failed to do a fair test of the assumption of constant TFP growth, the assumption could still be correct. Our parsimonious model of a nearly constant TFP growth rate and low elasticity of substitution DID fit the data very well with sensible parameter values (except perhaps for our very high capital share, although even that could reflect a situation of initial surplus labor in a non-market economy).

Unfortunately, the corrected model provided by Beare does not provide a very clear verdict on whether our story was correct or not. It would be desirable to relax

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We are grateful to Tobias Pfutze for diligent and thoughtful research assistance.

our rigid assumptions so as to be able to test the alternative hypothesis of fluctuating and/or declining TFP growth and a higher elasticity of substitution in a completely unconstrained model. However, as Beare implicitly notes, it may be overly ambitious to estimate a fully flexible spline regression over time combined with a nonlinear function of capital with only 38 observations, which may not provide enough information content to allow sharp nested tests in a completely unconstrained model.

Indeed, in Beare's piecewise continuous time trend using the CIA data, he finds that he can reject that TFP growth rate is equal across decades, but he cannot reject that the capital share is zero, and he cannot pin down the elasticity of substitution anywhere between zero and one. Beare's 95% confidence interval for the capital share is {-0.724, 1.824} and that for the elasticity of substitution is {-0.7788, 2.2788}. In his results, we do not know whether capital has a positive (or negative) effect on output in any time period, whether capital and labor have zero substitutability or such high substitutability that labor is not even necessary in the long run (the latter would have made extensive growth very feasible). Hence, Beare is not able to go from his finding of decelerating TFP growth to also refute our story that stressed the low elasticity of substitution and sharply declining marginal product of capital as the downfall of the extensive growth strategy.

We tried a more parsimonious specification of the TFP trend which turned out to allow us to still precisely estimate the capital share and the elasticity of substitution. This was simply specifying a quadratic function of time for the TFP element. The results were as follows:

Model with Trend and Squared Trend:
ln(Y)=C1+C2*T+C3*T^2+gamma/(gamma-1)*ln(alpha*K^(gamma-1)/gamma+(1-alpha))

Parameter	Estimate	T-Stat
C1	-0.3844	-4.11
C2	0.0243	3.08
C3	-0.0003	-2.85
alpha (capital share)	0.83	11.45
gamma (elasticity of substitution)	0.49	7.56

The coefficient on the squared trend is significant and negative, indicating that we were wrong on constant TFP growth—TFP growth did decelerate.[3] Since this exercise was inspired by Beare's criticism, we are grateful to Beare for correcting the part of the story that implied constant TFP growth. However, the elasticity of substitution is still very low, indicating that there were also sharply di-

3 The turning point of the quadratic function of time is slightly beyond the sample period, indicating that TFP growth fell to close to zero but never became negative.

minishing returns to capital accumulation under "extensive growth". This affirms our main point—the Soviet growth collapse was due in part to a rigid economy that did not allow much substitution of labor with capital. As labor scarcities developed, the extensive growth strategy was doomed.

In addition, Beare's comment leads to the further conclusion that the rate of technical progress declined over the course of the history of the former Soviet Union—a result that we regard as very plausible, and which we thank Beare for pointing out as also being implied by the data.

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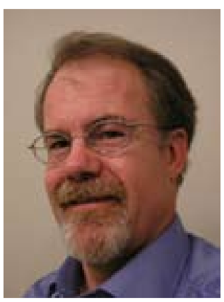
ABOUT THE AUTHORS



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Go to Beare's Comment
Go to May 2008 Table of Contents with links to articles
Go to Archive of **Comments** Section

Econ Journal Watch,
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Editor's Note: Prior to providing their reply to this rejoinder, Professors Grinols and Mustard requested assurance that Professor Walker would not be permitted to reply to it in this journal, and that assurance was given. Readers interested in discourse beyond *EJW* may wish to visit Professor Walker's website. Link.

The Diluted Economics of Casinos and Crime: A Rejoinder to Grinols and Mustard's Reply

Douglas M. Walker[1]

A Rejoinder To: Earl L. Grinols and David B. Mustard, "Correctly Critiquing Casino-crime Causality," *Econ Journal Watch* 5(1), January 2008: 21-31. Link.

Abstract

I appreciate that Professors Grinols and Mustard (2008) replied to my comment (Walker 2008). They say that, since I did not present empirical evidence for the "*potential* problems" I raised, they have no reason to alter their initial conclusions about the relationship between casinos and crime (30).

But if *A* estimates some effect, and *B* shows that the estimation is based on bad formulations and iffy data, it is rather irrelevant for *A* to respond: "But *B* hasn't shown that reality is something other than my estimates." My criticism wasn't based on the claim that I know what the crime effects of casinos are; it was based on the demonstration that Grinols and Mustard have not given credible grounds for claiming that *they* know what the crime effects are.

I wish to revisit the crime rate issue as it pertains to the cost of casino crime estimates. I will also discuss the public consumption of research results.

The Shift from Crime Rate to Cost Burden per Person

Consider Tunica County, Mississippi. The county population is around 10,000, and the county has 15 million visitors each year.[2] Suppose there were 1,000 Index I crimes reported during a particular year. If we include the visitors

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2 The 2005 estimated population was 10,321 (www.census.gov). The tourism estimate is from the Tunica Chamber of Commerce (www.tunicachamber.com).

in the denominator, then the crime rate is 0.000067; if not, it is 0.10.

Grinols and Mustard label the first the "diluted' crime rate and the second "undiluted." In the Tunica case, the "undiluted" crime rate is 1493 times greater than the "diluted" rate. To the extent that the Grinols and Mustard results depend on similar counties with a large ratio of tourists to residents, the exclusion of visitors from the population measure will have a real impact on the estimated crime rates. Clearly, one's estimated cost of crime will change massively depending on whether one uses 0.000067 or 0.10 as the crime rate. Yet, Grinols and Mustard argue that differences in crime rate measures are not the central issue because the rate used depends on "what the researcher wants to do" (Grinols and Mustard 2008, 23).

In my comment, I had interpreted Grinols and Mustard's purpose as estimating the change in the probability of casino county residents falling victim to crime with the introduction of casinos. My reason was that most of Grinols and Mustard's justification for using the "undiluted" crime rate focused on showing that the "diluted" rate can fall even if the probability that a resident falls victim to crime increases. In their 2006 *Review of Economics and Statistics* article they write:

> Should the number of crimes be divided by population—the conventional way to generate the crime rate (undiluted)—or by population *plus* visitors (diluted)?... Some have argued for one combination or another without realizing that the choice is not methodological, but depends on what questions the researcher wants to answer. A common but invalid claim is that the diluted crime rate should be used to determine the change in probability that a resident would be the victim of a crime. However, knowing what happens to the diluted crime rate does not give the needed information and could even move the answer in the wrong direction… (Grinols and Mustard 2006, 34)

Grinols and Mustard then give a lengthy, flawed example (discussed in Walker 2008, 8), and continue, "Thus, in this case the diluted crime rate falls while the probability of a resident being victimized rises" (Grinols and Mustard 2006, 35).

Grinols and Mustard (2008, 22) reply that my interpretation is wrong. They say they are *not* estimating the change in the probability of casino county residents falling victim to crime. They quote from their 2006 paper: "In this study we are interested in the costs to the host county associated with a change in crime from whatever source. We are therefore interested in the total effect of casinos on crime, and thus use the undiluted crime rate…."[3] And then immediately add: "In other words because crime perpetrated in a given geographical area can impose costs that fall on local taxpayers,

3 This quotation follows immediately after the quotation above.

it is appropriate to consider the total number of crime incidents relative to the local population and tax base" (Grinols and Mustard 2008, 22).

So they want us to focus on the costs of crime to casino hosting counties, rather than the risk to residents of falling victim to crime. Yet, the estimated cost of casino crime on casino hosting counties will be overstated if the crime rate attributed to county residents is overstated, as it is using their "undiluted" rate. If Grinols and Mustard want us to think in terms of cost burden per resident, they might elude the crime rate criticism, *but the basis for that criticism simply re-emerges in terms of cost burden per resident*.[4]

Costs of crime

Grinols and Mustard estimate the casino crime cost burden to be $75 per adult casino county resident per year.[5] This figure is emphasized in their abstract (2006, 28) and it is the basis of their brief policy discussion (41-42). But does this figure actually mean what Grinols and Mustard say it means? No. As I pointed out in my original comment, Grinols and Mustard have implicitly attributed the entire cost burden to the residents—even if they are not victims of crimes! That is why I said their story would only make sense if all crimes are committed against casino county residents, and none against visitors (Walker 2008, 9-10).[6]

Grinols and Mustard use estimates of the cost per crime from Miller, Cohen, and Wiersema (1996, 24) in order to estimate the cost of casino crime per casino county adult resident ($75). Yet, Miller et al. (1996) emphasize, "this study focuses on victims' costs" (9) and "deliberately excludes two of the largest costs associated with crime—the cost of operating the criminal justice system and the cost of actions taken to reduce the risk of becoming a crime victim" (17). In fact, most of the estimated costs of crime are intangible costs such as lost quality of life, fear, pain, and suffering (21), that are *borne by the crime victims*—not by the taxpayers in the counties in which the crimes are perpetrated.[7]

Miller et al. (1996, 24, Table 9) estimate the cost of each robbery, for example, at $13,000. Of that amount, $10,700 is intangible costs. Grinols and Mustard's failure to distinguish between resident and visitor victimizations effectively assumes that all costs—including intangible costs borne by casino county visitors

4 Rhetoricians might say that Grinols and Mustard's argument for using the "undiluted" rate is an *ipse-dixitism*.

5 They write, "We use cost per victimization figures…to calculate the total social cost of crimes committed in casino counties that are attributable to the presence of casinos…" (Grinols and Mustard 2006, 41). The abstract (28) indicates that these costs are on an annual basis. The details of the calculation are not provided by Grinols and Mustard.

6 Grinols and Mustard attribute the costs to *adult* county residents—71% of the average county's resident population (Grinols and Mustard 2006, 41).

7 Even the estimated tangible costs fall heavily on crime victims. Still, *some* of the estimated costs fall on society, as shown by Miller et al. (1996, 11).

—fall on the residents. Otherwise it makes no sense for the "per person" part of the calculation to include only the county residents.

An analogy might help to confirm our understanding of what is going on. Let's take an example that involves not costs per person, but benefits per person. The economic principle of mutual gains from voluntary interaction suggests that there are net benefits to those who voluntarily engage in sexual activity. What Grinols and Mustard have done is like saying that the sex benefits per person—that is, net benefits from sexual activity occurring within the county divided by the county population—is much higher in Carson City County, Nevada, than in practically all other counties in the country. But, in fact, the ordinary person who resides in Carson City County might in fact experience sexual benefits only about equal to or a little higher than ordinary residents in the other states. Benefits per person will appear artificially high if the numerous visitors to Carson City County are left out of the denominator. My suggesting that the residents should not open a casino because of the crime cost burden per person is $75 (as Grinols and Mustard estimate) would be analogous to suggesting that they should authorize brothels because their sex-benefit per person will go up significantly. But it is no more legitimate to encourage residents with the benefits of other people's sexual experiences than it is to discourage them with costs of other people's crime experiences.

It is certainly true that crimes impose some costs (policing, court, incarceration, etc.) on the county taxpayers, even when crimes are perpetrated against visitors. But Grinols and Mustard have not estimated that, since many of the costs that *are* borne by county taxpayers are explicitly ignored in the cost of crime estimates used by Grinols and Mustard (Miller et al. 1996, 17).

To summarize, most of the estimated cost of a crime against a visitor is not borne by residents—it is borne by that visitor. Furthermore, many of the costs that *are* borne by county residents and taxpayers are *ignored* in the cost of crime estimates used by Grinols and Mustard.[8] Their $75 estimate of the cost of casino crime per adult casino county resident is simply not meaningful.

Public consumption of gambling research

If Grinols and Mustard's estimates were ignored by the world, the dispute would be of merely scientific interest. But the results of Grinols and Mustard's crime study (2006) have certainly had an influence on the public discourse on gambling. Professor Grinols was recently quoted in *Parade Magazine* (Flynn 2007) as saying that "if the damage [from gambling] were spread evenly among all of us, there'd be no gambling. Grinols also co-authored an op-ed (Grinols and Rose

8 An additional problem is that, to the extent that casino-related crime is perpetrated on casino premises, the casino bears much of the costs of enforcement. Yet, Grinols and Mustard made no distinction between on-premises and off-premises crimes.

2007) based on his other social cost research.

Professors Grinols and Mustard are frequently quoted in newspapers, on anti-gambling websites, and in academic debate over the social costs of casino gambling.[9] As a result, they have had, and will likely to continue to have, a significant impact on the ongoing political debate in states like Kansas, Kentucky, and Massachusetts;[10] and in other countries where casino legalization is under consideration. It is therefore worthwhile to examine some of the other "cost of gambling" figures that Grinols, in particular, often repeats to the media, in order to illustrate just how arbitrary are much of the publicized data on the economic effects of casinos.

Grinols and Mustard (2001, 154)[11] and Grinols (2004, 171, 176) provide estimates of the social costs of pathological gambling. They estimate costs at over $10,000 per pathological gambler, per year,[12] and include a component for the costs of crime. Based in part on this cost estimate, the authors argue that casino gambling fails a cost-benefit test, with a ratio of 1.9:1 to 3:1, or even greater (Grinols and Mustard 2001, 155; Grinols 2004, 175-176).

Grinols has repeated these estimates in his op-ed on the effects of casinos (Grinols and Rose 2007), and has been quoted in the *Wall Street Journal* as saying that the introduction of a casino results in a net cost of over $97 per resident per year (Whitehouse 2007). In another newspaper article, Grinols was quoted, "I have concluded gambling as a whole is probably a bad idea for society" (Monahan 2007). Mustard was quoted in the *Washington Post* as arguing that "even using conservative estimates of costs and generous estimates of benefits, we still find the costs exceed the benefits" (Morin 2006). More importantly, the Grinols and Mustard analysis served as an important component of a just-released Canadian study which its authors hope will be the new "gold standard" for social cost of gambling research.[13]

Clearly, Grinols and Mustard have been strong advocates in the public discourse on casinos. Yet, the social cost estimate they provide is based on an average of cost estimates from other studies. Most of those studies arrive at their

9 See Walker (2008, 4, note 2).

10 I recently received an unsolicited email by someone requesting my help: "I am on the vanguard of keeping casinos out of Kentucky and have written several articles over the last few years. The legislature is scheduled to bring a bill to amend the constitution soon. I was doing some research and ran across your piece, Do Casinos Cause Economic Growth… I gather from the abstract that your answer was a resounding 'NO'. Is this what you concluded? I have had many discussions with Dr. John Kindt and also Dr. Earl Grinols, both of whom have helped me considerably in the past. I appreciate any 'ammunition' you can provide in this fight against this race to the bottom."

11 Incidentally, Grinols and Mustard were the guest-editors of the issue of *Managerial and Decision Economics* in which their 2001 paper was published.

12 The costs attributable to pathological gamblers are widely regarded as being the major component of the costs of legalized casinos. However, these estimates by Grinols and Mustard depend on numerous very questionable assumptions.

13 *The Socio-Economic Impact of Gambling Framework* (2008, 6). The findings by Grinols and Mustard (2001) are repeated on pp. 91, 95, and 99, and are promoted elsewhere in the report.

estimates in ways highly arbitrary. Here are a few examples:[14]

- In the study by Thompson, Gazel, and Rickman (1996, 19), the authors write, "The cost of probation and parole was estimated from the state budgets for corrections minus the costs of the operation of prisons, jails, and juvenile corrections. We assigned two-thirds of the residual budget to probation and parole costs, and divided the costs by the number of persons in these programs."

- Schwer, Thompson, and Nakamuro (2003, 15) explain how they calculated court costs: "[An earlier] study found that each federal court action costs $7,500. Considering that these actions may not be as complicated or long enduring as some others, we assign a 50 percent cost factor of $3,750 for each…case."[15]

- Focusing on money inflow and outflow to/from South Carolina as a representation of the benefits and costs of video gaming machines, Thompson and Quinn (1999, 10-12) explain, "There are 31,000 machines [in South Carolina]…They carry a [total] value of $46,500,000. The machines are for all intents and purposes manufactured out of state. We can assume that $46,500,000 leaves the state each year because of the machines." Summing over all components, they conclude, "The money leaving the state…equals $133.3 million compared to $122 million coming into the state. In direct transactions, the state's economy loses."

These are only three examples, but they are sufficient to show just how arbitrary such cost estimates are, both in methodological and empirical terms. Indeed, such studies have long been criticized for their poor quality, as discussed by the National Research Council (1999).[16]

CONCLUSION

I do not fault Professors Grinols and Mustard for participating actively in the public discourse. I admire their vitality.[17] But given the "*potential* problems" in

14 See Grinols and Mustard (2001, 153-154) and Grinols (2004, 172-174). Grinols and Mustard (2001, 152) acknowledge that only one of the eight studies on which they base their estimate was peer reviewed.

15 Schwer et al. (2003) is cited by Grinols (2004) but obviously not by Grinols and Mustard (2001). For a discussion of other problems in the Schwer et al. (2003) analysis, see Walker (2007b, 628-637).

16 Walker and Barnett (1999) discuss many of these studies in detail, and the social costs of gambling in general. Problems in quantifying social costs are also addressed in Walker (2007a, 2007b).

17 Perhaps they believe gambling is immoral. Grinols (1997, 8) has argued that "the Christian economist should differ from the non-Christian economist in what he does, how he does it, and why he does it." In a recent newspaper article (Monahan 2007), Grinols discussed the charge by another researcher that his

their crime study, I do not believe strong conclusions about the costs of casino-related crime are justified. For any policy issue, researchers should acknowledge potential problems, and policy conclusions should be tempered accordingly. I agree with Grinols and Mustard that I do not have a good estimate of the cost of casino crime. But neither do they.

Perhaps more importantly, this is an excellent example of *caveat emptor*. Given some of the examples of research discussed here, consumers of gambling research must be very careful to scrutinize the evidence—even when it is published in a reputable journal like *Review of Economics and Statistics*.

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"economic arguments against casinos are tainted by his evangelical Christian values." Citing prostitution as an example, Grinols was quoted: "What is morality, other than a balancing of economic costs and benefits?… Moral opposition is based on the fact that we have had [prostitution] long enough to know it causes more damage than benefits." Since Grinols and Mustard believe the costs of gambling exceed the benefits, perhaps they are morally opposed to it.

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Go to Walker's Original Article
Go to Grinols and Mustard's Reply
Go to Grinols and Mustard's Second Reply
Go to May 2008 Table of Contents with links to articles
Go to Archive of **Comments** Section

Econ Journal Watch,
Volume 5, Number 2,
May 2008, pp 156-162.

Connecting Casinos and Crime: More Corrections of Walker

Earl L. Grinols[1] and David B. Mustard[2]

A Reply To: Douglas M. Walker, "The Diluted Economics of Casinos and Crime: A Rejoinder to Grinols and Mustard," *Econ Journal Watch* 5(2), May 2008: 148-155. Link.

Abstract

Less than one month following the publication of our detailed response to Professor Walker's first commentary on our peer-reviewed work (Grinols and Mustard 2006), we were presented with a second manuscript by Mr. Walker (Walker 2008b). In his present commentary, Professor Walker again provides no new data or research, articulates comments that are already resolved through a careful reading of Grinols and Mustard (2006), and declines to respond to the failings that we raised about his earlier critique. In his second endeavor he also introduces new issues that were not in his first commentary note (Walker 2008a), expands his attention to the work of other authors, and makes factually incorrect statements about their work. Because Mr. Walker's complaints stem from errors of fact, correcting some of them might benefit future readers. We limit ourselves to four.

1. Mr. Walker says that our paper gives a "flawed example." Mr. Walker is incorrect. The model presented in the original paper is a system of three equations.

$$c = s_1 + s_2 + (\sigma_1 + \sigma_2)V/P$$

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$$D = (s_1 + s_2)\frac{P}{P+V} + (\sigma_1 + \sigma_2)\frac{V}{P+V}$$

$$\pi = s_1 + s_2$$

where c is the crime rate, D defines the diluted crime rate, P is the resident population, V is the visitor population, s_1 and σ_1 are the shares of the resident and visitor population, respectively, victimized by residents, and s_2 and σ_2 are the shares of the resident and visitor population, respectively, victimized by visitors. π is the probability that a resident will be victimized. The purpose of the model is to show that the diluted crime rate (number of crimes divided by local population + visitors) can fall while the probability of a resident being victimized rises (i.e. π and D can move in opposite directions.) For example, assume as in the paper that $\sigma_1 = 0$. Let $s_1 = 0.10$. In the no visitor situation $V = s_2 = \sigma_2 = 0$ and $\pi = D = 0.10$. Now let visitors be present, $V = P = 1000$, where $s_2 = \sigma_2 = 0.04$. As we originally showed, then π rises (from 0.10 to 0.14) while D falls (from 0.10 to 0.04). There is an infinite number of other ways the same result may occur.

2. Mr. Walker also expands his criticisms beyond Grinols and Mustard (2006) to papers written by other authors (Thompson, Gazel, Rickman 1996b; Thompson and Quinn 1999; and Schwer, Thompson, and Nakamuro, 2003). This is consistent with his pattern of writing "rebuttals" in which he provides no original research or no new empirical work.[3] In his present commentary, Mr. Walker also misinterprets or misunderstands the work of other researchers. He writes:

> These are only three examples, but they are sufficient to show just how arbitrary such cost estimates are, both in methodological and empirical terms. Indeed, such studies have long been criticized for their poor quality, as discussed by the National Research Council

3 In 1998 he produced a rebuttal of Gross (1998) that provided no original research or empirical work. A few years later he wrote a rebuttal of Kindt (2001), which criticized the gambling industry and originally appeared as part of a symposium of 10 papers dealing with gambling in America. Walker was not a participant in the original symposium and none of the papers in the symposium cited any papers by Mr. Walker. His paper contained no new results or additional empirical work of his own. The same year the Las Vegas Sun reported, "Earlier this month, the Nevada Resort Association—the chief lobbying group for Nevada casinos—commissioned a rebuttal report by Georgia College & State University Assistant Professor of Economics Douglas Walker, who said the results of Thompson's study were 'unreliable because their analysis is seriously flawed' " (Benston 2003). The referenced study is Schwer, Thompson, and Nakamuro, 2003 (later Thompson and Schwer 2003). In 2005 the Casino Association of Indiana hired Mr. Walker to write a rebuttal of the study by Policy Analytics 2006. Again, no original research or empirical work was conducted. In 2006 Mr. Walker was hired by the Taiwan Amazing Technology Co. Ltd, a manufacturer of gambling machines. A paper could not be found on the web to know if it contains original research or is promotional in nature. The following year in 2007 the American Gaming Association commissioned Mr. Walker to write a rebuttal of casino cost-benefit studies. No original empirical research was involved. Two months later Mr. Walker wrote his commentary on Grinols and Mustard (2006).

(1999). (Walker 2008b, 153)

Two of the three studies Mr. Walker cites are, in fact, not cited by the National Research Council (NRC) report. One of these papers could not possibly have been cited in NRC (1999) because it was written four years after the report was published. Furthermore, Thompson, Gazel, and Rickman (1996b) was cited *favorably*, not critically, by the NRC.[4] The NRC refers to the study as "an excellent example" and a study that "makes a significant contribution to the literature on the economic impacts of gambling."

3. In the same section, Walker cites Grinols (2004) in his footnote 14, inserted after the sentences "Most of these studies arrive at their estimations in ways highly arbitrary. Here are a few examples." As shown above, the basis for much of Mr. Walker's perspective is misreading of the original papers. Walker (2007), written for the American Gaming Association, the chief lobbying body of the gambling industry, discusses the need for gambling research to adjust for co-morbidity (multi-causality). He writes that "a mechanism is needed to allocate the harm among coexisting disorders, yet most authors make no such attempt." He states: "Most social cost researchers (e.g., Grinols 2004, Grinols and Mustard 2001, and Thompson et al. 1997) simply attribute all of the costs to gambling." This statement, too, is false. Page 173 of Grinols 2004 states that the reported cost figures were adjusted by the author to correct for multi-causality, as well as for a second issue, sample selection bias (representativeness of sample). Both are explained in detail on pp. 170-71.

4. Mr. Walker devotes nearly four manuscript pages to his view of how

4 Thompson, Gazel, Rickman 1996b is cited four times by NRC. They are singled out positively on page 173:

> An excellent example of this type of analysis is a study that looked at the economic effects that casinos have had in Illinois and Wisconsin (Thompson et al., 1996b)….The result was a set of estimates of the positive and negative monetary effects of casino gambling in both Illinois and Wisconsin. This, in turn, provided a good estimate of the positive effects of casinos in the two states….

The three other citations are simple references to earlier results as on page 28:

> Studies primarily of gamblers seeking help suggest that as many as 20 percent will attempt suicide (Moran, 1969; Livingston, 1974; Custer and Custer, 1978; McCormick et al., 1984; Lesieur and Blume, 1991; Thompson et al., 1996), and two out of three help seekers have turned to criminal activities to support their gambling (Lesieur et al., 1986; Brown, 1987; Lesieur, 1989).

NRC also reports favorably on Thompson, Gazel, Rickman 1996a on page 181:

> A second study that makes a significant contribution to the literature on the economic impacts of gambling is one that identifies and quantifies the social costs of gambling in the state of Wisconsin (Thompson et al., 1996a).

NRC recognizes that the research they cite can be improved—for example Thompson, Gazel, Rickman do not consider the full range of social costs—but the sentence that points this out is positive, reading, "The most sophisticated gross impact studies painstakingly attempt to measure the net positive economic effects of casino gambling without considering the full range of costs." Two sentences later appears the "excellent example" statement quoted above.

crime statistics should be measured. As stated in our previous response, we addressed this issue extensively in the original paper and encourage readers to return to our original paper. In Mr. Walker's initial commentary, he objected to our using conventional, federally-reported crime rates in our study. His position has now evolved. In his earlier commentary he said,

> the apparent objective of the Grinols and Mustard paper is to analyze the risk of casino county residents falling victim to crime. (Walker 2008a, 7)

and

> In this case, clearly the 'diluted' crime rate is the appropriate one to use if we are trying to measure the risk to residents and/or visitors of being victimized. (Walker 2008a, 10)

His previous concern was based on an incorrect reading of the paper. Our paper states, "We are therefore interested in the *total effect of casinos on crime*" (Grinols and Mustard, 30, emphasis added) as "Correctly Critiquing Casino-Crime Causality" further confirmed.

His original comment resolved, Mr. Walker now shifts his focus to crime costs. In his new comment he says, "they might elude the crime rate criticism, *but the basis for that criticism simply re-emerges in terms of cost burden per resident*." He says,

> Grinols and Mustard have implicitly attributed the entire cost burden to residents—even if they are not victims of crimes! (Walker 2008b, 150)

This also is false. We have not "implicitly attributed the entire cost burden to residents" as Mr. Walker erroneously claims. In fact we explicitly said the opposite. In the section titled "Visitor Criminality," we said that crime could rise "because casinos attract visitors who are more prone *to commit and be victims of crime*" (Grinols and Mustard 2006, 32, emphasis added). Earlier in the paper we provided theoretical explanations of how casinos might affect crime. We said, "These factors are not mutually exclusive, and our empirical results estimate *the total effect*" (emphasis added). That crime might increase in casino counties because visitors were crime victims was repeated again on page 40 in our section labeled "Evaluation" where we wrote, "The regressions in table 4, of course, cannot decompose the net number of offenses to assign them to each alternative explanation."

While Mr. Walker appears to want to ignore crimes committed against visitors, we believe that crimes committed against visitors are part of total costs. We explicitly stated in the original paper and reiterated in our first response, that in calculating the estimated costs we deliberately chose results that provided smaller estimates of the casino effects than did some of our other specifications.

Concerning the denominator, the social costs associated with increased crime involves the change in absolute number of crime incidents. The divisor used in the initial reporting of crime (whether population or population + visitors) disappears before the final step, hence is irrelevant. In the portion of the paper detailing costs, the paper reports:

> In 1996 the total costs for the 178 casino counties exceeded $1.24 billion per year. (Grinols and Mustard 2006, 41)

What would the costs be if they were extrapolated to the U.S.? The next sentence reads:

> If the estimated coefficients from table 4 are applied to a representative county of 100,000 population, 71.3% of which are adults (as is representative of the United States as a whole), then the social costs per adult are $75 in 2003 dollars.

The answer is $75 per adult, as reported.

In conclusion, many papers, Grinols and Mustard 2006 included, describe areas where further research could improve on earlier contributions. Indeed, this is the theme of much, if not most, academic research in the social sciences. When we saw the need for better research on the statistical link between casinos and crime we researched, wrote, and published Grinols and Mustard (2006). Unless better research comes along to confirm or deny our peer-reviewed results, we stand by our conclusions.

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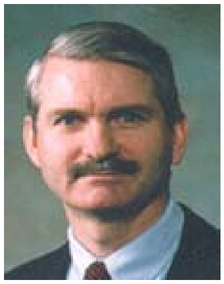

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Go to Walker's Original Article

Go to Grinols and Mustard's Reply

Go to Walker's Rejoinder

Go to May 2008 Table of Contents with links to articles

Go to Archive of **Comments** Section

Econ Journal Watch,
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Smoking in Restaurants: Rejoinder to Alamar and Glantz

David R. Henderson[1]

A Rejoinder to: Benjamin Alamar and Stanton Glantz, "Smoking in Restaurants: A Reply to Henderson," *Econ Journal Watch* 4(3), September 2007: 292-295. Link.

Abstract

In their last paragraph, Alamar and Glantz (2007) write, "Hen-derson (2007) does not accurately identify any problems either theoretically or statistically with our analysis." This is an amazing conclusion, given that I did identify theoretical and statistical problems with their analysis. I will respond to the specifics, but I invite the reader to read my article and their reply together. Alamar and Glantz (2007) have largely chosen to ignore my criticism. I close with a challenge to Alamar and Glantz.

Economic Theory

A quick recounting of our theoretical differences, up to but not including their response, is in order. Alamar and Glantz (2004) assert that smoking in restaurants imposes externalities. They argue that the large number of customers "with greatly varied preferences" with regard to smoking causes negotiation costs to be high. This, they argue, "violates the assumption of low costs in the Coase theorem" and, they conclude, makes smoking an externality that is not internalized.

My criticism (Henderson 2007) is that restaurant owners do not have to negotiate with customers. All they need do, whether the issue is smoking, dress-code policy, music, or menu choices, is make their decision and see how successful

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I thank Warren Gibson, Rena Henderson, and Michael Pakko for helpful comments.

they are. Customers who want to eat in a non-smoking restaurant can do so; customers who want to eat in a smoking restaurant can do so; customers who want to eat in a restaurant that allows smoking in a designated area can do so. Customers show their values of these various options by the prices they are willing to pay and by the frequency of their patronage. Restaurant owners have an unbiased incentive to trade off the values put on smoking by various potential patrons and, therefore, do not reach a biased result in favor of allowing smoking. Introducing the desires of restaurant employees complicates the analysis without changing the bottom line: restaurant owners have an incentive, via wages paid and workmen's compensation, to take account of the desires of the employees also.

In their reply, Alamar and Glantz write:

> It is not possible for a restaurant owner to internalize the cost of second-hand smoke on the health of the staff or patrons. There is no mechanism by which a restaurant owner can compensate a patron for any health costs related to second-hand smoke, therefore it is not possible for the owner to have completely internalized the costs of the externality imposed by the smoker. (292)

Alamar and Glantz have completely missed my point. My argument is that not only is it possible for restaurant owners to internalize the cost, but also that that is what they do. There is no need for a "mechanism" to compensate a patron. Instead, the patron decides on the negative value he or she puts on a restaurant that has smoke and that negative value is reflected in what he or she is willing to pay for the restaurant experience. In that way, the putative externality is internalized. That is why I said that they beg the question: they start with the assumption that smoking in a restaurant imposes externalities rather than establishing that it does. It is not surprising that if one *assumes* that there is an externality, one will be driven to the conclusion that there *is* an externality. Interestingly, Alamar and Glantz (2007) avoided responding to my analogy between smoking policy and T-shirt policy. Yet all their reasoning, if correct, can be applied to T-shirt policy.

In deciding whether to buy, consumers come to the experience. If, following Alamar and Glantz's logic, smoke in restaurants is to be called an externality, then people who don't like some dimensions of experiences they came to would also suffer an externality. Customers at sports clubs who don't like the boisterous cheering would be suffering an externality. Patrons at the movie theater who don't like having to sit up so as to peer above the people seated in front of them would suffer an externality. People at Disney World who don't like having to wait in lines would suffer an externality. Alamar's and Glantz's definition of "externality" is too broad. It ignores the most basic characteristic of the idea: effects on parties who are *external* to the decisions that make for those effects. If you decide to go a restaurant, sit down, and buy a meal, you are not external to the ordinary and expected features of experience you find in the restaurant.

Finally, Alamar and Glantz (2007) state, "Henderson (2007) claims that we do not put enough faith in these entrepreneurs' views," namely the view that if consumers value it highly enough, some entrepreneurs will gain from a smoke-free environment. But I said nothing about faith. Rather, I believe that entrepreneurs often experiment and that some entrepreneurial restaurant owners will experiment with a ban on smoking. Then if banning smoking is as good for the bottom line as Alamar and Glantz claim, they will stick with the ban. When other restaurant owners observe the results, then they too will be more inclined to ban smoking—if, that is, Alamar and Glantz are right about the profitability of instituting bans on smoking. This is the standard story about what happens in competitive markets. Where is the "faith" in this story? Indeed, it is Alamar and Glantz (2007) who have faith, in two ways. First, they assert, "When these entrepreneurs only have the biased information given to them from the tobacco industry (without being told that it is coming from the tobacco industry (Alamar and Glantz 2004)) how are they to know that the information is biased?" Um, maybe by getting other information? Alamar and Glantz have complete faith that the tobacco industry has been such a powerful persuader that they have made all restaurant owners completely uncurious. This view that the restaurant owners have considered no other information is a strange view and one for which they give zero evidence.

There's another group in which Alamar and Glantz seem to have faith: voters. Basic public choice analysis has explored how enlightenment depends on incentives to overcome the costs and biases of ignorance, and on the complexity of the issues considered. Alamar and Glantz turn these teachings on their head. They show remarkably little confidence in the self-regarding wisdom of restaurant owners who have a huge amount at stake, and yet they have great confidence in the wisdom of voters, most of whom individually have little at stake. The externality from smoking, Alamar and Glantz argue, "is one reason that the public has demanded laws to make restaurants smokefree."

Yet, compared to restaurant owners, voters have much less incentive to become more enlightened in the matter, and the political issue they face as voters is vastly more complex than the decision a restaurant owner faces concerning his own particular business and his own customers. Somehow Alamar and Glantz overlook such elementary analysis.

There are better explanations for the fact that voters sometimes support smoking bans. One is that a non-smoking majority simply doesn't care about the effect that smoking bans have on restaurant owners and diners who would like to smoke. Another is that the typical voter who supports bans foolishly thinks that banning smoking is morally and socially right, and he doesn't overcome that foolishness, because, even if only implicitly, he knows his vote will not affect outcomes.

The Empirical Evidence

In Henderson (2007), I made three main criticisms of the empirical evidence in Alamar and Glantz (2004). Alamar and Glantz (2007) respond to only one of the criticisms. Again, I shall recount the debate quickly.

The statistical evidence in Alamar and Glantz (2004) was that in a cross-sectional study of restaurants, those restaurants in areas that banned smoking had a higher Price to Sales ratio (P/S) than those restaurants in areas that did not ban smoking. P here is the price at which a restaurant is sold and S is the annual sales revenue of the restaurant. From this finding, and this finding alone, Alamar and Glantz (2004) concluded that bans on smoking make restaurants more profitable.

My criticisms were three and I shall consider them in turn. First, the P/S ratio tells us nothing about the magnitude of P. I pointed out that if P/S is higher in areas with bans on smoking, this could be because P actually fell but S fell even more. This is the criticism to which Alamar and Glantz (2007) respond. They admit the mathematical point. But they cite literature showing that in fact S (sales) did not decline after a smoking ban had passed. If that were all there is to say on the issue, Alamar and Glantz (2007) would have made a good argument.

But that is not all there is to it. In citing the literature on sales, Alamar and Glantz (2007) say something interesting and revealing. They write that the literature cited included "all data points both pre and post implementation of a smokefree law." Their statement is consistent with one of my other criticisms of their original article. I had pointed out that in the areas where the ban was implemented, the restrictions may have wiped out some restaurants, making the remaining restaurants more profitable. Studies that consider the data on restaurants before and after a ban will miss the negative effect on restaurants that are eliminated; one cannot study what does not exist. Alamar and Glantz 2007 completely ignore the point, even though I made the point in the section titled, "The Forgotten Restaurants."

My third criticism was that to know the effect of a non-smoking ordinance, one would want to study the data in an area before and after the ordinance. Instead, as noted above, Alamar and Glantz (2004) do a cross-sectional study at a point in time. Not only do Alamar and Glantz (2007) not respond to this criticism, but also they do not even learn from it, as they misstate their own findings. They write, "We found a positive effect on this ratio when smokefree laws were implemented." But they did not find that at all, as they admit in Alamar and Glantz (2004). They did not look at what happened "when smokefree laws were implemented." That would have been the before-and-after study I called for. Instead, as noted, they looked at cross-sectional data.

Finally, there is another empirical problem with the results in Alamar and Glantz (2004) that I failed to point out in Henderson (2007). Dunham and Marlow (2000) point out that smoking bans on restaurants are more likely to be legislated in cities where a large percentage of the population already has a strong desire for such bans. This desire for bans, no doubt, reflects their own taste for restaurants that do not allow smoking. This means that any adverse effect of a smoking ban on restaurant revenues in such communities will tend to be low. But one cannot generalize from that fact to a conclusion about the effect of smoking bans in general.

Alamar and Glantz may attack Dunham and Marlow (2000) on the grounds that it was funded by a tobacco company. In their earlier paper, Alamar and Glantz put a great deal of weight on the funding source of various articles in lieu of judging the content of those articles. Surely funding by a tobacco company should make one approach such a study with more skepticism than otherwise. But the funding source alone is not enough to justify disputing its findings without giving reasons. Glantz has received much of his funding from the anti-smoking lobby and, indeed, from taxes on tobacco. That should make one skeptical of his findings. But the skepticism is not enough to warrant rejecting his findings. Notice that in no part of Henderson (2007) and in no part of this rejoinder do I dispute anything Alamar and Glantz write based on the source of their funds. My argument is solely based on logic and evidence, as it should be. To reject any finding on the basis of its funding is to attack the character of the researcher. As any elementary logic text will point out, such ad hominem attacks are illegitimate.[2]

A CHALLENGE

As noted above, a major part of the argument in Alamar and Glantz (2004), which they repeat in Alamar and Glantz (2007), is that restaurant owners did not have access to information about the effects of a smoking ban other than the information that tobacco companies provided. Again, they gave no evidence for that claim. Let's assume for a minute, though, that Alamar and Glantz are correct that the restaurant owners had no other information. But now they do. The California non-smoking law in restaurants has been in effect since January 1, 1995. That has given us 13 years of experience. So their key argument about bad information, if it ever applied, surely does not apply now. There is no good case, therefore, even from their viewpoint, for imposing a ban in California today. If they are right, then ending the ban will cause no restaurants to start allowing smoking. If I am right, at least some will. I call on Alamar and Glantz to put their policy prescriptions where their theory is and call for ending the ban. And if they refuse to do so, it is fair for the rest of us to ask them, "Do you really believe your own imperfect information story or are you being the big bully on the block who believes that might makes right and doesn't care about the minority?"

2 See, for example, Joseph Gerald Brennan, *A Handbook of Logic*, New York: Harper & Row, 1961, p. 217.

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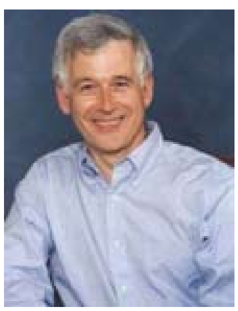
About the Author



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Go to Henderson's Original Article / Alamar and Glantz's Reply

Go to Alamar and Glantz's 2nd Reply

Go to May 2008 Table of Contents with links to articles

Go to Archive of **Comments** Section

Econ Journal Watch,
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Externalities in the Workplace: A Response to a Rejoinder to a Response to a Response to a Paper

BENJAMIN C. ALAMAR[1] AND STANTON A. GLANTZ[2]

A REPLY TO: DAVID HENDERSON, "SMOKING IN RESTAURANTS: REJOINDER TO ALAMAR AND GLANTZ," *ECON JOURNAL WATCH* 5(2), MAY 2008: 163-168. LINK.

ABSTRACT

PROFESSOR HENDERSON HAS SIMPLY REPEATED THE SAME TWO POINTS HE made in his earlier critique (Henderson 2007) of our article "Smoke-free Ordinances Increase Restaurant Profit and Value" (Alamar and Glantz 2004). He argues 1.) that secondhand smoke is not an externality, therefore no government intervention is required to protect workers and customers in restaurants and bars, and 2.) the empirical results in the paper are not conclusive because the data are cross-sectional. Henderson also issues a challenge for us to advocate for the repeal of the California law on smoke-free restaurants. While we enjoy a good debate and do not mind adding another publication to our CVs, we do hope that Prof. Henderson will not find the burning desire to restate his position again, after we respond this last time.

AN EXTERNALITY EXAMPLE

It is often useful, as Prof. Henderson has done repeatedly, to use abstract examples that bear some resemblance to the theoretical point being made to insure that the point is fully understood. Because Prof. Henderson has not understood or chosen to ignore one of the central goals of smoke-free laws—protecting people from adverse health effects of secondhand smoke—we will use an example now to fully illustrate it.

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We would imagine that, while we all desire higher salaries, Prof. Henderson believes he is more or less adequately compensated by the Naval Postgraduate School. Any lack of salary is likely made up for in a nonpecuniary way by the lovely location of Monterey Bay, CA. His view might change, however, if asbestos was found falling from the ceiling in his office. Suddenly, he is made aware of a health risk that he has been bearing for years, yet has not been compensated for in any way. Now, since the School did not know about the asbestos either, imagine that Prof. Henderson goes to the administration, makes them aware of the situation and kindly asks them to remove the asbestos. Unfortunately for Prof. Henderson, the administration is, as academic institutions tend to be, cost conscious and responds that they simply cannot afford to remove the asbestos.

Since Prof. Henderson is a free individual, he is free to seek employment elsewhere, so he begins to look for a new position. Imagine now though, that there is asbestos in virtually every academic office in every institution of higher learning in the United States. But in Prof. Henderson's view of the world, once this becomes known, there would be a series of institutions experimenting with asbestos-free workplaces, so, since he prefers an asbestos-free workplace, he should have no trouble finding an institution that was willing to experiment in this way and bear a cost that they believe to be large.

It is possible that this imaginary world could work this way, but we do not share the same confidence in the willingness of academic administrators to be on the cutting edge. Instead, we see the asbestos as a clear externality that has been imposed on Prof. Henderson without his knowledge and without any sort of compensation. Moreover, his ability to change that situation is severely limited.

Likewise, workers in restaurants and bars are not compensated for bearing the risk associated with high and repeated exposure to secondhand smoke. Just as the government has taken action in numerous cases to protect workers from unnecessary and extreme risks, we see laws that end smoking from bars and restaurants as laws that remove an externality from the workplace.[3]

Empirical Critique

Henderson concedes the point that we made in our original response (Alamar and Glantz 2007a) regarding the Price to Sales (P/S) valuation ratio that our study (Alamar and Glantz 2004) utilized to assess the effect of smoke-free restau-

3 Incidentally, while we disagree with Prof. Henderson's view of the world (and wish him luck in finding a safer working environment) if he is correct, his argument provides an alternative explanation for why restaurants actually make more money when they are smoke-free. If Prof. Henderson is correct, then restaurant workers are currently being compensated for the added risks associated with long term exposure to secondhand smoke. Once the restaurant is smoke-free, the wages for the workers would decline, thus increasing the profit margin of the restaurant.

rant laws on restaurant profits. He correctly stated that a decrease in S could have led to an increase in the ratio. We responded that extensive research has already shown that S does not decline, and therefore the change must come in the form of an increase in P. He agreed with this assessment.

He has, however, repeated his "forgotten restaurant" critique as well as his suggestion that cross sectional data are not appropriate for this study.

Henderson's "forgotten restaurant" critique suggests that some restaurants may be harmed from the smoke-free law and therefore, we should not impose such a law. Henderson is apparently trying to restrict governmental action to action that is Pareto improving, that is, a law should only go into place if it benefits everyone. Clearly this is not the standard at which the United States has set government action.

Additionally, this point is not ignored by our original work as Henderson argues, but rather studied carefully. We performed a Monte Carlo simulation with our estimated results to examine the distribution of the smoke-free premium (the increase in value of the restaurant when, all else being equal, it is in a jurisdiction with a smoke-free law). The simulation estimated that 0.3% of restaurants would lose value. So, while a smoke-free law does not uniformly increase the value of restaurants (or get to the 0% harmed that Henderson suggests), it comes exceptionally close to meeting his extreme standard.

As for Henderson's critique of our use of cross-sectional data, we do not disagree that every study has its limitations. On any issue that is of real importance, multiple studies, using varied data sources and different approaches should be required. Happily on the issue of smoke-free restaurants there are multiple studies using various data sets and techniques which all converge to the same result that smoke-free laws either improve or do not effect a restaurant's economic situation (Scollo et al, 2003). The same holds for bars (Alamar and Glantz 2007b).

Additionally, since our data set includes transactions in California both before and after the smoke-free law came into effect, we are able to calculate a pre-law and post-law average P/S ratio. Our sample contained 45 restaurants in California that were sold pre-law and 98 restaurants in California that were sold post law. The average pre-law P/S ratio was 0.353 with a 95% confidence interval of $\pm$0.007. The average post-law P/S ratio was 0.373 with a 95% confidence interval of $\pm$0.003. These results demonstrate an increase of 5.6% in the P/S ratio. Thus, if restaurant owners were bailing out of the business because of losses, as Henderson suggests, they were somehow getting a higher price for their exit from the industry than they would have if they sold their restaurant before the law was in place.

THE CHALLENGE

Prof. Henderson suggests that as restaurant owners now can see the economic benefit of being smoke-free, thus if we repealed the California smoke-free restaurant law, we should see very few restaurants allowing smoking. While we do believe that this would be the case (as this was not designed to be a Pareto improving law, there would be some switch back), we are not ready to gamble with the health of restaurant workers to resolve an academic debate.

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Professor Stanton A. Glantz has been a leading researcher and activist in the nonsmokers' rights movement since 1978, when he helped lead a state initiative campaign to enact a nonsmokers' rights law by popular vote (defeated by the tobacco industry). In 1983, he helped the successful defense of the San Francisco Workplace Smoking Ordinance against a tobacco industry attempt to repeal it by referendum. He is one of the founders of Americans for

Nonsmokers' Rights. In 1982, he resurrected the film "Death in the West," suppressed by Philip Morris, and developed an accompanying curriculum that has been used by an estimated 1,000,000 students. He helped write and produce the films "Secondhand Smoke" and "120,000 Lives." Dr. Glantz conducts research on a wide range of issues ranging from the effects of secondhand smoke through the reductions in heart attacks observed when smokefree policies are enacted, to how the tobacco industry fights tobacco control programs. His work in this area was identified as one of the "top research advances for 2005" by the American Heart Association. He has written several books, including the widely used *Primer of Biostatistics*, and *Primer of Applied Regression and Analysis of Variance*. He is author of more than 200 scientific papers. His book *The Cigarette Papers* played a key role in the ongoing litigation surrounding the tobacco industry. His book *Tobacco Wars: Inside the California Battles* chronicles the battles against the tobacco industry in California. He also wrote *Tobacco: Biology and Politics* for high school students and *The Uninvited Guest*, a story about secondhand smoke, for second graders. He is now running two educational projects, SmokeFreeMovies.ucsf.edu, which is working to end use of movies to promote tobacco, and TobaccoScam.ucsf.edu, which is countering tobacco industry efforts to co-opt the hospitality industry. Working with the UCSF Library, he has taken the lead in making nearly 50 million pages of previously secret tobacco industry documents available to the entire world via the internet. He served for 10 years as an Associate Editor of the *Journal of the American College of Cardiology* and is a member of the California State Scientific Review Panel on Toxic Air Contaminants. He was elected to the Institute of Medicine in 2005. He is a Professor of Medicine (Cardiology) and American Legacy Foundation Distinguished Professor of Tobacco Control as well as Director of the Center for Tobacco Control Research and Education at University of California, San Francisco. His email is glantz@medicine.ucsf.edu.

Go to Henderson's Original Article

Go to Alamar and Glantz' Reply

Go to Henderson's Rejoinder

Go to May 2008 Table of Contents with links to articles

Go to Archive of **Comments** Section

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Symposium: Gender and Economics

Reaching the Top? On Gender Balance in the Economics Profession

Christina Jonung[1] and Ann-Charlotte Ståhlberg[2]

Abstract

In 1911 the Higher University Council at the University of Lund debated a committee proposal to admit women to professorial positions. Knut Wicksell, then professor of economics at Lund and an ardent advocate of women's rights, made the following characteristic comment:

> On the whole, all masculine reasoning about what women are or are not capable of accomplishing is probably quite superfluous and moreover, in particular as concerns their exclusion from higher office, is to no mean degree reminiscent of the custom of some primitive tribes to render certain especially savory morsels of food "taboo" for womankind.[3]

Today, almost one hundred years since Wicksell's eloquent plea, it is high time to assess the situation with respect to women's access to those delicacies—particularly the positions at the top of the academic career ladder. The 1911 proposal failed. Not until 1925 were women entitled to hold public offices in Sweden, including professorships. The first female professor to hold a regular chair was appointed in medicine in 1937 at the Karolinska Institutet in Stockholm. At Uppsala University the first women professor, in geography, was inaugurated in 1949, while Lund University waited until 1965 to appoint its first woman, in history.

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3 Knut Wicksell in 1911, as quoted in Petrini (1934, 20). Our translation.

This article addresses the under-representation of women in academic economics against the background of the situation in Sweden. We examine the participation, opportunities and success of women in economics at Swedish universities (with details relegated to appendices). We compare the situation to Australia, Canada, Great Britain, and the United States. In all, women are under-represented in economics. We discuss various explanations.

In many other countries scholarly attention has been given to the status of women in the economics profession. Economic associations have taken explicit measures with the aim of promoting the careers of female economists. In the US, the American Economic Association in 1972 inaugurated CSWEP, the Committee for the Status of Women in the Economics Profession. Ever since, the committee has closely monitored the situation of women and acted to improve it. In Great Britain, a group within the Royal Economic Society has been working on similar tasks since 1996. In Canada, women economists have had a network of their own since 1990. More recently, in 2002, the Economic Society of Australia established the Committee for Women in Economics.[4]

These committees all pursue a variety of activities: building female networks, helping women early in their career to be included in conference programs, publishing newsletters with professional advice, opportunities, and profiles of senior successful women economists, supporting research on women/gender issues, and organizing mentoring workshops. Above all, they regularly gather information and chart women's progress and identify barriers to advancement.

No similar organization exists in Sweden.[5] Neither has the representation of women in economics been the subject of any systematic studies. In 2003 we published an article in Swedish in the journal of the Swedish Economic Association, *Ekonomisk Debatt*, with the aim of putting the status of women in economics on the agenda in Sweden and drawing comparisons to other countries.[6] Our interest in the issue of the presence of women in academic economics is based not only on the opinion that women should be able to partake in the gourmet meal. We also believe that, if more economists are women, economic analysis will be richer, and if more women are familiar with economic reasoning, public debate will be stronger and deeper.

WOMEN IN ECONOMICS IN SWEDEN

Sweden introduced a modern doctoral program in 1969.[7] Before that, only a

4 See the following webpages for more information about the purpose and activities of the different committees: www.cswep.org, www.res.org.uk/society/women.asp, www.yorku.ca/cwen, www.ecosoc.org.au/women. See also the Explorations section of *Feminist Economics* No 3 (Jacobsen 2006) and Bartlett et al. (1998).

5 Nor, so far as we know, has the European Economic Association taken any such initiative.

6 The present article is largely an English, updated adaptation of the *Ekonomisk Debatt* article (Jonung & Ståhlberg 2003).

7 See Wadensjö (1992) for a description of the changes from the old to the new PhD-system.

handful of women obtained a higher university degree in economics. Thus women were absent from university faculties as well.

In short, our results for Sweden are the following:[8]

- In undergraduate economics programs, the proportion of women among first semester students is almost 50 percent—hence, near parity at the "starting line." But there is a large drop between the first and second semester; a step that usually involves a decision between economics or business administration.
- Women constitute roughly one third of the students beyond second semester in undergraduate economics programs.
- The number of women completing a Master's degree in economics has been steadily rising, but women's fraction of the total number has varied due to variations in the number of men.
- Between 1970 and 2005, women's proportion of doctoral degrees increased from zero to 26 percent. A total of 135 women and 624 men were awarded a doctoral degree between 1970 and 2005. Thus, women constituted 18 percent over the period.
- There were 39 women with a completed PhD working as academic staff in economics departments at Swedish universities in 2006, making 16 percent of the corresponding total.
- That 16 percent is slightly lower than women's 18-percent share of the total number of economics PhDs in the country.
- Women's representation of assistant professors is 19 percent, thus lower than their recent PhD graduation record.
- Docent is a title based on system-wide review and awarded to mature scholars, but it is not necessarily connected to employment. Women advance to docent at a rate somewhat lower than men. The proportion of women among docents was 22 percent in 2006, but women make 13 percent of the total number with "docent qualifications", i.e. with a rank of associate professor or higher.
- The first female professor of economics was appointed in 1993. In 2006, five women, or six percent, were full professors in economics, i.e. those with the Swedish title "professor."
- Among the male academic economics staff, full professors make 36 percent; among the women, full professors constitute 13 percent.
- In 2006, women represented 16 percent of the members in The Swedish Economics Association. The association features invited speakers and commentators and women's share was 10 percent in the 2000s, but below that level in previous decades.
- The Swedish Economics Association publishes a public-discourse journal

8 The data is found in Appendices 1 and 2.

Ekonomisk Debatt. Female representation in authorship rose from around 2 percent in the 1970s to around 7 percent in the 1980s and up to around 10 to 15 percent in the 1990s and 2000s.

A LEAKY PIPELINE?

The situation for women in academic economics has been described as a "leaky pipe-line." In Sweden, the most significant "leak" is after the very first semester—as noted, a large portion of women exit then. From the second semester, we find rates of continuation to higher levels, through graduate education, only slightly lower for women than men.

Until the 1970s only two women had graduated with a PhD in economics in Sweden. The first, in 1924, was Margit Cassel, daughter of the famous economist Gustav Cassel. The second, Karin Kock, completed her degree in 1929. She became Sweden's first female cabinet minister and was given the title of professor in economics in 1945, albeit without a chair. In addition, three other women completed the licentiate-degree in economics, a somewhat lower degree, but at the time more or less equivalent to the PhD of today. Out of these five, Karin Kock was the only one who worked as an academic teacher and researcher.[9]

The 1970s saw the influx to the Swedish universities of the large birth-cohorts of the 1940s, as well as the establishing of the new US-style doctoral program. Since then, significant changes have taken place. Women's share of PhD completions rose from 7 to 9 percent during the 1970s and 1980s, up to around 17 to 18 percent in the 1990s, reaching 26 percent in the beginning of the 2000s. The number of women graduating during the last decade is almost triple that during the two and a half decades before combined.

The data in appendix A indicate that male and female rates of completing graduate studies once you have entered graduate school are about equal. If there is no further leakage at this level we can expect that in the coming years the proportion of women among PhD graduates in economics will rise to 30-35 percent.

As for the career after PhD, women have remained in academic pursuits to almost the same extent as men.[10] However, the proportion of women among assistant professors today compared to the proportion of females of the PhDs in the past decade gives cause for concern, as there appears to be leakage from

9 See Henriksson (2000) for an overview of Karin Kock's impressive career. Recently Niskanen (2007) has applied a gender approach to analyze Kock's career as a scientist and a public official. She exposes the strong gendered structures of economics at the time, as well as how Kock handled the direct and indirect discrimination she met.

10 The Swedish academic system has by tradition been rather closed with little cross-border movements, perhaps with the exception for movements among the Nordic countries. Thus we think this conclusion is plausible. Recently, international recruitment and competition is increasing, especially on the full professor level, but as yet we do not find it large enough to influence the conclusions in this study.

graduation to continued academic research.

The greatest hurdle for women is advancement to full professor. This is another significant "leak." The proportion of women of the full professors in economics corresponds to women's proportion of completed doctorates in the 1970s, three decades ago. If women progressed as men do, what percentage of full professors in 2007 would we expect to be women? It takes many years to qualify for a full professorship. We must then look at women among the PhDs in economics about 15 years back in time. At that time this proportion was around 8-10 percent. From a gender equality perspective, we should thus have had about three more women full professors at Swedish universities today.

The situation in other disciplines

Is the gender balance in economics worse than in other academic disciplines in Sweden? The proper norm for comparison may of course be a subject of contention. Should it be women in education or academic positions in general at the universities, women in the other social sciences, or maybe women in the natural sciences or the engineering schools? The answer might be related to one's view on the nature of economics—is the essential characteristic of the discipline found in abstract explorations or in applications and public discourse?

The homepage of Statistics Sweden displays pretty much any norm of comparison one may wish to use.[11] We have selected data for the universities as a whole, the social-sciences as a whole, and political science, business administration and mathematics. The results are displayed in Table 1.

Table 1. Graduate Students, Teaching and Research Staff by Subject Area and Sex. Percent women 2005

	Total (%)	Full professor (%)	Doctoral students/ degrees (%)
All	29	17	45[a]
Social sciences	34	19	51[a]
Political science	27	23	52[b]
Business administration	20	13	58[b]
Mathematics	15	5	17[a]
Economics	17	1	26[a]

Note: a) completed degrees b) registered doctoral students. Source: www.scb.se, Utbildning och forskning.

11 The data is found under "Education and research" (Utbildning och forskning) at www.scb.se.

The collection and presentation of data by Statistics Sweden is not the same as ours. It counts academic staff by type of employment, not credentials. Thus, persons with a docent title cannot be identified. Also, for reasons we have not been able to determine, Statistics Sweden registered only one female professor in economics in 2005. In order to have data comparable between the subjects, we stick to the data from Statistics Sweden throughout Table 1.

Accordingly, in 2005, women constituted 29 percent of all those employed as professors, research assistants, and university lecturers at Swedish universities, and 17 percent of the full professors. Finally, women completed no less than 45 percent of all doctoral degrees. In the social sciences, women fared even better, constituting 51 percent of the doctors, 34 percent of the teaching and research staff, and 19 percent of the full professors. Table 1 shows that women are much more present in business administration and political science than in economics. All of the comparisons indicate under-representation in economics.[12]

Doctoral degrees by women are 17 percent in mathematics as compared to 26 percent in economics. Percent faculty women is 15 percent in mathematics, versus 17 percent in economics. However, women full professors in mathematics were 5 percent (a total number of 8 persons), higher than the 1 percent (1 person) in economics according to Statistics Sweden. From these data, economics appears to be more like mathematics than the other social sciences.

ACROSS THE 5 COUNTRIES, THE SITUATION IS REMARKABLY ALIKE

From student enrollment[13] to faculty positions, the representation of women in economics in leading Anglophonic countries is rather similar to Sweden. In Table 2 we provide comparative data on the female representation among graduate students and academic staff. Our overall impression is that the situation for

12 The figures above are for the point in time 2005. The Swedish National Agency for Higher Education (Högskoleverket 2006) recently presented longitudinal data for the cohorts graduating with a PhD between 1980 and 1991. Not surprisingly they find that men become full professors to a much larger extent than women, regardless of academic field. As an illustration, among those who obtained their doctorates in 1991, 8 percent of the men, but only 4 percent of the women became full professors within a twelve-year period. There is no indication of later cohorts catching up.

13 In the US in the 1970s women made about one-quarter of undergraduate economics majors. Women's proportion of economics majors peaked in the mid-eighties, then fell and stabilized around 30 percent in the early nineties, and briefly re-attained the earlier peak 35 percent in the early 2000s (Blau 2004, Siegfried 2006). For 2004-05 Siegfried (2006) reports 32 percent women among economics majors. For Great Britain in 1998, Booth et al. (2000) note a proportion of 34 percent women among students enrolled for a Masters in economics, whilst the latest survey of the Royal Economic Society finds the level up to no less than 42 percent in 2004 (Burton and Humphries 2006). The British study (Burton and Humphries 2006) points to the differences between UK-citizens and students from abroad. Of the students from the UK, the proportion of women is only 26 percent, and thus it was significantly higher among foreign students. A Canadian study reports that women made 39 percent of the total Masters program participants in 1999 (CEA 2001). In Australia, in 2002, 42 percent of the total number of undergraduates and 37 percent of the honors enrollment were women (Hopkins 2004).

women in academic economics is remarkably alike across the five countries. The countries show the same pattern of increasing under-representation by seniority and status. Women PhD students today make around one third of the enrolled in the five countries, but much less among academic staff. In four of the countries we also find almost a third of the assistant professors to be women. Sweden with 19 percent differs significantly.

Most striking is the low representation of women among full professors, ranging from 5 to 9 percent. In several countries, like in Sweden, women economics full professors are new since the 1990s. Also, they are few in absolute numbers. Australia, for example, reports four women full professors in 2003. Another common observation in all the studies is that women are under-represented in economics relative to academia as a whole.

Ginther & Kahn (2004) include comparisons by academic field within the US. They find a lower percentage of women doctorates in economics than in statistics, political science, and the life sciences, about the same in physical sciences, while it is higher in economics than in engineering. The percentage of tenured female faculty follows the same pattern. The differences between men and women in the probability of promotion and the duration to tenure are the largest in economics.

Table 2. Representation of Women by Academic Grade in Sweden, USA, Great Britain, Canada and Australia. Percent Women

Country	Sweden 2006	USA 2005	Great Britain 2004	Canada 1999	Australia 2002
Full professor	6	8	9	5	5
Associate professor, senior lecturer	22	20	16	11	13/16
Assistant professor, lecturer	19	30	30	31	31
Total	16	16	19	13	20[a]
PhD-students	32	32	32	32	31
Completed PhD	26	31		27	

Note: For Sweden ”Full professor” is ”professor”, ”associate professor, senior lecturer” corresponds to ”docent” and ”assistant professor, lecturer” is researcher/teacher with a completed PhD. a) includes associate lecturers. Sources: USA: CSWEP's annual report 2005, Great Britain: Burton & Humphries (2006), Australia: Hopkins (2004), Canada: CEA (2001), Sweden; this article Table A3, A4 and A5.

Although the studies lack extensive trend data, they nonetheless indicate that women have made significant gains over time among doctoral students and those employed in lower academic ranks. The Australian study reports stability in the overall proportion of women faculty, but an improvement in female representation at the top end of the academic hierarchy between 1997 and 2002. PhD enrollment increased slightly during the same period. The Royal Economic Society surveys, conducted bi-annually since 1996, show a gradual increase in the proportion of women graduate students as well as faculty, with greater gains at higher academic grades. The proportion of female professors, although low in number, doubled over the period. The Canadian study provides only the situation for one year, 1999.

In the US the status of women in economics has been monitored over many years, ever since the inception of CSWEP in 1972.[14] Since 1972, women's fraction of PhDs awarded has quadrupled, from 8 percent to 31 percent. In 1972 women made less than 5 percent of faculty members compared to 16 percent in 2005. In addition women full professors constituted 8 percent in 2005, up from only 2 percent in 1972. The development has varied from year to year. A few periods of stagnation or decrease notwithstanding, the trend has been a continual rise. Nonetheless there is some indication that the female share of new PhDs has more or less plateaued since the late 1990s, but the percentage of female assistant and associate professors has risen in the last decade as a whole. On the other hand, women's representation among full professors was at about the same level in 2005 as ten years earlier. While the flow of women into the economics profession seems to have started somewhat earlier in the US, the other countries included in Table 2 now appear to be catching up. Despite the US lead in the output of doctorates for a number of years, we do not see the corresponding differences at the seniority level among academic staff. The low representation of women at the academic grade of full professor in the US cannot be explained as a cohort effect.

It is intriguing that the situation is so similar across the five countries, despite large differences in academic systems, labor markets, women's labor-market participation, fertility, and family policies.

Why so few women economics professors?

It is tempting to once again cite Knut Wicksell—this time from the poetic "Address to Woman" delivered at the Nordic student festival at Uppsala University in 1878. This fiercely feminist poem, which appeared before he started studying economics and which immediately made him notorious all over Sweden (Gårdlund 1995, p. 38-39), departed radically from the traditional themes of women's grace and beauty and instead included verses on poverty, prostitution, and women's low wages. In a sharply ironical verse, he suggested that because of women's diminu-

14 The annual reports from The Committee on the Status of Women in the Economics Profession (CSWEP) can be found in the Proceedings section of the May-issue of the American Economic Review. The data we have used for the US are from economics departments with a PhD program.

tive appetites, men wisely "arranged it all," such that the fruits of women's labors "in just proportions should be small." However, this suggestion was set out many years before Wicksell started studying economics. Let us thus speculate further about reasons for the lack of gender balance in economics.

There are three questions to be answered:

1. Why are women underrepresented in economics?
2. What explains the rapid inflow of women during the 1990s and 2000s?
3. Why do women not advance more readily to full professorship?

The study of the field of economics by the Swedish National Agency for Higher Education explains the low presence of women in economics in the following way: "The discipline is considered to make demands for abstract and analytical thinking as well as proficiency in mathematics, which are said to not attract female students" (Högskoleverket 2002, 10).[15] We find it difficult to believe in such a simple explanation. In particular, how then would we explain the strong flow of women into economics in the five countries, at the same time as the profession has moved in the direction of more abstraction and more mathematics?

In general, economists regard occupational segregation by gender as the outcome of complicated interplay of supply and demand.[16] Tastes and preferences, talents and capabilities may be part of the story but economists generally stress human capital investments guided by expectations about future labor market participation. Other economic theories emphasize discrimination or structures and institutions that inhibit certain choices or make them more costly for one gender than the other.

Most likely it is increased and extended labor force involvement—not a change in women's capacity for abstract and analytical thinking—that has contributed to their enhanced interest in economics. Goldin et al. (2007) analyze the narrowing and reversal of the US gender gap in college attendance. According to their findings increased expectations about future employment, rising age at first marriage, more effective birth control methods, and postponed childbearing encouraged women's investments in education. At the same time as women increased their overall investments, they started taking more math and science courses in high school and began catching up with boys on achievement tests in this area. These same factors, investing for a future in the labor market, should have worked to stimulate women's entry into economics.

In addition, the more visible women economists become—as economic journalists, public intellectuals, public officials, professors, and researchers—the more women will see economics as a realistic, attractive, and profitable career

15 This is similar to one of the arguments made by Lawrence Summers in his now infamous presentation at Harvard, suggesting that women and men differ in abilities. See May (2006).

16 For a review see Jonung (1998).

choice. This is the so called role-model effect (Booth et al. 2000, Hopkins 2004, and Burton & Humphries 2006). It is worthwhile to note that such a role model effect also influences the demand side. If employers get used to seeing women in positions as professional economists, the basis for and expected return of any statistical discrimination will be reduced and employers will be less hesitant to hire women economists. A vicious cycle inhibiting women's choice will change to a virtuous cycle of expanding choice. With increased attachment to the labor force by women, we should expect a continued rise in women's fraction of economists.

Research on the market for new PhDs in the US also indicates that the problems for women in the "early pipeline" are limited (Siegfried & Stock 2004, Siegfried & Richs 2006, Stock 2006). For most of the outcomes studied—financial aid, attrition, time-to-degree, full-time permanent employment, obtaining an academic job, starting salary, and measures of job satisfaction—there are no significant differences between men and women, controlling for other determinants.

Why then have women economists found it difficult to advance and obtain the same economic rewards within university hierarchies as men? Is this just a cohort effect reflecting the recent entry of women into economics, where we can expect the young women streaming into economics to catch up eventually and find their way into professorial positions? After all, with the exception of the US, there were hardly any women professors at all before 1990. Thus, there is certainly a cohort effect present. However, all of the country studies cited above conclude that the cohort effect and the time it takes to move along the pipe-line is not a sufficient explanation for the low presence of women at higher levels. Everywhere there appears to be a glass-ceiling, either in the tenure-decision or in the promotion to full professor. Other studies, mainly on the basis of US data, have identified gender differences in wages and other economic returns as well.[17]

The proposed explanations for the lack of women professors in general and their lower economic rewards are no different than the causes suggested for the lack of women in positions of authority in other areas, as managers, business leaders or top public officials. Roughly we can divide the explanations in four areas: (1) discrimination—those responsible for recruitment and promotion prefer men; (2) preferences and family obligations—women take the larger share of family and child-care which restricts their input at work and lowers their productivity or makes women chose career tracks that are less demanding; (3) societal institutions—the combination of labor markets, wage setting, family policies, social policies, tax policies creates differential incentives for men and women and may encourage a gender division of labor; and (4) institutional factors internal to a profession or an organization. All of these explanations have in various forms been put forward to explain the lack of women at the top positions in economics as well.

Several studies of women in economics focus on the probability of obtain-

17 See Blackaby et al. (2005), Ginther & Kahn (2004), (2006a), (2006b), McDowell et al. (1999, 2001), and Stock (2006).

ing tenure and being promoted to full professorships. They find a promotion gap, such that women's prospects of obtaining tenure are inferior to those of comparable men. The differences in probability of promotion cannot be explained by observable characteristics such as age, family characteristics, quality of PhD-training, field, employer, or publications.[18] The promotion gap difference still persists in the 1990s (Ginther and Kahn 2004). While family characteristics (marriage and children improving men's promotion chances while harming women's) and productivity differences to some extent explain women's slower progress in economics, a significant portion of the gender promotion gap remains unexplained.

Ginther and Kahn (2004, 2006a, and 2006b) further explore the differences between economics and other academic fields. An important result of their work is that the outcomes for women differ significantly by academic field. In all aspects, economics is found to be an outlier, the academic field with the greatest gender differences in career attainment. The female proportion of the tenured faculty is lower in economics than within the sciences and far lower than within the social sciences. Women in economics take longer and are less likely to achieve tenure compared to men than in other disciplines. The effect of factors such as having small children on obtaining a tenure track job or promotion to tenure or full professor is stronger than within the other sciences. The explanatory variables used, representing various family and productivity characteristics, explain less of the promotion gap in economics than elsewhere.

One commonly invoked explanation for women's slow advancement in economics is that they publish less in scientific journals; as such publications are the most important academic qualification today. Indeed, some research indicates that publications are more important for women's advancement than men's (Ginther & Kahn 2004). Lindqvist (2003) ranked all Swedish researchers in economics on the basis of their publications in international journals from 1969 to 2002. He looked at a somewhat wider group than ours, including emeriti, persons at external research-institutes and some who do not have a degree in economics but work in economic research institutes. In his study, there are four female professors out of 130, i.e. three percent. The corresponding number for female associate professors (docents) is 18 percent and for PhDs 20 percent. The publications in his study are weighted according to the status of the journal in which they have been published.

Women economists publish far less than men according to this ranking. Although women in total constitute 14 percent of the researchers studied, they only gather 1.8 percent out of the total publication points. However, the situation looks quite different according to the researcher's level of qualifications. The female full professors hold only 0.1 percent of the total publication points for full professors, the female associates hold 5 percent, but the female PhDs hold as much as 16 percent of the total points for this group. Thus, among the younger group of

18 See e.g. Kahn (1995), McDowell et al. (1999, 2001), Ginther and Kahn (2004).

PhDs, women's publication record is not that much poorer than that of men. In other words, women's average publication points in relation to those of men are 3 percent, 26 percent, and 74 percent for the respective academic rank.[19] However, we do not know if the gap will grow as these women get older.

Publication credits as measured by the method of ranking of Lindqvist are very unevenly distributed. The ten most prominent economists in Sweden are credited with 40 percent of the total publication points. Thus, the absence of women in the top group has a significant effect. The highest-ranking woman is found in place 79 with 32 points compared to 1300 for the highest-ranking man.

Henrekson and Waldenström (2007) study different measures ranking the performance of researchers in economics in Sweden. They caution against the use of one-dimensional measures of output and criticize, for example, the endorsement of a specific measure by the European Economic Association. Comparing seven different measures, they find that the measure which is endorsed (the one used by Lindqvist above) gives rise to a particularly skewed distribution of output among professors and exhibits the weakest correlation with other measures. Most interesting from our perspective is their result regarding the performance of men and women. Among the seven measures, it is only one (the one favored by EEA) where the gender variable is found to be statistically significant, indicating better research performance for male professors. With the six alternative measures, based on publications as well as citations, no difference can be ascertained. The choice of a performance measure may thus in itself be gender biased.

Even if a less impressive publication record might partly explain women's sluggish progress in economics, the question remains: Why do women publish less than men? One suggestion is the presence of sorting by gender, such that men write with men and women with women, thus making it more difficult for the minority, women, to find co-authors.[20] Women single-author more frequently than men. A recent study (Boschini & Sjögren 2007) does indeed establish that co-authorship in economics is not gender-neutral and that there is a gender sorting, not related to field, university affiliation, or seniority. As the share of women in the profession rises, women may find it easier to find co-authors, but on the other hand gender-segregation may increase and we cannot expect the prevalence of gender-mixed team to automatically grow.

Another suggestion, particularly in the US environment, is that the academy is a family-hostile environment, with the expectations of total career commitment and short leaves at child-birth.[21] Why then would Sweden—according to some

19 Results in Boschini & Sjögren (2007) are similar; the publication gap is small or non-existent for younger women. According to the investigation by the Canadian Economic Association (2001) younger women publish even more than younger men, while the opposite is true for more senior researchers.

20 This is i.a. suggested by Ginther & Kahn (2004), Kahn (1995) and McDowell et al. (1999, 2001).

21 See e.g. the discussion on Work and Family in Academia: Striking the Balance in *CSWEP Newsletter* Spring/Summer 2007.

gender equality indices the most equal in the world[22]—have been so slow in attracting women into professorships and have such a scarcity of women at top levels in management and professions? In Sweden many of the family-friendly policies seen as the solution for achieving more gender equality in other countries are already in place, such as generous parental leaves at child-birth and for sick children, extended child-care, opportunities for part-time work, as well as large degree of public and political consensus on the goal of gender equality. However, as pointed out by many researchers, these policies may act as a double-edged sword.[23] While the family policies help in combining work and family, they may not encourage women's aspirations to higher positions. The generous leaves for child-care make women take long absences from work (80 percent of leave time taken is taken by women). The high level of taxation, partly generated by such generous policies, may make the pecuniary rewards of a high work effort and time-input seem too small. This is re-enforced by relatively high wages at the bottom end of the wage-distribution, making it expensive to buy services. Thus, the family-friendly climate in Sweden may encourage fertility and labor force participation by women, but provide small incentive for pursuing top-level careers.

As standard variables have not been able to explain the lack of progress of women through rank, a number of studies have discussed factors internal to departments or research organizations (e.g. Kahn 1995, Siegfried & Stock 2004, McDowell et al. 2001, and Booth et al. 2000). Women tend to do more teaching than men (Stock 2006). Women may choose to take on—or be pressured into taking on—administrative work. The desire to have female representation on various committees and for external assignments tends to over-load the small number of women. Being a minority may make it more difficult for women to find inspiration, mentors, or engage in professional networks. These are the very reasons for the founding of female-economist organizations and networks.

Sometimes it is suggested that the very professional culture, developed through history by men, keep women out.[24] The "rules of the game" are set by the values of the majority. Klein (2005, 145) describes academic economics as a self-organizing, self-validating club, in which you are initiated through graduate training, similar at top schools throughout the world. Looking at PhD production and placement, he shows that the top economics departments dominate and arguably set the tone of the profession. His paper does not consider gender issues. While Klein worries that the outcome may be a conformist political culture, another outcome could be a culture shaped by male values, putting women off. There are a number of studies on the influence of values and social norms on economic decision making. As far as we know they have not been applied to the economics

22 See for example the gender empowerment measure by UNDP (link), or the global gender gap report by the World Economic Forum. Link.

23 See e.g. Albrecht et al. (2003), Meyersson et al. (2006), and Booth (2007).

24 May (2006) describes the arguments that have been put forward to defend women's absence in academia, adopting a historical perspective.

profession itself. Colander and Holmes (2007) have studied how women fare at the top US graduate schools in economics. They find evidence that women are less satisfied with their studies and less integrated in the academic economics profession than men.[25] We do not know if the same applies to female faculty.

Finally, all the possible reasons for the under-representation of women professors suggested above apply to all scientific fields. We are still left with the question why economics, from the studies available so far, seems to be less gender-balanced than any other field? Why does economics differ from other social sciences as well as natural sciences? Maybe future studies could identify fields more similar to economics—perhaps mathematics or philosophy?

THE FUTURE

What are the changes in gender balance that we can expect in the foreseeable future? How quickly will the current trend of a rising supply and greater publication consciousness of women doctorates in economics be reflected in the composition of teachers, researchers, and professors at the universities? In the immediate future in Sweden, many faculty members from the large birth cohorts of the 1940s will enter retirement, making room for change.

In our Swedish article on the status of women in the economics profession (Jonung & Ståhlberg 2003), we used information on the distribution of the academic staff in economics across age, sex, and academic rank in 2001 to estimate the number of retirees as well as the recruitment base for the positions likely to be opened for the next ten years through the process of retirement.[26] We estimated that if women were to receive positions in direct proportion to their fraction of the recruitment base, by 2011 there would be about 6 percent women among full professors and 18 percent women among associates and lecturers.

The purpose of our calculations was to demonstrate how slowly the process of transformation works in a system such as a university hierarchy, where it may take up to two decades to demonstrate ability and achieve advancement. We pointed out that the calculations assumed a static system and might be altered by the creation of new positions, the arrival of competent persons from abroad, new forms for professorships, etc. Today, we can already observe that the expectations have been surpassed. Women's proportion of full professors is up to the level projected for 2011 and their proportion of lecturers/associates has surpassed expectations. Women have so far progressed through the system faster than anticipated.

25 They suggest that one of the causes may be a focus on abstract theory rather than applied policy in core courses of major graduate programs.

26 The distribution table was developed by the Swedish Confederation of Professional Associations. See Jonung & Ståhlberg (2003) for references and methods.

On the other hand, we may compare the Swedish situation with developments in the United States. Despite a strong output of women doctorates for a number of decades, the growth in the proportion of women among full professors has only been around a few percentage points per decade. During the last decade the proportion has remained around 8 percent, varying between 6 and 9 percent. An increased supply of women with research competence does not automatically lead to an increased representation of women among full professors.

REACHING THE TOP

Women in economics were rare in Sweden well into the 1970s. Since then substantial changes have taken place. Nevertheless, despite an increasing number of women attaining a PhD during the past three decades, only 5 women, or six percent, were full professors of economics in 2006. In the pipeline we find women to be about one fifth of the associate and assistant professors, about one fourth of the PhD graduates and about one third of the upper-level economics students. Women's representation in academic economics in Sweden is remarkably similar to that in other countries for which we have comparable data. Women are under-represented and everywhere the hardest glass ceiling is found at the top, either in the tenure-decision or in the promotion to full professor. Another common experience in various countries, including Sweden, is that economics is less gender-balanced than the universities as a whole or the social sciences in general, and in some cases less so than in the natural sciences as well.

There is no consensus as to the causes of women's slow advancement in academic economics. Even after adjusting for factors representing family background or productivity a considerable portion of the gender promotion gap remains unexplained. In addition, the search for explanations has to consider the exceptionality of economics.

The much sought-after fruits are there—at the top, in principle accessible to all. However, to reach them requires climbing—ladders and branches. It is a laborious climb; it requires purposefulness, technique, cooperation, capacity to take hard blows, and now and then a helping hand from those higher up. Neither is it evident from the bottom where the most savory pieces of fruits are located, nor where the most advantageous climbing route is to be found. Our survey shows that women are on track, have developed a taste for economics, appropriated the climbing techniques and are numerous enough to assist each other en route. However, it has also illustrated that the climb is long and time consuming. It will take several decades before we can hope for a common, gender-balanced, feast.

University training in economics is an excellent route by which to acquire influence in public debate and policymaking. When women in the field of economics are few, it means that their influence on the economic agenda—the questions studied by economists, the methods by which they are analyzed, and the answers given—will be relatively small. Thus, the absence of women in economics is not only an example of gender inequality; it may also cause further gender inequality. Alternatively, if as economists we believe in economics as a rich and powerful tool to analyze the world and to reach wise decisions on policy, we would want this tool to be in women's hands also. A recent article in the *Economist* (April 12, 2006) coins the term *Womenomics* to describe women's rising importance in the global market place—as workers, consumers, entrepreneurs, and investors. As more economic decisions are taken by women, more women with training in economics would seem to be a wise social investment. Thus, there are reasons to monitor women's progress and the gender balance in economics.

APPENDICES

Appendix 1: Data on Women Economists in Sweden, by Christina Jonung and Ann-Charlotte Ståhlberg. Link.

Appendix 2: Women in Professional and Public Economic Debate, by Christina Jonung and Ann-Charlotte Ståhlberg. Link.

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Go to *Gender and Economics* Symposium Page

Go to May 2008 Table of Contents with links to articles

Go to Archive of **Investigating the Apparatus** Section

Econ Journal Watch,
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Symposium: Gender and Economics

On Gender Balance in the Economics Profession

Ann Mari May[1]

A comment on: Christina Jonung and Ann-Charlotte Ståhlberg, "Reaching the Top: On Gender Balance in the Economics Profession," *Econ Journal Watch* 5(2), May 2008: 174-192. Link.

Abstract

Jonung and Ståhlberg speak to the issue of missing women in the economics profession in five industrialized countries—the United States, Australia, Great Britian, Canada, and Sweden. As their article indicates, women have made significant strides in the last third of the twentieth century in expanding their representation as students in higher learning in these and other countries throughout the world. However, what they ask us to consider is the sometimes ticklish question of why we have not seen a proportional increase in the representation of women as faculty—particularly in the discipline of economics.

They are not the first ones to ponder this question. In the US, where women have made the most progress, it is increasingly obvious that while women are present as students, they are distinctly absent as faculty in the halls of ivy—at least in some halls. As the former president of Harvard University, economist Lawrence Summers discovered, it requires some finesse to try and explain their absence.

Of course most economists are ill-equipped to answer this question. "Academic Anthropology 101" it would seem, might just require a historical perspective, critical habit of thought, and an understanding of the importance of "shared tacit knowledge," academic rituals, and hierarchy.[2] Most important of all, it may require an appreciation for the ways in which power is distributed in institutions and gender is reflected in power relationships—all of the things that economists

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2 Michael Polanyi, 1958.

eschew or that have been beaten out of them in graduate school.

Jonung and Ståhlberg provide support for the notion that the paucity of women in certain disciplines, including economics, is widespread—not just a matter of a few "bad apples," be they departments or counties. We might expect to see this in the US, but when we see this same pattern in Sweden, the apotheosis of a country tuned-in to gender equity, we cannot help but be somewhat concerned. Moreover, when countries such as Sweden, Canada, and Norway, have implemented programs to increase the representation of women faculty, these programs have often been bitterly criticized by male faculty and thrown out by (mostly male) courts.[3]

Economists, not surprising perhaps, have been instrumental in providing an explanation for these missing women across the academic globe. They have tended to focus on investments in human capital—an explanation that seems to perform double duty in blaming women themselves for their own absence while providing the soothing balm that it is just a matter of time before their representation in the dismal science increases. In this, Jonung and Ståhlberg are appropriately skeptical.

While the view of that it is just a matter of time may make for good bedtime reading, it is, in the light of day, a bit more problematic. Take, for example, the US which has the most experience from which to draw. Women, who had worked to gain admittance into institutions of higher learning in the later part of the nineteenth and early part of the twentieth century, were, however, increasingly segmented into disciplines thought to be appropriate for women.

Not only were women driven from the field of economics to the feminized field of home economics in the early decades of the 20th century, but evidence that they were ever even present seems to have been carefully wiped away through "systematic misattribution" and "lack of citation of the work of women economists."[4] As Mary Ann Dimand, Robert Dimand, Evelyn Forget and many others have shown, women were active participants in researching and publishing in the emerging professions of the social sciences including economics. It will surprise many to learn that the lead article in the inaugural issue of the *American Economic Review* was by Katherine Coman, a female economist.

More helpful perhaps, are other studies that Jonung and Ståhlberg discuss that ask questions about an institutional culture that works to vet women early in their coursework, makes their graduate school experience less satisfying, prevents

3 In 1995, the Swedish government created 32 posts at the full professor level, the so-called Tham professors, especially for women. Men were allowed to apply but would only be given the job if there were no qualified women. But in 2000, the EU Supreme Court turned down the Tham proposal. In 2000, the University of Oslo implemented a plan to improve gender diversity among faculty by reserving 12 full or associate professorships for female candidates. However, in January 2003 the European Free Trade Association Court ruled it illegal for the University of Oslo to reserve faculty positions for women. See Chronicle of Higher Education, World Beat, "Court Bans Female-Professor Quota at U. of Oslo," February 29, 2003.

4 Mary Ann Dimand, Robert Dimand, and Evelyn Forget, 1995, ix.

women from achieving tenure at the same rates as men, and provides fewer opportunities for co-authorship, hence diminishing their publication record.

However, neither the human capital explanations nor these insights into women's systematic disadvantage in the academic culture of economics, probing as they do, a set of important, but narrowly constructed questions, seem to fully explain the unique situation of economics as a social science.[5] To do this may require that we think more carefully about the ways in which power is distributed in institutions and about the political economy of knowledge. This may, of course, be more than most economists can muster. However, we might do well to consider the views of one of the original economic anthropologists of the modern era—Thorstein Bunde Veblen.[6]

In *The Higher Learning in America* and *The Theory of the Leisure Class,* Veblen reminds us that we must view academia as an institution that distributes power (much like any other institution), is preoccupied with status maintenance (probably more than other institutions), and is influenced by the values and imperatives of society. Veblen argued that women play a central role in status maintenance in higher education and he viewed the reluctance of institutions to accept women as a ceremonial vestment aimed at status maintenance.[7] That higher education today is driven by status maintenance should be uncontested and the role that women have played in this status maintenance is significant, even without a t-statistic propping it up.[8]

To the extent that women were allowed the privilege of admission into the higher learning—in other words, when serving as a useful source of revenue, Veblen points out that women were thought to be constrained to acquire knowledge in those areas that would allow for the "better performance of domestic service" or to the "quasi-scholarly and quasi-artistic" areas that come under the head of a "performance of vicarious leisure."[9] In other words, women are accepted as consumers in the higher learning and segregated in areas thought appropriate to women.

5 There is disagreement on the assertion that the discipline of economics is different than other disciplines in its failure to accept women and integrate feminist insights. Psychologist Virginia Valian (1999), whose important book *Why So Slow?*, examines these issues in a variety of professions, disagrees with me a bit on the unique nature of economics. In a session at the AEA meetings on women in the economics profession which included the pre-eminent psychologist along with MacArthur Foundation award-winning economist Heidi Hartmann, only two men found the topic of sufficient interest to attend—one of whom was the president-elect of the sponsoring association and the other a reporter for the *Chronicle of Higher Education*. The reporter, who wrote a nice little piece about the progress that women were making in economics, apparently missed the significance of a room full of women and one lone male economist! The degree to which sessions on topics related to gender often fail to attract the interest of male economists would appear to reflect the unique nature of economics—at least among the social sciences.

6 For a full discussion of Veblen, women and higher education see May forthcoming.

7 Thorstein Veblen, [1899] 1998, 376.

8 For further evidence see, for example, May and Moorhouse 's unpublished paper which shows a direct relationship between status measures, such as Barron's Profiles of American Colleges, and the representation of women faculty at research universities.

9 Veblen [1899] 1998, 376.

Veblen provides a key, perhaps, to understanding the relationship between increased competition and increased pressures for status maintenance. Periods of increased competition put a premium on reputation and heighten status-seeking activites. I agree with David Colander, that the discipline has changed such that mathematical model building is no longer *as* fashionable as it once was and empirical, statistical analysis has gained in stature. Increased competition in higher education, with its pressure to publish the "unread and unreadable," has made ideology in a narrow sense less important than having a skill set that is easily transferable. Marxist economists (if any still exist) can work along side neoclassical economists (if any still exist) so long as they can "take an obscure little problem that no one has thought much about, blow it all out of proportion, and solve it, preferably several times, in prestigious [journals].[10] The degree to which women are willing to engage in such activities is a question yet to be answered. I would venture to say that, to the degree that they may be more likely to eschew such nonsense, they will continue to be marginalized in this discipline.

Why does it matter? Jonung and Ståhlberg suggest the following: "… if more economists are women, economic analysis will be richer, and if more women are familiar with economic reasoning, public debate will be stronger and deeper." On this I would agree, with the caveat that women's familiarity with economic reasoning may indeed allow them to better articulate the often flawed nature of that reasoning. But I think it is more important than even this.

At the heart of the matter is the question of identity and agency. Those with the access to write the canon are powerful shapers of identity—how we view ourselves and others. The construction of identity is particularly important in that it so strongly influences agency—the ability of individuals to act within the context of being affected by institutions. While most discussions on the importance of education by economists focus on pecuniary considerations, the role of education in determining agency is potentially profound. As Amartya Sen[11] and Martha Nussbaum[12] have argued, education adds not only to *human capital* but *human capability*, enabling women to exercise their legal rights as well as strengthening their political and civic engagement. It is perhaps because central economic questions concerning provisioning might well change with the full inclusion of women's voices, that this terrain is so deeply contested.

10 Kenneth Lasson, 1990, 936.

11 Amarta Sen, 1999.

12 Martha Nussbaum, 2000.

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Go to *Gender and Economics* Symposium Page

Go to May 2008 Table of Contents with links to articles

Go to Archive of **Investigating the Apparatus** Section

Econ Journal Watch,
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SYMPOSIUM: GENDER AND ECONOMICS

Mr. Max and the Substantial Errors of Manly Economics

DEIRDRE NANSEN MCCLOSKEY[1]

A COMMENT ON: CHRISTINA JONUNG AND ANN-CHARLOTTE STÅHLBERG, "REACHING THE TOP: ON GENDER BALANCE IN THE ECONOMICS PROFESSION," *ECON JOURNAL WATCH* 5(2), MAY 2008: 174-192. LINK.

ABSTRACT

JONUNG AND STÅHLBERG DO A VERY HELPFUL JOB IN REVIEWING THE EVIdence on how much and why women are so under-represented at the heights of academic economics. The countries they study are of course not the worst, and perhaps among the best. The five women in Sweden with the title of "professor of economics" could meet in a closet with the number of such women in the Netherlands—there, too, the numbers are small, and in the Netherlands the numbers are smaller still in, say, History.

It *is* interesting, as Jonung and Ståhlberg observe, that the facts are so similar across countries, "despite large differences in academic systems, labor markets, women's labor-market participation, fertility, and family policies" (181). As they note, family-friendly policies do not seem to be the key—though such policies are desirable in themselves. A study at Princeton found that women were *less likely than men* to ask for extensions of the tenure clock for childbirth. "When we asked [female] people to comment, they said things like: we don't know if it's OK to ask for it, we're afraid we'll be seen as less serious, we're afraid we'll be penalized in the tenure consideration" (Adams 2008, 832).

But I do worry a little that the studies Jonung and Ståhlberg cite—proving for example that outcomes don't differ between men and women—turn on the magic word "significant." Looking directly for instance at Siegfried and Stock (2004) on the outcomes point confirms the worry. "Insignificant" differences there are not accorded any weight in the prose summaries of the results. But they

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should, if the coefficients are large enough to be important, regardless of the alleged "sampling" variability. I worry that so-called "significance" is being used to decide all the issues in the literature, regardless of what the size of the coefficients are. Jonung and Ståhlberg, for example, summarize the results of Henrekson and Waldenström as saying that "no difference" can be found in publication records between men and women, by which they mean "statistically" significant.

This is illogical. Statistical significance, though widely used in such a way in medicine and economics (though not in physics and geology), does not imply substantive significance. A "good" *t* test is neither necessary nor sufficient for a variable to be scientifically important, as Stephen Ziliak and I argue in our new book, *The Cult of Statistical Significance* (Ziliak and McCloskey 2008; and McCloskey and Ziliak 2008). The point is not ours. It has been made for a hundred years at the upper levels of statistical sophistication, without changing the belief at the lower levels that statistical significance is the same thing as proving that an effect "exists." The journal *Feminist Economics,* bless it, founded and edited by Diana Strassmann, has long had a policy of asking people to talk about the substantive significance of a variable, and I'd like to see Jonung and Ståhlberg do so. Strassmann's argument is that a properly feminist economics should be about important questions and substantive results. Surely. Blind tests of evaluation of CVs, for example, have shown that in the physical and biological sciences gender matters (Adams 2008, 836). The result is parallel to that found for race: American "black" names (Latisha as against Laura) substantially reduce uptake for employment. Such experiments, which could be replicated in economics, are more robust evidence that something is wrong than the standard errors of a non-sample sample in a questionably specified multiple regression.

And in any case our own experiences sometimes reveal more than the econometrics. The first time I was in a group of economists as the only woman, at Erasmus University in the winter of 1996, I made an economic point, which the guys ignored. A few minutes later George made the same point, and the other guys all said, "Gee, George, that's a *great* point!" I said to myself in triumph (I had been a woman at the time for two months only, and was worried about being accepted as one), "*Wonderful:* they're treating me like a woman! Hurrah!" I can tell you that I got over being pleased by such treatment very quickly!

Jonung and Ståhlberg write that "if more economists are women, economic analysis will be richer" (175). I agree—but only if the women are not enticed in their economic analysis to become honorary men. If they do that—and some of the most successful women in economics tend to—then we end up with the same economics we have now. Jonung and Ståhlberg note a worry, confirmed in the study they mention by David Colander and Jessica Holmes in *Feminist Economics* (2007), that economics has become "a culture shaped by male values, putting women off." The "leak" of young women away from the first course in economics, and even the leak between Ph.D. and full professorship, may have to do with the masculine character of *Homo economicus*. The fellow in question is not Latin *Homo*, really, meaning "human being," German *Mensch*, Greek *anthropos,* but *Vir*, "man" in the gendered sense. Since the invention of Samuelsonian economics, *Vir economicus* has been characterized as Max U (which in Burmese would mean "Max Mister": freely, "Mr. Max"; Max would be a more sensible person, incidentally, if he had a gender change and became Maxine U). What qualifies as "theory" in Samuelsonian economics is not, for example, the national income accounting and aggregate theorizing that characterized Swedish economics in the 1920s and was adopted by Keynes, not to speak of "habit" as understood by institutionalist economists or "discovery" as understood by Austrian economists, but exclusively a maximization of a utility function under constraints. Most young male economists nowadays simply can't understand a piece of economic reasoning that is not expressed as the adventures of Max U. It's game playing, or game theory: scoring football goals as a theory of the full life. No wonder that many women find it a trifle silly.

Women are discriminated against when they don't believe in Max U. It's a peculiar use of the phrase "discriminated against," of course, since the very game of Samuelsonian economics is to do Max U and to do Max U and to do Max U right through middle age and a full professorship, then to do it and to do it and to do it for a couple of decades more. But that means that *anyone* who doesn't believe in Max U as a full-feature description of economic agents and as the only method by which the study of humankind in the ordinary business of life may be pursued is being discriminated against, losing heart, wondering if it's all worth it, depressed, confused. "Is this really science?" In this sense the blessed Adam Smith, John Stuart Mill, Edwin Cannan, J. M. Keynes, Friedrich Hayek, Ronald Coase, and Amartya Sen represents a wisely feminine sort of male economist. Only the 45-year old boys who just *love* one more argument that if you specify Max U *this* way you get *that* "result" can succeed at the Samuelsonian intellectual life.

But it's an idiot savant's life. It's made worse by the techniques added on to the game-playing of modifying Max U assumptions endlessly, namely, existence theorems and *t* tests. It's not mathematical modeling and the fitting of hyperplanes that are to be condemned. They are necessary in a science of observation. It's the particular techniques of existence theorems and *t* tests, widely practiced but never actually defended as to method, that need to go into the rubbish bin, in favor of loss functions and Bayesian econometrics and computer simulations and verbal theorizing and close study of multiple-dimensioned facts. Not to speak of common sense, an experience of life (having children, for example), and listening to the wisdom of the culture. Neither endless modifications of Max U nor existence theorems nor *t* tests tell us about the world as it actually is. If any of them did there wouldn't be so very many of them. That's economics.

So I would argue that the lack of gender balance in the economics profession indicates a more fundamental imbalance. Women of sense join with men of sense who *also* don't believe in Max U in all his mechanical glory, with his foolish aides the existence theorem or the *t* test, as the sole considerations in an economic argument—Austrian, institutionalist (old and new), feminist, public choice, development, some Marxist, many experimental, some behavioral economists, or merely economists wise to the follies who nonetheless have not yet found a group. True, the non-Samuelsonian groups sometimes have their own follies and modernist sins. But a pluralism that listened, really listened, to other economists would not be mechanical or game playing, and would be more attractive to people of sense and sensibility. We would get beyond Paul Samuelson's Ph.D. dissertation. We would get back, I would say, to a properly Smithian economics, which did economics as though people were people.

The virtue of prudence—and Max U is part of prudence, if nothing like its whole—belongs in economic arguments, of course. I myself have written whole books arguing so. We do the poor women of the world, for example, no favors if our economic advice ends up hurting them because we have indulged *our* sense of justice, say, rather than watching the budget constraints and keeping in mind the people offstage. Economists have always been especially good at giving advice on the virtue of prudence. "What is prudence in the conduct of every private family," wrote the great Smith, "can scarce be folly in that of a great kingdom." But an economics that ignores courage, justice, temperance, love, hope, and faith is always going to suffer, in its staffing and in its intellectual life, from a lack of balance.

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Go to *Gender and Economics* Symposium Page

Go to May 2008 Table of Contents with links to articles

Go to Archive of **Investigating the Apparatus** Section

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Symposium: Gender and Economics

Diversity in Tastes, Values, and Preferences: Comment on Jonung and Ståhlberg

Catherine Hakim[1]

A comment on: Christina Jonung and Ann-Charlotte Ståhlberg, "Reaching the Top: On Gender Balance in the Economics Profession," *Econ Journal Watch* 5(2), May 2008: 174-192. Link.

Abstract

Swedish propaganda has long maintained the superiority of the Swedish model of the welfare state, and Swedish social scientists typically reflect and support this world view. It is thus rare for publications to present evidence that raises doubts about the received wisdom, and even rarer for such challenges to come from Swedish scholars. Jonung and Ståhlberg's analysis of continued male dominance in the economics profession in Sweden is thus most welcome. As they point out, Sweden has invested heavily in "gender equality" and family-friendly policies.

The cross-national comparisons in Table 2 are the most valuable element in Jonung and Ståhlberg's paper. The comparisons show that the situation in Sweden, a relatively tiny and socially homogeneous society, is very close to that in at least four other affluent modern liberal societies that have also invested heavily in equal opportunities policies: the USA, Britain, Canada and Australia. Indeed, their analysis suggests that in some respects Sweden lags well behind these other countries as regards women's penetration into the economics profession. In Sweden, only one-fifth of assistant lecturer/lecturer posts in economics are filled by women, compared to one-third in the four other countries. The USA and Britain have the highest proportion of full professors of economics who are women: 8-9% compared to 5-6% in Sweden, Canada and Australia. This percentile difference is small but substantively large and important given that Britain and the USA have always had far fewer of the family-friendly policies that are claimed to help

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women to remain in their jobs after having children and to achieve work-life balance. On this evidence, the institutional context is far less important than Jonung and Ståhlberg want to believe. In fact, as they admit, family-friendly policies are probably counter-productive as they allow women to choose good work-life balance instead of a male-style work-centered career.

In seeking an explanation for these outcomes, Jonung and Ståhlberg rely on existing economic theories. In the 21st century, however, explanations must start with the diversity of lifestyle preferences and choices in modern societies, especially in the very large multicultural societies of the USA and Britain. This diversity is increasing with globalization.

As a sociologist, I have been working on issues of life goals, priorities, preferences, values, and gender-balance for some time. I was invited to participate in this symposium by the editor of this journal, who evidently knew something of my "take" on such matters. He encouraged me to use the opportunity to address findings and interpretations from my sociological perspective.

Within sociology, "preference theory" provides an explanation for the continued dominance of men in the highest-grade jobs and occupations, and for the relative lack of change in the standard measures of "gender equality" in recent years. Economists tend to think of behavior as reflecting 'revealed preferences'. In contrast, sociologists ask direct questions about values, priorities, preferences and life goals, and then examine how stated preferences and priorities play out in actual choices and behavior. My preference theory rests on a review of the most recent evidence to identify the three main types of lifestyle preference in modern societies. I have also shown that in some societies, the gap between preferences and behavior is wide; in others it can be small.

PREFERENCE THEORY

Preference theory is a theory for explaining and predicting women's choices between market work and family work. It is historically-informed, empirically-based, multidisciplinary, prospective rather than retrospective in orientation, and applicable in all rich modern societies (Hakim 2000).

Lifestyle preferences are defined as causal factors which thus need to be monitored in modern societies. Other social attitudes, such as patriarchal values and societal norms, have been found to be either unimportant as predictors of behavior or as having only a small marginal impact, by creating a particular climate of public opinion on women's roles (Hakim 2003b, 2004b). The theory has been tested with national surveys in Britain and Spain (Hakim 2002, 2003a) and the three lifestyle preference groups have been identified using existing survey data for several other modern economies.

Preference theory predicts a polarization of work-lifestyles, as a result of the

Table 1: The Four Central Tenets of Preference Theory

1. Five separate historical changes in society and in the labor market which started in the late twentieth century are producing a qualitatively different and new scenario of options and opportunities for women. The five changes do not necessarily occur in all modern societies, and do not always occur together. Their effects are cumulative. The five causes of a new scenario are: * the contraceptive revolution which, from about 1965 onwards, gave sexually active women reliable control over their own fertility for the first time in history; * the equal opportunities revolution, which ensured that for the first time in history women had equal access to all positions, occupations and careers in the labor market. In some countries, legislation prohibiting sex discrimination went further, to give women equal access to housing, financial services, public services, and public posts; * the expansion of white-collar occupations, which are far more attractive to women than most blue-collar occupations; * the creation of jobs for secondary earners, people who do not want to give priority to paid work at the expense of other life interests; and * the increasing importance of attitudes, values and personal preferences in the lifestyle choices of affluent modern societies. 2. Women are heterogeneous in their preferences and priorities on the conflict between family and employment. In the new scenario they are therefore heterogeneous also in their employment patterns and work histories. These preferences are set out, as ideal types, in Table 2. The size of the three groups varies in rich modern societies because public policies usually favor one or another group (see Table 3). 3. The heterogeneity of women's preferences and priorities creates conflicting interests between groups of women: sometimes between home-centered women and work-centered women, sometimes between the middle group of adaptive women and women who have one firm priority (whether for family work or employment). The conflicting interests of women have given a great advantage to men, whose interests are comparatively homogeneous; this is one cause of patriarchy and its disproportionate success. 4. Women's heterogeneity is the main cause of women's variable responses to social engineering policies in the new scenario of modern societies. This variability of response has been less evident in the past, but it has still impeded attempts to predict women's fertility and employment patterns. Policy research and future predictions of women's choices will be more successful in future if they adopt the Preference Theory perspective and first establish the distribution of preferences between family work and employment in each society.

Source: Hakim, *Work-Lifestyle Choices in the 21st Century*, Oxford University Press 2000.

diversity in women's sex-role preferences and the three related models of family roles. It argues that in prosperous modern societies, women's preferences become a central determinant of life choices—in particular the choice between an emphasis on activities related to children and family life or an emphasis on employment and competitive activities in the public sphere. The social-structural and economic environment still constrains women's choices to some extent, but social-structural factors are of declining importance - most notably social class.[2]Preference theory forms part of the new stream of sociological theory that emphasizes ideational change as a major cause of social behavior. Individualization frees people from the influence of social class, nation, and family, and agency becomes more important as a determinant of behavior. Men and women not only gain the freedom to choose their own biography, values and lifestyle, increasingly they are forced to make their own decisions, because there are no universal certainties or collectively agreed conventions, no fixed models of the good life, as in traditional societies. Preference theory can be seen as an empirically-based statement of the choices women and men actually make in rich modern societies. In sum, preference theory predicts diversity in lifestyle choices, and even a polarization of lifestyles among *both* men and women, although the initial emphasis has been on women.

Along with all the social sciences, economics tends toward a variable-centered analysis, which focuses on the average outcome, the modal pattern, and the central tendency. Such approaches tend to obscure the underlying diversity of family models and lifestyle choices. Those underlying realities only emerge clearly in studies using person-centered analysis, which is still uncommon, even in sociology and uses a different methodology (Cairns, Bergman and Kagan 1998 and Magnusson 1998).

Preference theory specifies the historical context in which core values become important predictors of behavior. Five historical changes, listed in Table 1, collectively produce a qualitatively new scenario for women in affluent modern societies in the 21st century, giving them options that were not previously available.

Reviews of the research evidence for the last three decades, particularly for the USA and Britain (Hakim 2000; 2004a), show that once genuine choices are open to them, women choose between three different lifestyles: home-centered, work-centered or adaptive (see Table 2). These divergent preferences are found at all levels of education and ability, in all social classes and income groups. Social origins become less important than motivation, personal life goals, attitudes, and values.

2 The declining importance of social class as a predictor of behavior and choices in the 21st century is most obvious in politics—as illustrated by the fact that personal values, rather than social class, differentiated support for Al Gore and George W. Bush in the closely contested USA election of 2000.

Table 2: Classification of Women's Work-lifestyle Preferences in the 21st Century

Home-centered	Adaptive	Work-centered
20% of women varies 10%-30%	60% of women varies 40%-80%	20% of women varies 10%-30%
Family life and children are the main priorities throughout life.	This group is most diverse and includes women who want to combine work and family, plus drifters and unplanned careers.	Childless women are concentrated here. Main priority in life is employment or equivalent activities in the public arena: politics, sport, art, etc.
Prefer *not* to work.	Want to work, but *not* totally committed to work career.	Committed to work or equivalent activities.
Qualifications obtained as cultural capital.	Qualifications obtained with the intention of working.	Large investment in qualifications/training for employment/other activities.
Number of children is affected by government social policy, family wealth, etc. Not responsive to employment policy	This group is *very* responsive to government social policy, employment policy, equal opportunities policy/propaganda, economic cycle/recession/growth, etc. Including: income tax and social welfare benefits, educational policies, school timetables, child care services, public attitude towards working women, legislation promoting female employment, trade union attitudes to working women, availability of part-time work and similar work flexibility, economic growth and properity, and institutional factors generally.	Responsive to economic opportunity, political opportunity, artistic opportunity, etc. Not responsive to social/family policy.
Family values: caring, sharing, no-competitive, communal, focus on cohesion	Compromise between two conflicting sets of values.	Marketplace values: competitive rivalry, achievement orientation, individualism, excellence.

Source: Hakim, *Work-Lifestyle Choices in the 21st Century*, Oxford University Press 2000.

The three preference groups are set out, as sociological ideal-types, in Table 2, with estimates of the relative sizes of the three groups in societies, such as Britain and the USA, where public policy does not much affect the distribution. In this case, the distribution of women across the three groups corresponds to a "normal" statistical distribution of responses to the family-work conflict.[3] In practice, in most societies, public policy is biased towards one group or another, by accident or by design, so that the exact percentages vary between modern societies, with inflated numbers of work-centered women or home-centered women. For example, Swedish fiscal and social policy appear to have squeezed home-centered women to a tiny group, and have inflated the size of the work-centered group, but the three groups are still identifiable in substantial numbers (see Table 3 on next page).

Work-centered women are a minority, despite the massive influx of women into higher education, and into professional and managerial occupations, in the last three decades. Work-centered people (men and women) are focused on competitive activities in the public sphere, in careers, sport, politics, or the arts. Family life is fitted around their work, and many of these careerist women remain childless, even when married. Qualifications and training are obtained as a career investment rather than as an insurance policy, as in the adaptive group. The majority of men are work-centered, compared to only a minority of women, even women in professional occupations (Hakim 1998, 221-34; 2003a, 183-4). Preference theory predicts that men will retain their dominance in the labor market, politics, and other competitive activities, because only a minority of women are prepared to prioritize their jobs (or other competitive activities) in the same way as men. In the long run, it is work-centered people who are most likely to become high achievers in demanding occupations (Hakim 2006).

Adaptive women prefer to combine employment and family work without giving a fixed priority to either. They want to enjoy the best of both worlds. They are generally the largest group among women, and are found in substantial numbers in most occupations. Certain occupations, such as teaching, are attractive to women because they facilitate a more even work-family balance. The great majority of women who transfer to part-time work after they have children are adaptive women, who seek to devote as much time and effort to their family work as to their paid jobs. In some countries, such as the USA and southern European countries, and in certain occupations, part-time jobs are still rare, so women must choose other types of job, if they work at all. For example seasonal jobs, temporary work, or school-term-time jobs all offer a better work-family balance than the typical full-time job, especially if commuting is also involved. When flexible jobs are not available, adaptive women may take ordinary full-time jobs, or else withdraw from paid employment temporar-

3 The distribution set out in Table 2 is based on an extensive review of the empirical evidence for the last two decades presented in Hakim (2000), and has been reconfirmed by subsequent national survey research in European countries (Hakim 2003a), in the USA (Hattery 2001, 170), and in other countries (Table 3).

Table 3: National Distributions of Lifestyle Preferences Among Women and Men

	Family centered	Adaptive	Work centered
Britain			
all women aged 16+	17	69	14
women in full-time work	14	62	24
women in part-time work	8	84	8
all men aged 16+	?	<48	52
men in full-time work	?	<50	50
men in part-time work	?	<66	34
Spain			
all women aged 18+	17	70	13
women in full-time work	4	63	33
women in part-time work	7	79	14
all men aged 18+	?	<60	40
men in full-time work	?	<56	44
Belgium-Flanders			
all women	10	75	15
women with partners	12	75	13
all men	2	23	75
men with partners	1	22	77
Germany			
women	14	65	21
men	33		67
Czech Republic			
all women aged 20-40	17	70	13
women in employment	14	69	17
wives aged 20-40	14	75	11
Sweden			
women in 1955 birth cohort: actual lifestyle choices by age 43 (1998)	4	64	32
Japan			
Ideal lifecourse of unmarried women			
1987	37	55	8
2002	21	69	10
2005	20	70	10

Source: Data for Britain and Spain, 1999, extracted from Tables 3.14 and 3.15 in Hakim (2003a, 85, 87). Data for Belgium-Flanders extracted from Corijn and Hakim (forthcoming) based on a 2002/3 survey. Data for Germany extracted from Bertram et al (2005). Data for Czech Republic from personal communication from Beatrice Manea, based on a November 2005 survey. Data for Sweden extracted from Huang et al (2007)reporting analysis of a longitudinal dataset. Data for Japan from National Institute of Population and Social Security regular surveys of Views of the Unmarried about Marriage and Family in Japan provided by Professor Akihide Inaba, Tokyo Metropolitan University.

ily. Adaptive women and men are the group most interested in schemes offering work-life balance and family-friendly employment benefits, and will gravitate towards careers, occupations and employers offering these advantages.

The third group, *home-centered or family-centered women*, is also a minority among women, and a relatively invisible one in the Western world, given the current political and media focus on working women and high achievers. Home-centered women prefer to give priority to private life and family life after they marry. They are most inclined to have larger families (Hakim 2003c), and these women avoid paid work after marriage unless the family is experiencing financial problems. They do not necessarily invest less in qualifications, because the educational system functions as a marriage marketplace as well as a training institution. Despite the elimination of the sex differential in educational attainment, an increasing percentage of wives in the USA and Europe marry a man with substantially better qualifications, and the likelihood of marrying a graduate spouse is hugely increased if the woman herself has obtained a degree (Hakim 2000, 193-222; Blossfeld and Timm 2003).[4] This may be why women remain less likely to choose vocational courses with a direct economic value, and are still more likely to take courses in the arts, humanities, or languages, which provide cultural capital but have lower earnings potential. This group of workers is most likely to drop out of demanding careers relatively early in adult life. Surveys suggest this group is tiny among men (see Table 3).

Given this diversity of lifestyle choices among women and men, and the fact that the three groups cut across ability and education levels, it is predictable that men will continue to predominate at the highest levels of any occupation, and the economics profession is no exception. In addition, modern economics values and rewards mathematical abilities.

INTELLECTUAL ABILITY ALONE IS NOT ENOUGH

Research by psychologists rounds out any explanation for the continuing predominance of men in the highest grades of most professions, long after equal-

4 Studies of 'self-service' marriage markets in modern societies show that most women are concerned to marry a man with equal or better education (and thus equal or better earnings potential), whereas most men place far less weight on this criterion in their choice of spouse. The majority of men with education beyond basic secondary education marry women with less education, because men give more weight to physical attractiveness (Hakim 2000, 193-222).

opportunities policies reconfigured expectations of the "proper" roles for men and women.

A study that was started in 1971 in the USA evolved into a 50-year longitudinal study of more than 5000 intellectually talented individuals identified over a 25 year period. The Study of Mathematically Precocious Youth selected youngsters from the top 3% on a conventional attainment test administered in secondary schools at around age 12-13. In practice, the study covers all intellectually precocious young people, whether verbally or mathematically talented. They were followed up for 35 years, well into adult life, with many followed up to age 47-48.

The results of the study (Lubinski and Benbow 2006; Park, Lubinski and Benbow 2007) show that even among exceptionally talented people entering the workforce after the equal opportunities revolution, there are still large sex differences in interests, values, and preferences. Using numerous measures of ability, sex differences in math and verbal ability were small or non-existent. However males had a greater "tilt" towards quantitative ability rather than verbal ability, and males scored higher than females on all measures of ability (Park, Lubinski and Benbow 2007, 951). Sex differences in income were explained entirely by hours worked. There were substantial differences in hours worked, and in the hours people would be willing to work in their *ideal* job, with many women choosing to be full-time homemakers or to work part-time only, despite apparently being highly qualified. Even within this exceptionally talented sample, ability level remained a predictor of earnings, achieving tenure at a top 50 USA university, and earning patents. The researchers note that, in all fields, notable accomplishments are rarely achieved by people who work 40 hours a week or less, and that world class performers often work 60-80 hours a week. They conclude that similarly able people will still make different lifestyle and career choices, and they note that all the sample were similarly satisfied with their current careers and life in general at age 33 (Lubinski and Benbow 2006, 334).

These results may reflect sex-role socialization as well as abilities and lifestyle preferences. However they suggest that sex-role differentiation is not eliminated by equal-opportunities policies regarding roles in public life. Even within this exceptionally talented group, men worked longer hours and had higher earnings than women, many of whom chose to be full-time mothers and homemakers.

Work-life balance versus careerist goals

Case studies of professions that employ equal numbers of men and women also reveal the limited impact of educational qualifications in isolation (Hakim, 1998, 221-234; 2003a, 183-4). Across modern societies, pharmacy now employs equal numbers of men and women, and also employs unusually high numbers of ethnic minority people. Due to chronic labor shortages, it is widely agreed that the profession is completely free of sex and race discrimination. Studies of the profession in the USA, Canada, Britain, and other European countries still show a large degree of *job* segregation within the occupation, however. Women gravitate towards jobs that are local, can be done part-time or for short periods, and to jobs with fixed hours of work that can be fitted around family life. Men in the profession gravitate towards ownership of independent pharmacies, which entail the long work hours and additional responsibilities of self-employment and running a small business. Other men work towards management jobs in the large retail chains, again accepting long hours, greater demands, more responsibility, and less flexibility. Given labor shortages and the absence of sex discrimination in the profession, women are free to choose whatever working arrangement they prefer. So sex differentials in outcomes reflect quite different priorities and life goals. In Britain, there is no earnings difference between full-time and part-time workers in the profession, but there is a large 27% earnings differential between women and men working full-time, close to the average pay gap for all fully integrated professions in Britain (Hakim 1998, 226-7). Case study research shows that these sex differentials in the professions are due to substantively different work orientations among men and women, even among university graduates (Hakim 2000; 2004a, 178-182), and hence to people choosing very different career paths. In practice, most women choose work-life balance, while most men focus more strongly on success in their career.

This is the reason why studies find women far less likely than men to seek and ask for promotion, responsibility, and pay rises (Babcock and Laschever 2003). Women ask instead for more convenient or shorter work hours, factors that improve work-life balance rather than maximize career success (Hakim, 2004a, 90-97; McGovern et al, 2008, 120-122).

Academic studies almost invariably omit to measure personal lifestyle preferences and values—as distinct from the acceptance of societal norms, which is widely covered by opinion polls and the like (Hakim 2003b; 2004b). So the impact of lifestyle preferences is routinely overlooked or attributed to other more easily measured factors, especially in studies seeking to explain the pay gap.

THE ASSUMED SUPERIORITY OF WESTERN WELFARE STATES

There seems to be no doubt that family-friendly policies are popular among many women, and make it much easier for them to combine paid jobs with family work. What is doubtful is that such policies produce gender equality in the workforce. The latest research evidence is that family-friendly policies do not make any major positive difference to gender equality in the labor market, as indicated by levels of occupational segregation, the pay gap, and the glass ceiling. On the contrary, they exacerbate these problems. This conclusion has now been drawn by several scholars working independently (Charles and Grusky 2004; Hakim 2004a; Jacobs and Gerson 2004). The research evidence suggests that it is unrealistic to expect that women could soon achieve half of the top jobs.

Cross-national comparative studies by the ILO, OECD, EC (Anker 1998; Melkas and Anker 1997, 1998; OECD 2002; European Commission 2002, 18-45), and by academic scholars (see the reviews in Charles 1998; Hakim 2004a, 170-182; Charles and Grusky 2004), have been indicating that some well-established assumptions are myth rather than fact. We now know that there is no direct link between occupational segregation and the pay gap; the association is coincidental rather than causal, and the two are independent social developments or constructions. Furthermore, economic and social development is not causally linked to occupational segregation or the pay gap; modern societies do not necessarily have better scores on these two indicators of gender equality in the workforce. The country with the lowest level of occupational segregation in the world is China, not Sweden, as we have been led to believe. Many countries in the Far East have lower levels of occupational segregation than in western Europe. The lowest pay gap in the world is not found in Sweden, as so many claim, but in Swaziland and Sri Lanka. Most important, higher levels of female employment in a society produce higher levels of occupational segregation and a larger pay gap; they do not improve gender equality in the workforce, as previously assumed, but worsen it. Even within western Europe, countries with the lowest female employment rates tend to have the smallest pay gaps, as illustrated by Portugal and Spain compared to Finland and Germany.

Even more disconcerting is the evidence that family-friendly policies generally *reduce* gender equality in the workforce, rather than raising it, as is so often assumed. This conclusion has now been drawn simultaneously by several scholars working independently (Charles and Grusky 2004, 5-6, 10-11, 37, 297, 302-4; Hakim 2004a, 183; Hunt 2002; Jacobs and Gerson 2004, 7, 177). In particular, Sweden's generous family-friendly policies have created a larger glass ceiling problem than exists in the USA, where there is a general lack of such policies (Albrecht, Björklund, Vroman 2003; Henrekson and Dreber 2004). Women are more likely to achieve senior management jobs in the USA than in Sweden: 11% versus 1.5% respectively (Rosenfeld and Kalleberg 1990; see also Wright, Baxter and Birkelund 1995; Henrekson and Dreber 2004). There is no doubt that family-friendly policies help women to combine paid jobs with family work. What they do not do is solve the problem of gender inequality in the workforce.

What these research results suggest is that Jonung and Ståhlberg's expectations are based on out-of-date and discredited assumptions about the impact of social engineering in Sweden. As noted earlier, their own research indicates that the institutional context is far less important than social policy experts would have us believe.

NEW THEORIES FOR THE 21ST CENTURY

Pulling all these threads together, we have a complete explanation for continuing male dominance in the professions—including economics—in the absence of sex discrimination. At the highest echelons, ability alone is not enough. Long hours of work, motivation, and a strong career focus also count heavily. Even among the most able and talented, we find that tastes, values, and lifestyle preferences differ. Women are more likely to choose work-life balance, while men are more likely to value career success. Jonung and Ståhlberg claim the gender discrepancy in career attainment is largest in economics. The gender gap is likely to be even larger in engineering and other male-dominated disciplines that prioritize mathematical skills. In Britain, decades of effort to push more women into science and engineering courses and occupations have had little success, and the gender gap remains large at all levels. Similarly, in the USA, women comprise only 10% of tenure-track professors in electrical engineering and similar subjects (Sommers 2008). In all countries, the most enduring segregation of men and women is in the educational system, long before people enter the labor market, as women continue to prefer courses in the arts, humanities and social sciences while men are more likely to choose courses in maths, science and engineering. Sex differences in tastes emerge early and are resistant to attempts to impose politically correct choices because sexism is no longer the dominant factor in young people's lives.

Like engineering, economics puts a premium on quantitative and mathematical skills, and hence attracts more men than women into the profession. In Britain (and most countries) economics graduates are mostly male, while sociology graduates are mostly female.

It is time to accept that the equal opportunities revolution has served its purpose, and the feminist goal of 50/50 sex ratios in all occupations and jobs is unrealistic, given the diversity of tastes, values, and preferences among men and women. Social engineering attempts to impose identical outcomes and eliminate occupational segregation completely cannot succeed, being based on selective research evidence and incorrect assumptions. My prediction that men will continue to dominate in most occupations and the highest grade positions is not appealing, even to me, but it is based on the research evidence and is realistic. Social science is about understanding the real world, not about reinforcing fantasies and wishful thinking.

Jonung and Ståhlberg's metaphor of the "leaking pipe" is not appropriate, and they fail to appreciate that their own careerist values are not shared by all adult women. The more appropriate perspective is the 'competing values' framework of preference theory. Social scientists have to learn to recognize, and support diversity in values, world views, and lifestyles, most especially in multi-cultural societies.

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Go to *Gender and Economics* Symposium Page

Go to May 2008 Table of Contents with links to articles

Go to Archive of **Investigating the Apparatus** Section

Econ Journal Watch,
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May 2008, pp 219-226.

Symposium: Gender and Economics

Preferences Underlying Women's Choices in Academic Economics

John A. Johnson[1]

A comment on: Christina Jonung and Ann-Charlotte Ståhlberg, "Reaching the Top: On Gender Balance in the Economics Profession," *Econ Journal Watch* 5(2), May 2008: 174-192. Link.

Abstract

Jonung and Ståhlberg document a peculiar underrepresentation of women in academic economics, especially at the level of full professor. What is peculiar about the underrepresentation of women in economics is that no gender disparity of this magnitude exists in other behavioral sciences. In my own discipline, psychology, nearly 72% of new PhD and PsyDs in psychology are women (Cynkar 2007), and 39% of psychology faculty are female (Long 2001). In contrast, Jonung and Ståhlberg point out that the percentage of female economics PhD students in both my country of the United States and Sweden is only 32% and the number of female economics faculty in both countries is only 16%. Similar percentages are found in Great Britain, Canada, and Australia. They document comparable differences in gender disparity at the rank of full professor. In the U.S., 25% of all full professors in psychology are women (Cynkar 2007), whereas only 8.7% of full professors in economics at the top 50 US universities are female (Nelson 2004). Depending on the source one consults, the percentage of female full professors in economics in Sweden is somewhere between 1% and 6%. Why the gender disparities in economics are so much more dramatic than in other fields is an intriguing question, one that I am pleased to be able to explore with Jonung and Ståhlberg.

Jonung and Ståhlberg's intentions of course go far beyond documenting the underrepresentation of women in economics. They also hope to encourage further research and discussion on this topic in order to increase the number of women in economics and the number of women attaining the rank of full professor in econom-

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ics. They provide two reasons for wanting to do this. One is that women are missing out on an exceptionally rewarding career: "Our interest in the issue of the presence of women in academic economics is based not only on the opinion that women should be able to partake in the gourmet meal" (175). The second is that women have a unique perspective that will improve the field of economics: "We also believe that, if more economists are women, economic analysis will be richer, and if more women are familiar with economic reasoning, public debate will be stronger and deeper" (175).

Naturally, our ability to increase the number of women in the field of economics depends on an understanding of the factors that inhibit the entry of women into this field. Jonung and Ståhlberg list four factors that might explain both the overall lack of women in the field and the relatively small number of women at the top positions in economics: (1) discrimination in the form of bias in recruiting and promoting women; (2) preferences unique to women that interfere with career advancement in economics; (3) social institutions and policies that create different incentives for men and women; and (4) cultural rules and values in the profession of economics that set a tone unwelcoming to women. Among these possibilities, Jonung and Ståhlberg are quick to dismiss gender differences in preferences: "Tastes and preferences, talents and capabilities may be part of the story but economists generally stress human capital investments guided by expectations about future labor market participation" (182). I believe that dismissing tastes and preferences just because economists have traditionally ignored such psychological variables until recently is a mistake. I would like to argue that preferences constitute an important explanation for the dearth of women in economics.

In addition to making a case for the explanatory importance of preferences, I would also like to call for a closer analysis of what constitutes underrepresentation versus appropriate representation of women in economics generally and female economists at the rank of full professor, specifically. Although the number of women pursuing a PhD in economics obviously bears, eventually, on the number of women attaining the rank of full professor, I think these two phenomena should be examined separately.

Finally, I think we need to consider carefully the two major reasons for trying to increase the number of women in economics—it is only fair and right for more women to be able to "partake in the gourmet meal" and that a feminine perspective will enrich, strengthen, and deepen economic analysis and public debate. I will explain how these may or may not be good reasons for increasing the number of women in economics.

Occupational Preferences: Is Economics a Gourmet Meal for Everyone?

In psychology we have an entire subfield, vocational psychology, founded on the premise that individuals find different kinds of activities intrinsically enjoyable. Differences in preferences for particular kinds of activities incline individuals toward different careers. Actual occupational choices are of course constrained by factors studied by economists such as the availability of different kinds of work in the job market and incentives such as salary and health care benefits. Nonetheless, preferences for particular kinds of activities represent a strong motivating force that drives occupational choices, sometimes even in the face of bleak employment opportunities and poor financial compensation. Many a starving artist will tell you that he or she is not in it for the money.

The dominant model of vocational preferences today is John Holland's (1959, 1997) RIASEC model. Fifty years of research on Holland's model has supported the utility of conceptualizing human personalities and work environments in terms of their resemblance to six prototypical categories: Realistic, Investigative, Artistic, Social, Enterprising, and Conventional. Researchers who employ the Holland model typically refer to each vocational-personality type by the first letter of the type label; hence the acronym RIASEC. Of particular interest to the question of women in economics is the RIASEC classification for an economist and research on gender differences in RIASEC preferences.

Economists, like research psychologists and other scientists, are considered to be primarily Investigative. Investigative individuals like working with ideas more than dealing with people. They do not mind laboring long hours in relative isolation. They also exemplify a cognitive style that Welsh (1975) termed *high Intellectence*. High-Intellectent Investigative persons disengage and distance themselves from the sensate world, preferring to relate to the environment indirectly through abstract symbols (Johnson 1994). Investigative individuals are therefore comfortable with abstract, mathematical representations of reality and enjoy theoretical research and puzzle-solving.

Psychological research has consistently demonstrated that males, as a group, score higher than females on measures of Investigative, Realistic, and Enterprising vocational preferences, while the reverse is true for Social and Artistic preferences (Browne 2006). At the same time, psychologists have also long recognized the existence of considerable individual differences *within* each sex-typical set of interests. Although women are generally more interested in occupations involving working with people (the Holland Social occupations), a certain number of women will be interested in more impersonal kinds of work, including Investigative careers (Lippa 1998). An important question is whether preferences for activities in the Investigative versus Social domains is simply a function of encouragement from parents, teachers, and role models, or whether these preferences are an inherent part of the nature of most men and women.

At one point in my career I was involved in a Johns Hopkins study of over 700 women who returned to college after being out of school for several years (Johnson 1980; Richmond & Lisansky 1984). At the time of enrollment, 37% of the women were unemployed or employed in unskilled jobs. Of the remainder, 90% were employed in traditionally female occupations in the Social, Conventional, and Artistic Holland categories, while only 10% were in nontraditional Realistic, Investigative, and Enterprising occupations. The career *aspirations* of women in this sample was heavily skewed toward the Social occupations (70%). Overall, 78% of the group reported aspiring toward traditional careers for women. Thus, when they began college, the percentage of the women aspiring to nontraditional careers was only slightly higher (22%) than the percentage of women working in nontraditional occupations (10%) prior to college. The increase in nontraditionality was mostly due to the number of women who aspired toward Enterprising careers. The study was, in part, a social experiment, as our research group had hoped to persuade a significant proportion of women to consider nontraditional careers. Half of the women received career counseling that included the agenda of exploring nontraditional careers, and half did not.

The presence or absence of counseling had no impact on the traditionality of the occupations actually attained after college. The percentages of the sample finding employment after college in different areas were as follows: Social (48%), Conventional (20%) Enterprising (15%), Artistic (2%), Investigative (1%) and Realistic (.5%). Oddly, the 13% of the sample who were unemployed at the time of follow-up actually indicated the highest rating of satisfaction with their current occupational situation. They were followed by women in Enterprising, Social, Artistic, and Investigative jobs (each about 3.25 on a four-point scale), women in Conventional jobs (2.95), with women in Realistic occupations at the bottom (2.25). Thus, neither encouragement to consider nontraditional careers nor the college experience itself was able to recruit more than 18% of the sample into nontraditional Enterprising, Investigative, or Realistic career tracks. Only 1% of the women obtained jobs in the sciences.

One might look at the failure of the Johns Hopkins project to encourage more women into entering nontraditional occupations as a problem of the historical time. The project was conducted at the end of the 1970s, so perhaps there were still too many cultural barriers and occupational stereotypes for women to consider nontraditional careers. Jonung and Ståhlberg note the steady increase of economics PhD completion for women in Sweden from 7-9% in the 1970s and 80s to around 17-18% in the 1990s to 26% in the early 2000s. They astutely dismiss the notion that the influx of women into economics and the rising percentages of PhD completion from the 1970s through the 1990s could be due to an increase in women's capacities for abstract and analytical thinking. Rather, they attribute the increase to changes in the labor market. Nonetheless, they also observe that the female share of new PhDs seems to have "plateaued since the late 1990s" (181). The question is whether it is reasonable to assume that the proportion of women in economics can reach parity (50%), or whether it is more likely that there is an upper limit (perhaps 30%?) to the number of women we can expect to see in academic economics. Browne (2006) presents reasons why the latter might be the more reasonable conjecture.

Browne (2006) notes that there are essentially two competing explanations concerning differential participation by men and women in different occupational fields. One he calls the "purely social view" (151), which attributes differences wholly to culture and claims that biology plays no role or a trivial role in behavioral differences between men and women. The other is the view that both biology and culture play important roles. The purely social view assumes no limits on the proportion of women or men becoming employed in any occupation. With proper encouragement and the removal of cultural barriers, the purely social view would suggest that, in principle, it is possible for women to obtain 50% or even 100% of the positions in academic economics. In contrast, the mixed biological-cultural view holds that any changes in behavioral differences between the sexes will be constrained by biological differences. Browne presents the following argument for the mixed view over the purely cultural view.

The purely cultural view has attributed the lack of participation of women in the sciences to an educational system that discourages girls from taking courses in math and science and hostility within the science professions toward women. Yet the evidence contradicts the hypothesis that girls are discouraged by parents, teachers, and peers from taking math and science courses. In fact, in the U.S., girls and boys take roughly the same number of math and science courses, and high school girls earn higher grades in math and science than boys. Girls are *less* likely than boys to report lack of attention from teachers about science, and female college students are *more* likely than male students to indicate choosing science because of encouragement from parents and teachers, whereas boys report pursuing science due to an interest in the subject (Browne 2006).

If there is hostility in the sciences toward accepting women, Browne (2006) notes that it takes an odd, selective form by subfield. Women earn relatively few doctorates in mining/mineral engineering, but considerably more in bioengineering. Biology is apparently welcoming to women, as women earn 45% of all doctorates in biology. So is medicine, as over 40% of new doctors are women. But biophysics must be hostile, because relatively few women earn a doctorate in this area. Women earn 67% of all doctorates in psychology, but mostly in developmental and child psychology. Few women earn doctorates in psychometrics and quantitative psychology. Finally, Browne specifically mentions the low percentage of PhDs earned by women in economics, compared to the number of PhDs in sociology and anthropology (14%, 36%, and 41% in 1995, according to Long 2001).

The common denominator for fields in which women are scarce, Browne (2006) observes, is that they "tend have the lowest social dimension, while those attracting larger numbers of women tend to have a higher social dimension" (150). Lippa (1998) found that both male-female differences in vocational preferences and differences in preferences for typically male or female activities *within* each sex are powerfully associated with a People-Things dimension of vocational interests, a dimension that accounts for significant variance in the RIASEC model. Women, as a group, prefer occupations involving people. However, due to individual differences in the masculinizing effects of prenatal hormones, some girls show preferences and cognitive traits that are more typical for boys (Browne 2006). They prefer boy toys and rough-and-tumble play activities and show superior spatial abilities. As adults, they are more likely to prefer typically male occupations.

In their discussion of the "leaky pipeline" in academic economics, Jonung and Ståhlberg (177) suggest that the most profound leak is the very first semester. The proportion of women students in undergraduate economics programs at that point is nearly 50%. But after the first semester, a significant number of women leave the program, many opting for business administration. This strikes me as prima facie evidence that these women, after tasting an economic meal, did not consider it to be gourmet quality. The people-oriented field of business administration seemed much more to their liking. As a psychologist, I can empathize with that decision. While mathematics is indispensable to any science, I have read too many economics working papers that have struck me as bloodless formalism bordering on mathematical fetishism. An appropriate goal, it seems to me, would not be to have absolute parity (50% female, 50% male) in academic economics. Rather, we should identify all women with strong Investigative interests (however many there may be) and encourage them to consider a career in academic economics.

What Could be the Unique Contribution of Women to Economics?

The second reason that Jonung and Ståhlberg give for increasing the number of women in economics is that there is something special about women that would enhance the field of economics. Surely they cannot mean that women have special perceptual and cognitive abilities that allow them to discover, through basic research, principles of economics that have eluded men. I cannot imagine a similar argument being used to recruit more women into physics and chemistry—that women have special research abilities that allow them to discern natural laws that men cannot comprehend. I assume that Jonung and Ståhlberg are referring more to the realm of economic decision making and policy setting, and here I think they have a good point. I'll explain by delving a little deeper into the application of Holland's RIASEC model to economics.

Holland's RIASEC classifications of occupations go beyond a simple mapping of each occupation onto one of the six types. In addition to the type of primary resemblance, Holland's "three-letter codes" also designate a secondary and tertiary resemblance to the remaining vocational prototypes. For economists, this is important for distinguishing different types of careers in economics. A professor of economics is classified as an IAS type. The secondary resemblance to Artistic indicates the importance of symbolic ideation in creative research to professors of economics, and the tertiary link to Social shows an interest in social problems and in teaching students. In contrast, an applied economist who consults with businesses and agencies, recommending policies and plans, is classified as an IEC type. The secondary Enterprising aspect shows involvement in real-world decision making in business, and the tertiary Conventional feature indicates comfort with working with large arrays of numerical data.

As a group, women surpass men in verbal skills and interpersonal competencies (Browne 2006). These abilities may indeed allow them to make uniquely valuable contributions to public discussions and decisions about economic policy. I am unfamiliar with the undergraduate program in Sweden, but I get the sense that the initial courses in economics are being taught as though all of the students are IAS Holland types who are on a track to become academics. If so, it would be no wonder that so many female students leave for Business Administration (Holland code: ESC). This is reminiscent of the attraction of Enterprising careers over other nontraditional careers for women in the Hopkins study. Careers with an Enterprising component are highly people-oriented, and may therefore be more attractive to women. If there is room in the Swedish economics curriculum for applied economists, and Swedish economics departments are interested in retaining more women in the major after the first semester, my recommendation would be to revise the first semester offerings to make them more people-oriented and therefore more palatable to students who are more inclined toward an applied career.

WHAT ARE THE OBSTACLES TO PROMOTION IN ACADEMIC ECONOMICS?

The question I find most troubling in the Jonung and Ståhlberg paper is whether women professors are being unfairly denied promotion to full professor. As someone who has served on promotion and tenure committees for decades, I must say that discrimination does occasionally occur, despite institutional safeguards to ensure fairness. Discrimination in economics departments may therefore be an impediment to promotion for women, but the way in which Jonung and Ståhlberg present percentages of men and women at different ranks makes it difficult to evaluate whether there is a pattern that might indicate discrimination. When they say that only 1% of all full professors of economics in Sweden are women (according to Statistics Sweden) or only 6% are women (according to their own data), this says nothing about the relative promotion rates of men and women. What we need to do is first rearrange their data to examine the relative proportion of professors at the assistant, associate, and full professor level for each sex separately. When we do, we find that those proportions (using Jonung and Ståhlberg's data) are 42%, 22%, and 36% for men and 54%, 33%, and 13% for women. One does not have to actually compute the chi-square to see that a significantly greater proportion of men are at the full professor rank. This does not demonstrate discrimination, but does not rule it out, either.

We can reasonably speculate that one reason for a greater proportion of men at the full professor rank is that more men than women have been in the system for a long time. Prior to 1970, only two women received PhDs in economics in Sweden. In the 1970s, 93% of all PhDs in economics were awarded to men. Men have therefore had an enormous head start on working toward the rank of full professor. It will be interesting to see how quickly the current assistant and associate professors (both male and female) advance as the 1940s birth cohort retires. If women do not advance as quickly when these positions open up, we might want to look harder for the reasons. Research cited by Jonung and Ståhlberg indicates that the previously slow progress of women in promotion to full professor cannot be readily explained in terms of family factors. For all but one index, publication rates are about the same for men and women. It is possible that devoting more attention to teaching and administrative work has distracted women from doing the research that is necessary for promotion, but it is not clear whether women have been pressured into these roles or whether it is because female professors are more person-oriented. I, for one, will be keeping an eye on gender trends in promotion to full professor of economics over the next 10 years.

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Go to *Gender and Economics* Symposium Page

Go to May 2008 Table of Contents with links to articles

Go to Archive of **Investigating the Apparatus** Section

Econ Journal Watch,
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SYMPOSIUM: GENDER AND ECONOMICS

What is the Right Number of Women? Hints and Puzzles from Cognitive Ability Research

GARETT JONES[1]

A COMMENT ON: CHRISTINA JONUNG AND ANN-CHARLOTTE STÅHLBERG, "REACHING THE TOP: ON GENDER BALANCE IN THE ECONOMICS PROFESSION," *ECON JOURNAL WATCH* 5(2), MAY 2008: 174-192. LINK.

ABSTRACT

> There is no consensus as to the causes of women's slow advancement in academic economics. Even after adjusting for factors representing family background or productivity a considerable portion of the gender promotion gap remains unexplained. In addition, the search for explanations has to consider the exceptionality of economics."
>
> –Jonung and Ståhlberg (2008, 188)

HERE I FOCUS ON THE POSSIBILITY THAT THE LOW REPRESENTATION OF women in economics is partially driven by genetic differences in tastes and abilities between the sexes, differences that may show up in both means and variances. Particularly in a field like academia, where essentially all employees are above the mean in abilities, variances are likely to be important. I'll review some of the recent findings regarding the matter, some of which are more recent than the Larry Summers controversy. Some useful surveys include Munger (2007), Allen and Gorski (2002), Zup and Forger(2002), Pinker (2002), and especially Hyde (2005) and Cahill (2006); the most prominent rebuttal of the views expressed by those authors is Spelke (2005). Although there is no precise information at the genetic level, the combination of analogies from other mammals, early childhood

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studies, well-documented impacts of sex hormones on brain structure, and the repeated finding of higher means and variances in relevant mental abilities (especially mathematical abilities) in males point toward the very real possibility that men and women differ genetically on average with regard to the advanced skills useful in graduate economics as the field actually exists today.

Let g^* denote the ideal ratio of female economists/total economists, something akin to the ratio that would exist in a competitive academic market in the absence of discrimination, affirmative action, and cultural barriers to the advancement of women. The important thing is the idea of our *prior distribution* about g^*. Denote $f(g^*)$ as this prior distribution about g^*. My goal in this short comment is to help update your priors about g^*, in the hopes that you, the reader, will place substantial mass to the left of $g^*=50\%$. Indeed, I hope that by the end of this comment, readers will place some weight to the left of $\hat{g}$, the current fraction of female economists across the rich countries, since affirmative action at both public and private universities likely increases the number of female economists above what it otherwise would be. If enough of us place substantial mass of $f(g^*)$ to the left of 50% and even $\hat{g}$, discussions about the issue will be broader and more tolerant.

EVOLUTION AS A REASON FOR SOFT PRIORS

Perhaps the strongest argument that there should be some mass of $f(g^*)$ to the left of 50% comes from the theory of evolution. Adaptationism—the concept that gene-carriers quickly adapt to their surrounding circumstances—is at the heart of the modern theory of evolution, and it is difficult to imagine that male and female humans have faced identical circumstances across the millennia. Most obviously, men and women have faced systematically different challenges, framed by the nature of the reproductive cycle. Determining exactly what those challenges are, and how they would change the incentives for brain development is an exciting, ongoing research agenda, and there are few solid answers at this point. Indeed, any of the standard popular references on the topic of evolutionary psychology would work quite well for laying out the verbal "just-so-stories" that frame the literature.

Once one accepts the adaptationist worldview, one accepts that evolution has no teleology of biological gender equality. Indeed, when an economist like Brad DeLong (2005) cleanly lays out the terrible dilemma facing women in academia, he inadvertently lays out an evolutionary dilemma as well. In discussing the Larry Summers controversy, he notes:

> The process of climbing to the top of the professoriate is structured as a tournament, in which the big prizes go to those willing to work the hardest and the smartest from their mid-twenties to their late thirties.

> Given our society (and our biology), a man can enter this tournament without foreclosing many life possibilities [since he can more easily intertemporally substitute fatherhood] ….But given our society (and our biology), a woman cannot enter this particular academic tournament without running substantial risks of foreclosing many life possibilities if she decides to postpone her family, and a woman cannot enter this particular academic tournament without feeling—and being—at a severe work intensity-related handicap if she does not postpone her family.

So women and men face different tradeoffs. More broadly, men face a greater expected payoff to taking big risks in the early parts of their life, and, empirically, men *are* more likely to engage in risky behavior than women. For men and their genes, there is almost always another day. For women, the trade-off is much crueler. DeLong's economic model is implicitly evolutionary. Different trade-offs across the timeframe of evolution for men and women lead to different genetic results, some of which are *a priori* likely to have an impact on the sexual differences within the brain.

But we have more than just theory to bring to bear. There are some useful facts about male-female differences in the human genome. Drawing on a recent line of research into the male-specific Y-chromosome (only sequenced in 2003 (Skaletsky, et al. 2003)), the *New York Times* reports:

> Men and women differ by 1 to 2 percent of their genomes, Dr. [David] Page said, which is the same as the difference between a man and a male chimpanzee or between a woman and a female chimpanzee….'We all recite the mantra that we are 99 percent identical and take political comfort in it,' Dr. Page said. 'But the reality is that the genetic difference between males and females absolutely dwarfs all other differences in the human genome.' (Wade 2003)

A final genetic note: The fact that men have only one X-chromosome is a fact too large to omit. A woman has two X chromosomes, so if a particular gene is non-functioning on one X chromosome, then she is very likely to have a functioning copy on her second X-chromosome. A man, by contrast, is in no such luck. A broken X-gene means no function. An entire field of male genetic abnormalities, "X-linked recessives," is the result of this absence. Again, we have no knowledge of brain functions on the X, but then we have little knowledge of what the X does in any case.

Stepping back, it appears that we don't know much about the precise genetic differences between the sexes—at least at the level of gene coding—but we do know that many exist. We know nothing about genetically-driven sex differences in normally-functioning brains—indeed, we know little about the genetics of brains in general. New knowledge is arriving rapidly, so answers are likely to arrive in the coming decades. In the meantime, our "just-so stories" are our best source of intuition, and they indicate that men are more likely to take big risks.

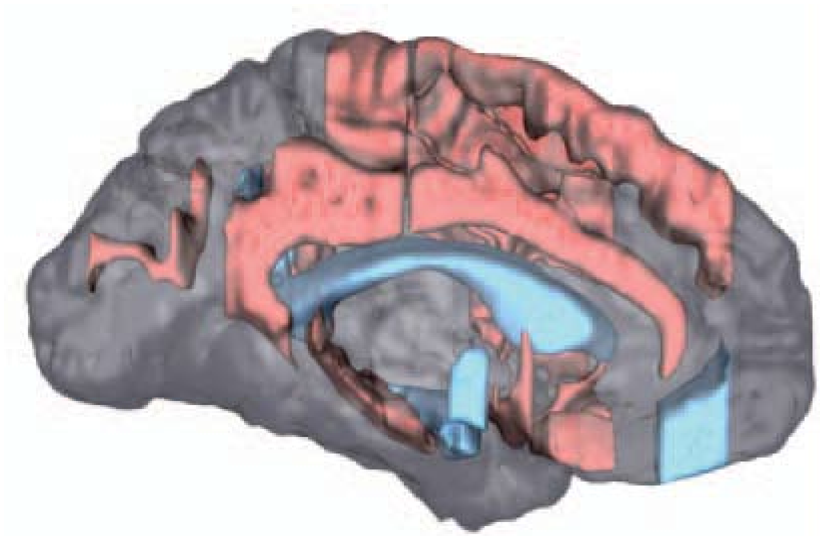
Brain Anatomy and Evidence of Sexual Differentiation

Humans and other mammals have sexually differentiated brains. As one might expect, the differences are rarely overwhelming. The best-understood channels involve readily-measurable differences in sex hormones, since lab biologists, like empirical economists, have a tendency to focus on the measurable and manipulable. The findings of Allen and Gorski (2002, 291) appear to sum up the consensus on hormones: "With respect to mammals, high levels of sex hormones—whether secreted by the testes or administered by a scientist—result in masculine brain development." That there *is* such a thing as masculine brain development, then, is the first, relatively minor point: Both tests on non-human mammals, tests on adult humans, and genetic abnormalities reinforce this view.

But do these hormonal differences drive functional differences? Halpern (2000, 180) points to a sizable area of research indicating that there appears to be in each sex a different optimizing point for one particular sex hormone, estriadol, a derivative of testosterone: "There are many studies in which low testosterone for males and high testosterone for females are associated with better performance on several different spatial tests" (171). Kimura (1999, 122) concludes that "the 'optimal' level of T[estosterone] for spatial ability in humans is that of the normal male with lower levels." Finally, when older men and older women have received hormone replacement therapy, or when people receive hormone therapy as part of a sex change operation, the "expected cognitive changes occurred" (Kimura 1999, 122). Thus, relatively well-understood hormonal differences appear to explain some of the differences in average spatial abilities between men and women. Economists use these spatial abilities in geometric and topological reasoning, so these differences may help explain why $\hat{g}$, the fraction of economists who are female, is below 50%.

Moving from hormonal differences, we can turn to differences in gross anatomy of the brain. The best-documented sexual dimorphism in mammals is in the pre-optic area of the hypothalamus, located just in front of the brain stem. This is about twice as big in human males as in human females—a difference visible to the naked eye—and is involved with reproductive behavior. Little else is known right now about the pre-optic area's precise functions, but it at least lets us know that brain anatomy is on the side of "some difference between the sexes." The hippocampus, a site related to memory and spatial organization, also differs between the sexes (Cahill 2006); it is larger in human females when adjusted for brain size—a relatively recent finding. The finding is unsurprising since women typically do better on tests of memory retrieval and spatial memory.

Figure 1. Differences in Brain Structure



Note: Pink regions indicate areas that are larger in a typical female brain, after adjusting for size differences in cerebellum. Blue regions indicate same for typical male brain. Reprinted from Cahill (2006), based on research in Goldstein, et al., (2001). (Sample size: 27 males, 21 females)

So while women typically perform worse on spatial *rotation* tasks, such as what the letter "F" looks like when rotated in three dimensions, they do better at spatial *memory* tasks, such as where she put the car keys. The typical "just-so story" invoked at this point is that males needed spatial rotation skills to hunt effectively, while females needed spatial memory skills to remember where useful plants were located.

Another well known fact regarding human brain anatomy is that men's brains weigh about 15 percent more than women's. While modern MRI scans indicate that within a given sex there *is* a positive correlation between brain size and IQ score (correlations of 0.3 to 0.4 are common), there is less evidence that men and women differ on average overall intelligence.

In the neuroscience literature, it's commonly observed that women's brains are "more balanced" or "better connected" between left and right hemispheres. Three separate connections—the corpus collusum, the massa intermedia, and the anterior commissure—are often found to be larger in women than in men (Allen and Gorski 2002; Kimura 1999, 132ff.). The evidence on the corpus collosum is more mixed than for the other two, but overall, the evidence appears to point to women having better lateral connectivity on average. Hearing and vision tests from the left and right sides likewise support the hypothesis that women's hearing and vision skills are better balanced between left and right (Kimura 1999, 135ff.).

Looking from front to back rather than from left to right, Figure 1 (Cahill

2006) tells much of the story: Women's and men's brains differ on average. Interestingly, this sample shows a *larger* corpus collosum for men—that's the apostrophe-shaped blue blob in the center of the brain. Though some of those brain differences may be environmental and social in origin—it would be surprising if it were otherwise—the impacts of fetal hormones on brain development are clear enough that there is little debate in the literature over whether *some* structural differences between men's and women's brains are genetically driven.

And not only do shapes and sizes differ between the sexes: functional MRI scans show that male and female brains consistently use *different* structures to solve the *same* kinds of problems:

> 'Every time you do a functional MRI on any test, different parts of the brain light up in men and women,' says Florence Haseltine, a reproductive endocrinologist at the National Institute of Child Health and Human Development (NICHD) in Bethesda, Maryland. 'It's clear there are big differences.' (Holden, 2005; see also Halpern 2000, c. 5)

TEST SCORES AS AN INDICATOR OF MENTAL ABILITY

I began by discussing data that are simultaneously the most unassailable and the least relevant: Sex differences in the human genome, driven by natural selection. These genetic differences are large, but we have essentially no empirics connecting them to differences in practical brain function. Instead, we have just-so stories about the different incentives faced by potential mothers and fathers across the ages. I then briefly discussed differences in brain anatomy and hormonal function. Some (but not all) of these differences are unambiguously genetic in origin, and the hormonal differences in particular appear to cause some differences in spatial abilities.

Now, we look at test score differences between men and women. These data are the most relevant to the question at hand—whether men and women differ in the abilities needed in actually existing economics—but they have the weakest ties to a clear genetic story.

First, to the question of overall intelligence. A common observation is that men have greater variability than women. Halpern (2000, 86) notes, "When we turn our attention to cognitive abilities researchers regularly (but not always) report that males are more variable than females." For instance, Feingold (1993, 74) reanalyzed a variety of national and state-wide intelligence and achievement-type tests:

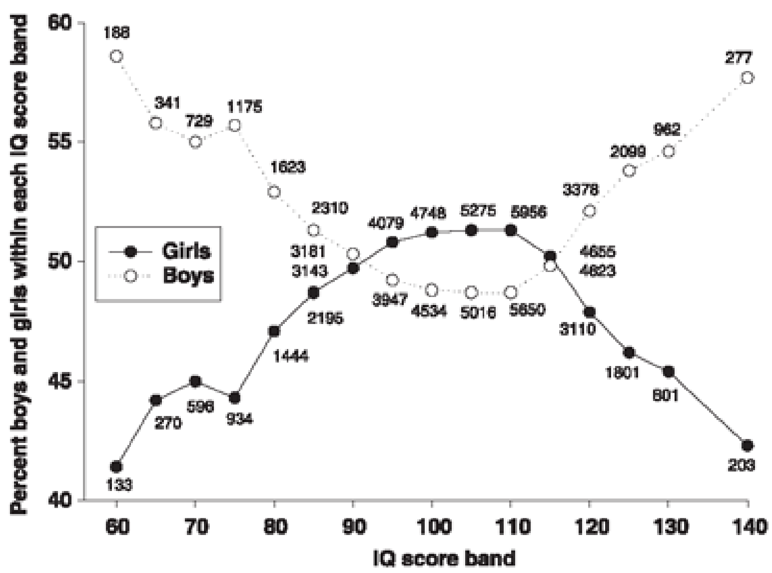
> It was consistently found that males were more variable than females in general knowledge, mechanical reasoning, quantitative

ability, spatial visualization, and spelling. There was essentially homogeneity of variance for most verbal tests, short term memory, abstract reasoning, and perceptual speed.

The high math variances are most relevant: On the SAT-Math, Feingold found that male variances were 20-25% larger for males in the four decades before his study, while on SAT-Verbal scores, male variances were about 5% higher. Of course, this could be driven by sample selection if men faced a *much* lower SAT threshold, something relatively unlikely by the 1980s. Even on the WAIS-R standardization sample (an explicitly representative sample designed to create norms for IQ scores), male variance averaged 8% higher across subtests.

But of course, one always wonders whether samples are really representative. One paper that addresses this issue is Deary et al. (2003): In a sample of 95% of the Scottish 11-year-olds in the 1932, covering 81,000 students, girls scored 1/90 of a standard deviation higher than boys on a set of IQ tests, but boys had a standard deviation of IQ that was 5% higher (corresponding to variances roughly 10% higher for boys). Thus, boys were overrepresented at both the top and the bottom of the distribution. Even such small differences can create quantitatively significant difference three or four standard deviations above the mean: At three standard deviations above the mean and with these values (5% higher variance,

Figure 2: IQ Scores for Scottish Boys and Girls in 1932



Note: "Numbers and percentages of boys and girls found within each IQ score band of the Scottish population born in 1921 and tested in the Scottish Mental Survey in 1932 at age 11. The y axis represents the percentage of each sex in each 5-point band of IQ scores. Numbers beside each point represent the absolute numbers of boys and girls in each 5-point IQ score band." Reprinted from Deary et al (2003).

1.1% higher standard deviation), we expect to find 50% more boys than girls, while at four standard deviations, we would expect to see twice as many boys. The small mean difference has little impact on this ratio—the effect comes mostly from the difference in standard deviations. Clearly, this isn't enough to explain the overwhelming predominance of men in the sciences, but it reinforces the widespread observation that males appear to be slightly higher in variance, even on a typical IQ test.

Now, I turn to the ability that is likely most relevant to the economics profession as it currently exists: Mathematical abilities. Indeed, as Jonung and Ståhlberg state in their abstract, when it comes to the actual representation of women within the field today, "[W]e find economics to be more akin to mathematics than to the other social sciences." The usual stereotype drawn from the psychological literature is that men are better at math and visuospatial skills than women, especially at the upper end of the distribution. The crucial caveats to this generalization are that women are consistently better (on average) at arithmetic and computation than men, and women are better at spatial memory, while men are consistently better (again, on average) at spatial rotation (Kimura 1999, Halpern 2000, and indirect support from Spelke 2005).

The fact that women are better at computation is especially intriguing in light of recent changes in the accounting profession: In a field that was formerly male-dominated, more than half of all Bachelor's degrees in accounting are now conferred on women (Koretz 1997, Briggs 2007). The ability of women to make great strides in a traditionally math-heavy field like accounting should caution against sweeping statements about g*, the ideal gender balance in economics. Even if the computation/spatial rotation difference continues to hold for centuries to come, future technological change could raise the relative value of computation or other skills useful in some future version of economics. "Skill-biased technological change" is apparently a reality, a reality that should feed into any discussion of women in academia. Could teacher bias be driving these results? That's unlikely, according to Kimura (1999): She notes that boys do better on math aptitude tests (with the exception of girls' superior computation ability), while girls do better on math achievement tests. By way of explanation, Kimura notes (78):

> Since both aspects of math are taught by the same person, teacher-related factors are unlikely to be the explanation. Nor do other 'socialization' explanations such as gender bias in problem content, math anxiety, parental expectation, and so on, adequately account for the differences.

And it turns out that psychologists indeed *have* addressed the possibility that their tests are biased: They've gone out of their way to write word problems that favor females (e.g., "Martha is making square cookies," Kimura, 1999, 77) but males still perform better on female-biased spatial rotation tests.

One source of evidence on the question of male-female differences is neurological disorders. Many such disorders are more common among men than among women; one that deserves particular attention is autism. Simon Baron-Cohen and his coauthors (2004, 2005) have theorized that autism is largely an "extreme male mind," one that focuses too much on regularizing and systematizing data, and that thus is unable to see the forest for the trees. In recent work, Baron-Cohen provides neuroanatomical evidence for his hypothesis. Since autism shows up at such a young age, it would be remarkable if this sex difference in autism (on the order of 3:1) were driven entirely by environmental differences. The predominance of autism among males, like the higher levels of Tay-Sachs among Ashkenazi Jews, may turn out to be largely driven by extreme cases of otherwise normal brain function within each particular subgroup.

Another source of data is meta-studies by psychologists. In a survey of meta-studies entitled "The Gender Similarities Hypothesis," Hyde (2005) collected dozens of meta-studies of gender differences in cognitive abilities and personality traits. Among her findings is that on tests of mental rotation, spatial visualization, and spatial perception, males consistently perform better than females, with a median estimate of 0.44 standard deviations above females. Female advantages on tests of verbal fluency, language, and spelling are of the same order of magnitude. Males are overwhelming more aggressive than females (about 0.5 standard deviations, regardless of measure), and females are more agreeable and (importantly, in my view) more conscientious by about 0.2 standard deviations. The female advantage in conscientiousness is likely of first-order importance, particularly in academia, where tenure-track professors need to be self-starters.

These differences are all likely to have some measure of "biopsychosocial feedback," as the psychologists like to say, which is to say that they are deeply endogenous. But given the well-documented links from manipulable hormones to spatial ability, the links running from genetics through hormones to average spatial ability don't appear all that weak. And as long as economics relies heavily on mental rotations—manipulations of production functions, linear algebra, separating hyperplanes, and the like—this difference is likely to remain relevant when explaining the relatively low representation of women in economics.

How big are these differences quantitatively? The table below provides an illustration based on a normal distribution; it's a concrete reminder of what's going on in the tails. If the men of today actually do have an advantage in spatial ability—an advantage, based on Hyde (2005), that raises their mean 0.5 standard deviation higher than the female mean—and if we temporarily assume that men and women have the same standard deviations on this ability, then, at two standard deviations above the female mean, the ratio of men to women is 2.4:1; at three standard deviations it's 4:1, and at four standard deviations it's 6.5:1. Adding in a 5% gender difference in standard deviations (as Deary 2003 found for IQ) raises these ratios to 2.5:1, 5:1 and 11:1, respectively.

But perhaps we're overestimating that gap, or we only think half the gap is genetic, or only half of it is important to economics. If we instead cut the M-F gap in half while keeping the 5% gap in standard deviations, then the ratios shrink to 1.8:1 , 2.8:1, and 4.8:1, respectively, as shown in the fourth column. And if we note that men apparently have about twice the standard deviation at math-related skills than women, then the final column may be relevant, where males swamp females at the extremes.

Table 1: Population density, Predicted M-to-F Ratios

	5% higher male standard deviation	**0.5 SD higher spatial**	**5% higher SD, 0.5 SD higher spatial**	**5% higher SD, 0.25 SD higher spatial**	**10% higher SD, 0.5 SD higher spatial**
2 SD	1.1:1	2.4:1	2.5:1	1.8:1	2.7:1
3 SD	1.5:1	4:1	5:1	2.8:1	6.2:1
4 SD	2:1	6.5:1	11:1	4.8:1	17.2:1

Note: Each box indicates the predicted number of men to women at two standard deviations, three standard deviations, and four standard deviations above the female mean. The shifts involve increases in the male standard deviation and/or shifts (measure in standard deviation units) of the male distribution relative to the female distribution. In all cases, I assume a normal distribution. Importantly, this predicts densities *at* these cutoffs, not cumulative distributions *above* these cutoffs.

CONCLUSION

If women and men differ genetically in the abilities that are important in fields like economics, then it would be difficult to argue that the ideal gender balance is 50%. Whether the ideal gender balance is greater or less then 50% turns on many things, including which skills are needed in economics as it actually exists today. If non-computational math skills are of first-order importance, and if men and women are roughly equal across all other relevant skills, then the evidence presented here indicates that the ideal gender balance tilts strongly toward a male-dominated economics profession.

But even if the sexes do turn out to differ genetically on spatial rotation ability, and even if such abilities are important in thinking about abstract mathematical models, what of the consistent female advantage, across ages and cultures, in computation, spatial memory, agreeableness, and conscientiousness, each of which may likewise be genetic in origin?

Future changes in the nature of the profession—driven from within by changes in what economists find interesting, or from without as technological change makes certain skills more important—simply can't be ruled out. And of course, by the time such changes occur, scientific advances could find easy work-arounds to any innate differences between men and women's abilities. Surely, there's a market for such advances. Just as the computer made slide-rule skills of manual dexterity irrelevant, and as eyeglasses made genetic differences in vision largely irrelevant, future innovations may shift the relative worth of various mental skills.

Economics as currently practiced does appear to draw on rare skills that are more common among men than among women: The higher male mean and variance on key abilities is likely to quantitatively swamp the areas of female strength. That said, the evidence for those male advantages being *genetic* isn't as strong as evidence for the mere *existence* of such advantages. With current scientific understanding, the male-female differences on mathematical skills appear likely to persist, even under plausible social interventions like gender-neutral teaching methods. So actually-existing economics is likely to remain a male-dominated field, as long as the supply of and demand for relative skills in the profession remain roughly constant. Thus, if the academic job market is close to competitive—indeed, since it is likely influenced by affirmative action in favor of female hires—then there are good reasons to place a sizable amount of the mass of $f(g^*)$ in the vicinity of $\hat{g}$. In other words, we may not be that far from the ideal ratio of female economists to total economists.

How could this change? How could economics change itself so that it has as many high-performing women as there are in fields such as literature, history, or sociology? The question answers itself: Economics could change itself so that it draws on the skills at which women, on average, excel. A more literary and historical economics, one more driven by verbal fluency and conscientious archival work, would be an economics that created greater opportunities for women. Other methods surely exist for raising the number of high-performing women in economics–by encouraging recalcitrant male economists to treat female economists fairly, by lengthening tenure clocks for promising female academics who bear children, and surely through other methods. But if men have substantially and persistently higher means and variances in the key skills that go into making an economist, then the only intervention of first-order importance may be to change economics itself.

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Go to *Gender and Economics* Symposium Page

Go to May 2008 Table of Contents with links to articles

Go to Archive of **Investigating the Apparatus** Section

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Character Issues

Honestly, Who Else Would Fund Such Research? Reflections of a Non-Smoking Scholar

Michael L. Marlow[1]

Abstract

Welcome to SourceWatch—your guide to the names behind the news. SourceWatch is a collaborative project of the Center for Media and Democracy to produce a directory of the people, organizations and issues shaping the public agenda. A primary purpose of SourceWatch is documenting the PR and propaganda activities of public relations firms and public relations professionals engaged in managing and manipulating public perception, opinion and policy. SourceWatch also includes profiles on think tanks, industry-funded organizations and industry-friendly experts that work to influence public opinion and public policy on behalf of corporations, governments and special interests.

—SourceWatch website (hyperlinks removed)

I qualify, evidently, as an "industry-friendly expert" who works to influence policy and opinion on behalf of corporations and special interests. The SourceWatch entry on me runs to more than 1100 words. It reports that I have "a long term relationship with Phillip Morris," which isn't true. It lists many of my writings on smoking policies and my acknowledgements to the tobacco industry for financial support. It also notes that I was a member of the Academic Advisory Board of a "pro-tobacco junk science report."

After working at US Treasury in Washington, DC, I came in 1988 to San Luis Obispo, California, to become an economics professor at Cal Poly. I still live

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in San Luis Obispo. Among its many charms, San Luis Obispo is noted for the fact, highlighted by its Wikipedia entry, that "The city also banned indoor smoking in all public locations, including bars and restaurants, in 1990, making it the first city in the world to do so." Despite this much lauded achievement and my longstanding interest in public economics, I had never given our smoking ban much thought prior to its launching. Prior to the ban, there were a few times that my wife and I had left some local restaurants prior to ordering because the smoke levels were unpleasant to us. But these restaurants were exceptions and mostly catered to tourists, and we never had any difficulty finding restaurants that had voluntarily forbid smoking, or had accommodated smokers and non-smokers by way of smoking/non-smoking sections, outdoor smoking patios, or other means that met our preferences. My only observation about the ban's introduction was that it didn't appear particularly contentious in a college town that appeared to have few smokers and conformed to the "California is different" perception that those of us originally from the East-Coast can't help but experience.

I am probably more tolerant of behaviors I don't follow than those who believe that it is critical for government to actively address personal behaviors. Deep-seated judgments regarding what matters as well as which ideas to pursue in research are important for readers to understand. My ideological sensibilities are in the direction of believing that private or free markets tend to allocate resources pretty well, and that pro-interventionists tend to not understand very well how private markets operate. I see pervasive paternalism, a hastiness to find fault with private markets and a tendency to romanticize government intervention. In short, my ideological sensibilities run along the lines of "public choice" analysis, with a strong dose of Milton Friedman's advocacy of empirical testing of competing hypotheses. My interest in public choice analysis is not surprising, given my graduate training at Virginia Tech when it housed the Public Choice Center. My fascination with empirical work stems from my view that the essential public-policy question is always: *Since both private markets and governments fail to allocate resources perfectly, which does a better job?*

My involvement in our local smoking ban issue came about when my fellow Cal Poly economist and Business School Dean Bill Boyes was contacted by a local group to propose an economic study of the ban under a public health grant made possible through state taxes on smokers. Bill thought this would be a good local public-relations project for our school and suggested we work together because he thought this policy issue was "right up my alley." At first, I was not particularly interested in pursuing a cost-benefit study of the smoking ban because I was writing a public finance textbook and didn't need the distraction. I also was hesitant about getting involved in what could become a contentious issue within our fishbowl town of about 40,000 people, and within the campus community. My first suspicion was that many locals were unlikely to be appreciative of any costs we uncovered since the local newspaper and public health groups were gushing about how progressive they were in pushing for the ban. Prior experience had taught me that individuals who push for government expansion rarely enjoy learning of attendant costs.

But I was just finishing a chapter on externalities in my public finance textbook which included a long section on Ronald Coase's (1960) article on social costs. I devoted five pages to the so-called Coase theorem. One of the vivid memories of my graduate student days at Virginia Tech during 1975-78 was reading Coase's paper during my first course in microeconomics taught by Robert Staff. Though I struggled with this paper, I came away with the understanding that the Coase theorem demonstrated that under certain circumstances private markets are reasonably capable of dealing with externalities and therefore provided a cautionary tale for the many economists who Coase apparently believed were too quick to jump to government intervention. At the time, I did not understand what all the fuss was about, why we were required to read Coase or who all these economists were who were so tempted to expand government's role in our economy. Over the years, I remained intrigued with Coase's focus on transaction costs, especially as I began to ponder why so many people I met were so enthusiastic for expanding government, which certainly carries its own transaction costs. My first 10 working years were spent in Washington, DC, first at George Washington University and then the US Treasury, and I learned that eager interventionists really are pervasive.

Coase highlighted the "reciprocal nature of externalities." The insight helps us to understand that affected parties may find diverse ways of coming to agreement. Now, we all understand that Coase's lesson was as much to highlight the need to assess comparative failure as to posit low transaction costs, but the issue into which Boyes was enticing me was, in fact, one enclosed within a well-defined sphere of private ownership. Transaction costs are low in the important sense that you can easily avoid a smoky experience by not going to the restaurant. Owners recognize your power to exit, and hence consider your interest in how the experience at their restaurants should be managed.

Thus, Coase's insights were "front and center" in the proposal that Bill Boyes and I presented to the grants committee in San Luis Obispo. We began with simple questions: *How did owners of businesses deal with the issue of smoking prior to the ban? Do owners manage their resources to enhance the long-term values of their businesses?* Undoubtedly, owners understand that smoking is unhealthy, since that has been public knowledge since at least the 1964 Surgeon General's Report on Smoking and Health. Owners also understand that many non-smokers find the smoke unpleasant.

Our proposal argued that, while negotiation and grimacing between restaurant patrons is not a good solution to the problem, strong incentives motivate owners to manage the place so as to accommodate, mitigate, and balance the interests of smokers and non-smokers.[2] The airspace in question is privately owned,

2 See Boyes and Marlow (1996), Boyes and Marlow (1997) and Dunham and Marlow (2000A).

and the owner has cause to manage it in such a fashion as to increase profits. As long as owners desire profit, it would appear reasonable that a private market in accommodation exists. The next logical step was to study this private market and see how it manages resources.

Part of our proposal was to collect and examine data on pre-ban accommodation practices of local business owners. This seemed to me an interesting question and necessarily an important part of any cost-benefit analysis of a smoking ban. I thought that it was pretty hard to ignore that smoking-nonsmoking sections were becoming commonplace in restaurants and that such accommodation was virtually nonexistent 20 years previous. Another major method, though less obvious to restaurant goers, is ventilation—in principle, a non-smoker could dine in comfort while seated alongside a smoker provided that fans made sure that the fumes never came her way. Ventilation is the secret to no-flush toilets in secluded homes off the sanitation grid.

We also proposed to survey owners regarding profit changes following the ban. I didn't predict that all owners would lose, and in fact, thought most would either be unaffected or somehow gain from a ban, since this was the first city to ban smoking so fully, and so it made sense to me that the city had relatively few smokers and thus little opposition. But, I thought that it was important to determine how many would lose, even if it was a small number, because I don't see how their welfare could be unimportant. I was enthusiastic about researching the Coase theorem and even wrote a five-page appendix applying it to smoking bans in my externalities chapter. I pulled the appendix after receiving negative feedback from my editors following outside reviewers who found it somewhat unsavory to discuss smoking in this way.

Perhaps, we should have anticipated reaction to our proposal when we presented it to the grants committee of about 7 individuals. I remember seeing quite a bit of apprehension on their faces as I discussed the reason for collecting information on the pre-ban accommodation practices of owners. When I explained that we needed the knowledge to understand who would tend to lose or gain, they looked at each other uncomfortably. Their facial expressions turned especially unfriendly when I mentioned that owners who previously had many smoking customers might suffer losses. We were then politely told: *We already know the answers to these questions. All owners benefit from the ban and so it would be a complete waste of money to collect such data.* A few days later, Bill Boyes informed me that they rejected our proposal and later I heard from another colleague of rumors that the grant committee concluded that we did not understand the economics of smoking bans very well.

I was not particularly sorry about our rejection because I had other work to deal with. However, Bill Boyes wasn't fazed by the rejection and quickly suggested that I put together a group of economics students to collect our own data without benefit of any grants. Cal Poly requires all students to complete a senior project and we had no problem enlisting eight students to collect data. We simply gave them our survey instruments containing a multitude of questions from our rejected proposal. The students administered one survey to all affected owners of restaurants and bars, asking about their pre-ban accommodation practices and what effects the ban would have on profits. The students also administered a consumer-side survey randomly to residents and tourists, asking about their own smoking habits, preferences, opinions, and experience as consumers.

The data were interesting and provided the empirical basis for our paper "The Public Demand for Smoking Bans," published in *Public Choice* (Boyes and Marlow 1996). From our survey of 764 randomly chosen individuals within San Luis Obispo, we found that 62 percent of nonsmokers and 40 percent of smokers believed that smoking/nonsmoking sections were "effective" accommodation strategies. Prior to the ban, 95 percent of nonsmokers and 31 percent of smokers requested non-smoking sections of restaurants.

On the supply side, we found that 68 percent of owners had not received "many" complaints about smokers prior to the ban. 53 percent of all owners had been in favor of the ban prior to its passage. To the question of profits, 25% of owners predicted negative impacts, 17% predicted positive impacts, and 57% predicted no effects. As for accommodation efforts prior to the ban, 61 percent of all owners reported having expended resources toward reducing smoke within their establishments. Of those owners, 62 percent reported the implementation of smoking/nonsmoking sections, and 19 percent had provided smoking patios. Thus, the data demonstrated active efforts in accommodation, with owners as intermediaries. It appeared that the Coase Theorem was alive and well in explaining how the private market dealt with the smoking issue prior to the ban in San Luis Obispo. It also demonstrated that the prediction by the grants committee of no adverse economic impacts was clearly wrong.

ARE WE WRONG TO INVESTIGATE MARKETS IN ACCOMMODATION?

Some economists simply dismiss out-of-hand the first-round empirical checks on whether there is a private market in accommodation. I offer just a few choice quotes here. An early dismissal is contained in a health economics textbook, that states:

> Trying to use agreements … between people in a restaurant to determine whether smoking would take place would be the height of absurdity, and nobody would think seriously of a full "property rights" approach to such a problem. The transactions costs of reaching agreements would overwhelm the problem. (Phelps, 1992, 430)

Private markets could not possibly deal with the complex bargaining issues of smoking, and we can simply save a lot of time and effort by jumping ahead to smoking bans. The hidden message appears to be: *Researchers who investigate whether private markets attempt to deal with smoking preferences should simply be dismissed or ridiculed by those who understand that private markets must fail at this endeavor*. Clearly, from the outset there is a bias in favor of government intervention.

A recent example comes during an exchange between David Henderson (2007) and Benjamin Alamar and Stanton Glantz (2007), which began when Henderson argued that both theory and empirics in the latter's 2004 *Contemporary Economic Policy* article were illogical. Alamar and Glantz (2004) concluded that imposition of smoking bans somehow raises values of restaurants (median increase of 16 percent) and therefore smoking bans not only exert no harm, but actually raise values of businesses. In response to Henderson's (2007) argument that a private market deals with smoking externalities, Alamar and Glantz (2007, 292) respond:

> This assertion cannot be true. It is not possible for a restaurant owner to internalize the cost of second-hand smoke on the health of the staff or the patrons. There is no mechanism by which a restaurant owner can compensate a patron for any health costs related to second-hand smoke, therefore it is not possible for the owner to have completely internalized the costs of the externality imposed by the smoker. This fact is one reason that the public has demanded laws to make restaurants smokefree. (Alamar and Glantz 2007, 292)

At the end of their paper Alamar and Glantz reiterate their main result and add: "This result is consistent with all other literature on the subject that has not been funded by the tobacco industry" (2007, 293). That statement is untrue—Boyes and Marlow (1996) was not funded at all, and, really, happened against long odds. But even if it is nearly true, perhaps it tells us about the unwillingness of parties other than the tobacco industry to fund research that might come to politically incorrect conclusions.

Why not further examine data on accommodation? But data is hard to locate or acquire or gather. I recommend that interested readers conduct an Internet search themselves and see how much data are available on accommodation. Best I can tell, no one else has ever gathered such data. What are abundant, however, are numerous web pages describing how smoking bans exert no harm on businesses because there are so many studies that prove that conclusion. A much cited literature review, coauthored by Glantz, reports:

> All of the studies concluding a negative impact were supported by the tobacco industry. 94% of the tobacco industry supported studies concluded a negative economic impact compared to none of

> the non-industry supported studies… *All of the best designed studies report no impact or a positive impact of smoke-free restaurant and bar laws on sales or employment. Policymakers can act to protect workers and patrons from the toxins in secondhand smoke confident in rejecting industry claims that there will be an adverse economic impact.* (Scollo, Lal, Hyland, and Glantz 2003, 13; italics added)

Why are there never any studies showing how poorly the private market works? The dominant belief must be that the issue is so well settled that no discussion is necessary, other than to tell readers that any research to the contrary is wrong and is somehow connected to the tobacco industry. Perhaps, not surprising, these other studies never seem to mention the few studies that address this issue.[3]

JOHN DUNHAM, CHIEF DOMESTIC ECONOMIST OF PHILIP MORRIS MANAGEMENT CORPORATION

I thought my work on the topic ended with my *Public Choice* paper with Boyes. About a year after completing the paper, John Dunham, who at the time was the chief domestic economist of the Philip Morris Management Corporation, contacted me about extending the work. I had never received a corporate grant before, and had never heard of John Dunham. But I was happy to receive funding on an issue that I thought was interesting and neglected. Dunham had heard about my work on smoking bans from Bill Orzechowski, who was then the chief economist for the now defunct Tobacco Institute. Bill and I knew each other from my days in Washington, DC when he was at the US Chamber of Commerce and we had co-authored several papers on tax issues unrelated to tobacco issues. I was also pleased that my work was considered valuable by Phillip Morris Corporation, certainly a very large business, and perhaps because as a business school professor I had many times heard the adage, "*those who can, do; those who cannot, teach*."[4]

My relationship with Dunham continued for several years in which we worked together on a number of projects from which we published four papers on the subject of smoking bans—in *Economic Inquiry* (2000), *Contemporary Economic Policy* (2000), *Applied Economics* (2003), and *Eastern Economic Journal* (2004)—four established, refereed economics journals, three of which are in the *Social Sciences Citation Index*.

The point is that my numerous professional works—with Boyes, with Dunham, and other sole-authored work (see reference list)—are very often simply

3 Research considering whether private markets accommodate smoking preferences has been both infrequent and unempirical; e.g., Thompson, Frost and Plaskett (1990), Lee (1991) and Tollison and Wagner (1992).

4 Woody Allen, of course, added: "and that those who cannot teach, teach gym."

ignored or dismissed, despite passing peer-review muster, simply because, as SourceWatch confirms, Philip Morris provided funding for some of my research. Others who have published politically incorrect findings and ideas face similar problems—it's not just in smoking research. There seems to be a tacit understanding that it is OK to dismiss such politically incorrect research.

The relationship with Dunham proved valuable to me because, not only does he have excellent insight into how regulations affect businesses, but his institution could fund the collection of data necessary to test hypotheses. I was also pleased that Dunham insisted that our research stand the test of peer-reviewed publication, because I would have been otherwise hesitant to pursue research ties. My writing was never subjected to internal censorship and always acknowledged the funding.

The primary focus of my work with Dunham was to uncover the depth of private markets in accommodation and to test the effects of such bans on business owners, workers, and their customers. The studies again found an active private market in accommodation. Here is a gloss of the findings:[5]

- National survey data indicate that in communities without bans owners do not adopt identical accommodation strategies. Some owners voluntarily ban all smoking and others allow smoking throughout or dedicate areas where smoking is not allowed. We found that nearly one-fourth allocated at least three-fourths of their seating to non-smoking use.
- Owners with fewer smoking customers allocate more resources toward non-smokers (more non-smoking seating, better ventilation, and other accommodations).
- Owners with more smoking customers predict losses from bans more often than those with few smoking customers.
- Owners often adjust prices, wages, hours of operation, and other business attributes in response to bans, so bans also affect customers and workers.
- Smoking bans are not fully enforced. Formal collection of noncompliance data appears to be only funded by public health and tobacco-control agencies. For example, the California Department of Health Services (2001) reported that 10 percent of bars attached to restaurants throughout California were noncompliant in 2001. Weber, Bagwell, Fielding and Glantz (2003) report a 24% noncompliance rate for freestanding bars in Los Angeles in 2002. Numerous newspaper articles document noncompliance across the country, but such frequent anecdotal evidence does not summarize easily. Common sense suggests that compliance is inversely related to the degree of harm imposed on owners.

5 See Dunham and Marlow (2000A, 2000B, 2003, 2004).

- Bars experience more harm than restaurants because bars are more hospitable to patron interactions. Few bars voluntarily disallow smoking or provide smoking/non-smoking sections. We found that 92 percent of bar owners allowed smoking throughout their establishments, and bar owners were more than twice as likely to experience losses as restaurant owners.
- Because bans are mostly adopted in jurisdictions with fewer smokers, jurisdictions that ban smoking experience less harm than would jurisdictions that had more smokers.
- The probability that an area has a ban is positively related to the non-smoking share of the population, so bans are endogenous and tend to be enacted after private markets have already been accommodating in favor of non-smokers.

Of course, the available research does not estimate "perfection" and prove that private markets achieve it. But the evidence does reveal a private market where owners allocate resources in directions consistent with economic theory and therefore predicts that, as smokers dwindle in numbers, smokers will continue to lose ground to non-smokers. The market process evolves over time, and it is undoubtedly true that owners expend greater accommodation efforts now than in the past, simply because they pursue profits in a competitive environment in which consumers are more opposed to smoke. As is often the case, the social trend precedes the political movement.

The next logical step is to examine how many owners are harmed by bans, because extent of harm is one issue that a community might consider when debating passage of a ban.

How to Show That Bans Inflict No Harm

In submitting smoking-ban research for publication at economic journals, I've received roughly 10 referee reports. Not one referee or editor questioned the prediction that bans would exert differential effects on businesses. Readers might then be surprised that a fairly large empirical literature reports that bans exert no adverse effects on owners—the public policy equivalent of showing that "water runs uphill." Some empirical studies even advance arguments that bans raise profits so much that owners should, in effect, thank ban advocates for raising their wealth. A recent literature review states, "the vast majority of studies find that there is no negative economic impact of clean indoor air policies, with many finding that there may be some positive effects on local businesses" (Eriksen and Chaloupka (2007, 375) I now discuss how these economic studies arrive at such conclusions.

Ignore, Denigrate, or Deny Private Markets in Accommodation

A common research strategy is to simply dismiss the idea that private markets can manage resources efficiently. There is, of course, a problem with this logic because governments, too, are imperfect. Without plentiful data on accommodation, concluding that bans are the only or the best solution becomes rather silly. I have yet to see researchers who claim that bans exert no harm ever collect, examine, or even acknowledge such evidence. I invite the reader to explore how many times pro-ban advocates cite any of my publications with Boyes and with Dunham and debate whether such accommodations are sufficient at dealing with smoking. Please let me know when you find one such instance.

Another means of denying existence of a private market is to forget that most economists would never claim that zero air or water pollution is a meaningful goal simply because reduction comes with both costs and benefits. A smoking ban, however, overturns this logic since, in effect, the arguments for zero smoking must mean no costs are ever incurred when owners are forced to eliminate smoke. Lost customers or sales following a ban, or lost smoker welfare, can clearly be considered a cost of purity. A more informed policy might allow a finite number of tradable permits to be auctioned off in jurisdictions whereby owners whose businesses benefit the most from smoking can, at a price, have such rights over their property. I have never seen this method proposed by ban advocates, even though it directly parallels the now accepted idea of tradable emission rights. Entertaining the argument would, of course, acknowledge that bans might harm some businesses.

An interesting twist is the strong reactions to improved ventilation. Drope, Bialous and Glantz (200) describe how the tobacco industry, in concert with the hospitality industry, "developed a network of consultants to promote ventilation as a 'solution' to secondhand smoke (SHS) in the USA" (41). These authors were alarmed that improved ventilation could undermine passage of bans, and, as their proof, these authors argue that the tobacco industry involvement must somehow demonstrate that ventilation does not solve any of the problem with smoke. A more useful approach might focus on how well ventilation works and whether there are sufficient incentives for private firms to invest and innovate in improved ventilation. But, again, this study would have to acknowledge existence of a private market in accommodation.

Employ the "Community Effects" Methodology, and Don't Reveal Its Shortcomings

Do researchers claiming that bans exert no harm investigate how many firms gain, lose, or are unaffected? Not really. Most studies employ a "community effects" methodology that aggregates all businesses into one number and then examines whether this aggregate changes following a ban. The examination becomes: *Do aggregate sales or tax revenues rise, fall, or stay the same following a ban?* Studies routinely conclude that sales and tax revenues never fall, but rise or stay the same.

The "community effects" method is like comparing over a ten year period how the average weight of 50 students has changed, and after observing that the average weight started 160 pounds and remained 160 pounds ten years later, concluding that no changes occurred over the period. Meanwhile, some students gained 20 pounds, some lost 10 pounds, and still others exhibited no change. Maybe there's a defense of the "community effects" approach, but I have never seen one, because these studies never address the basic criticism.

A recent example is the paper by Eriksen and Chaloupka (2007). In the abstract to "The Economic Impact of Clean Indoor Air Laws," published in *CA: A Cancer Journal for Clinicians*, they write:

> The vast majority of scientific evidence indicates that there is no negative economic impact of clean indoor air policies, with many studies finding that there may be some positive effects on local businesses. This is despite the fact that tobacco industry-sponsored research has attempted to create fears to the contrary. Further progress in the diffusion of clean indoor air laws will depend on the continued documentation of the economic impact of clean indoor air laws, particularly within the hospitality industry. This article reviews the spread of clean indoor air laws, the effect on public health, and the scientific evidence of the economic impact of clean indoor air laws. (367)

These experts appear rather convinced. But, again, most of the studies supporting their position employ the "community effects" approach and this fact is not discussed. No references to my studies with Boyes or Dunham are contained within their extensive survey of the literature. The authors are also apparently content to repeat the mantra that tobacco-industry funding is the reason for why any studies might contradict their conclusions.

There are possible defenses for the "community effects" approach, but they never seem to merit discussion. One possibility is that smoking bans might help solve a collective action problem of owners. Suppose, for instance, that, although a general ban might raise revenues of all owners, cartel-like cheating undermines this result. The fact that firms are tempted to cheat and be noncompliant does not demonstrate that the ban isn't in their collective interest. It merely means that individual owners can do even better for themselves by having a general ban and then cheating unilaterally. Of course, if everyone cheats, there is no gain. The difficult trick then would be to conduct "community effects" studies only in those communities where no cheating occurs.

It also might make sense to look at "community effects" when some owners lose because, even if individual firms do suffer, they might in principle be compensated by those who gain. There are, of course, well-known problems with "potential compensation" versus "actual compensation", but this approach is common in welfare economics and might merit discussion by advocates of bans. Entertaining the argument would, of course, acknowledge that bans might harm some businesses.

DISMISS ALL CONTRARY RESEARCH AS BIASED OR EVEN DECEITFUL, ESPECIALLY WHEN FUNDED BY THE TOBACCO INDUSTRY

A prime example of this strategy is Scollo, Lal, Hyland and Glantz (2003) which reviews 97 studies on the economic effects of smoking bans on restaurants and bars. This widely-cited study concludes that, while roughly one third of all studies indicate that bans impose some harm, they are all seriously flawed. The authors point out that 94% of studies showing harm are somehow associated with tobacco industry support and they don't appear to have any reservations about leaving readers with the thinly-veiled assumption that any association with the tobacco industry explains conclusions counter to their thesis that bans exert no harm. The fact that most other studies conclude either positive or no impact on businesses leads the authors to conclude: "Policymakers can act to protect workers and patrons from toxins in second-hand smoke confident in rejecting industry claims that there will be an adverse economic impact" (13).The fact that none of these studies received any tobacco industry support is also used to suggest that these conclusions are correct as well as intuitively obvious. The survey by Eriksen and Chaloupka (2007) is the most recent study using this tactic to explain away all contrary research, and cites Scollo, Lal, Hyland and Glantz (2003) as evidence.

It would take too many pages to point out the many flaws in Scollo, Lal, Hyland and Glantz (2003) and others, so let me highlight a few of the major problems. Studies finding no economic harm never formulate how smoking bans might affect businesses. They never allude to private markets in accommodation, so such facets never enter into hypotheses. They never mention studies that find private market accommodation. The extent to which authors develop hypotheses can be summarized in the following two-part story:

- Because owners cannot effectively deal with smoking, bans are necessary to protect patrons and workers from second-hand smoke; and
- Owners don't suffer harm because bans will draw more non-smokers into businesses, and, because there are more non-smokers than smokers in the population, businesses gain more than they lose when disgruntled smokers

spend less.

Even if this two-part story is correct, these studies cannot really test the no-harm hypothesis because they employ the above-discussed "community effects" approach and must, by research design, ignore effects imposed on individual owners.

Another common strategy is to denigrate all survey data of business owners as "biased". Scollo, Lal, Hyland and Glantz (2003) make the following case for why we should heavily discount any such data:

> Unverifiable predictions of future changes or estimates of recent changes in patronage or spending were deemed 'subjective'. Subjective measures included anecdotal reports and self report data collected in polls of, or interviews with, patrons or owners of restaurants, bars or similar businesses, conducted either before or after the policy was put in place. (14)

Would owners not be the best judge of whether bans hurt their business? Apparently owners' reports must be systematically biased. Invariably one finds insinuations about survey-based data collection and the tobacco industry, providing readers a reason to suspect that data collected on individual owners are biased and misleading. That such data are essential to examining differential effects on owners is never mentioned. Ban advocates repeat the "bias" argument over and over, since otherwise they might have to reveal to readers that something other than a "community effects" analysis might provide a better understanding of how bans affect owners.[6]

WHAT ABOUT SMOKERS?

In economics we discuss efficiency in terms of both producers and consumers, but this norm is not followed in research on smoking bans. Do smokers not deserve to be included in our welfare analysis? While non-smokers are clearly a majority of the population, and growing,[7] there are still many smokers left out of the analysis. Ignoring smokers is an easy way of ignoring costs imposed by the bans. Pro-ban researchers simply ignore the elimination of blocks of consumer

6 Even if we accept the "community effect" method, Dunham and Marlow (2000B) argue that past applications of this method do not really demonstrate no harm. Problems include: "cherry picking" of data, imperfect enforcement of bans, examinations based on revenues and not profits, and the error of concluding that bans don't harm when sales rise following bans when, in fact, bans exert harm when they cause sales to rise at a slower rate than otherwise would have occurred.

7 From 1990 to 2005, U.S. taxed sales per capita have fallen 38%, from 101 to 63 packs (Orzechowski and Walker 2006), and smoking prevalence of adults has fallen from 25.5 to 20.9 percent (Centers for Disease Control 2006).

surplus.

The costs to smokers find various manifestations. Loss of freedom over smoking may foster some of the following behaviors. Smokers may eat or drink out less often, spend less time in areas subject to bans, congregate outside of banned areas to smoke, smoke outdoors more often in inclement weather, smoke more at home, purchase higher nicotine cigarettes so that they spend less time puffing to achieve preferred nicotine levels, change intensities by which they puff since they must now "time" their smoking differently, suffer stigmatization in their smoking, and perhaps behave as "closet smokers." They may also substitute some or all of their cigarettes for smokeless tobacco, gum, or hard candy. The list of changes probably goes on and on. Some suggest that the new costs of smoking—higher prices, inconvenience, stigma—are a factor in growing obesity.[8]

Dunham and Marlow (2003) find that affected owners alter prices, wages, hours of operation, and other business attributes that raise or lower welfare of both smoking and non-smoking patrons and workers. Evans and Farrelly (1998) and Farrelly et al. (2004) find that higher taxes cause smokers to raise purchases of cigarettes with higher tar and nicotine. Adda and Cornaglia (2006B) analyze compensatory behavior of smokers as evidenced by data on concentrations of cotinine (a metabolite of nicotine) and find that higher taxes cause smokers to extract more nicotine per cigarette. While these studies do not explicitly study effects of bans, it is reasonable to suppose that bans are a factor. It is widely believed that higher concentrations exert more harm than lower concentrations and so bans might exert adverse health effects on some smokers.[9]

Adams and Cotti (2008) report an increase in fatal driving accidents involving alcohol following smoking bans in bars. They find evidence consistent with the explanation that smokers drive longer distances to get to a bar where they may smoke, sometimes to a bordering jurisdiction without a ban, sometimes to bars that still allow smoking, perhaps by non-compliance or an outdoor area. After departing the bar they have a longer drive, hence more alcohol-related driving accidents.

Surprisingly, there are studies that fail to find that bans reduce smoking. Buddelmeyer and Wilkins (2005) find that smoking bans in Australia don't significantly lower smoking for most types of individuals. They also find a significant "rebellion" effect among 18-24 year old smokers because they became more likely to continue smoking following bans. Apparently, a "James Dean" effect on youth may exist whereby bans make it easier to display "anti-social" behavior.

Non-smokers may also suffer adverse health effects if they inhale more second-hand smoke when they are with smokers within cars or homes. Adda and

8 Early studies connecting smoking cessation with weight gain are Williamson et al. (1991) and Flegal et al. (1995). Regal (2007) argues that, if smoking prevalence in 1999-2002 were at the higher 1971-1975 smoking level, estimated 1999-2002 obesity prevalence in 1999-2003 would be 22.5% rather than the actual value of 23.9%.

9 See Thun et al. (1997) for a discussion of these adverse effects.

Cornaglia (2006A) examine effects of bans on passive smoking through analyzing cotinine concentrations on a large sample of non-smokers over time. While they found that bans exert on average no effects on non-smokers, bans in recreational public places perversely increase some non-smokers exposure by displacing smokers to private places.

Adam Smith famously said that in the chessboard of human society each piece has principles of motion all its own. We do not—and cannot—know how the human chess-pieces react. The point is that acknowledgement of such factors, arising from imposed costs, are commonly left out. Advocates of bans typically represent the "man of system" attitude that presupposes inert "chess pieces"—exactly the mentality Smith was challenging (Smith 1790, 233-34).

Moreover, there are also reasons to believe that we do not fully understand health consequences associated with second-hand smoke. My point is simply that statements like *"Policymakers can act to protect workers and patrons from the toxins in secondhand smoke confident in rejecting industry claims that there will be an adverse economic impact"* (Scollo, Lal, Hyland and Glantz 2003, 13; italics added) are irresponsible.

Men of System

In the late 1990s I attended a session at the Western Economic Association meetings organized by a well-known public health economist who had invited 3 papers on the economics of tobacco control by researchers who had all received various public health grants. One of the papers concluded that smoking bans were ideal policies because they protected non-smokers from toxins in second-hand smoke. During the question period, I did something quite out of the ordinary for me. I asked: *"Are you concerned that bans might raise the intensity of puffing by smokers which, if true, might cause them additional harm?"* The presenter spent about 4 seconds on my question with a response something like: *"I don't really understand why this is a problem."* For some reason, I persisted and after about the third time, I rephrased the question as simply: *"So, if smokers want to smoke in this manner and worsen their health, this is not really our concern because it's really about protecting non-smokers?"* The presenter replied quickly with something like: *"That is correct,"* and interestingly, neither he, nor any those on the panel or in the audience, seemed to have any problem with his response.

If your gang simply points to smokers and claims *"they are the source of the problem,"* it might be easy to convince yourself that government should impose taxes or regulations on them. But—particularly in spaces customarily arranged to host and accommodate smoking—smokers might feel that non-smokers or busybodies are the problem, in which case they might just as well want their behavior corrected—that is, to butt out. Pro-ban advocates ignore Coase's insights and, along the way, reveal that they do not really care about harm to the minority or business owners that cater to them.

Towards an Unbiased Treatment of Biases

My reflections bring us into issues of systemic bias in the way research is initiated, formulated, funded, validated, and received—biases not merely in the reasoning of any particular work, but in the cultural structures and processes within which the work comes into being. It's plain to me that deep problems exist, so the deep biases need to be diagnosed. The trouble is: How does one assure that the diagnosis of biases isn't biased? And if I offer such assurances, how about assurances that the assurances of unbiased diagnosis of biases are not biased? Subscripts, anyone?

Are Non-Smoking Researchers Biased?

Although few academics might admit biases based upon political affiliation, many recent studies suggest how little diversity exists among professors in the social sciences, humanities, social welfare, and health fields.[10] Although a Gallup poll found Republicans more likely than Democrats to support smoking bans in restaurants, hotels and the workplace, the same poll indicates that supporters mostly rally around one characteristic: whether or not they smoke.[11] This result is consistent with what Boyes and I found in our study of the smoking ban in San Luis Obispo. It is also well known that smoking prevalence falls with education.[12] Highly-educated researchers are therefore unlikely to smoke and, as a result, may also strongly support bans on others smoking in public.

Biases Attendant to Non-Tobacco Industry Funding of Research

What are the funding sources other than the tobacco industry? It doesn't take long to recognize the variety and frequency of acknowledgements that authors make regarding their financial support. The National Cancer Institute and National Institute of Health are commonly thanked, as are public health groups such as the National Heart Association, Americans for Non-Smokers Rights, and the W.E. Upjohn Institute. Of course, many funding dollars are also granted by

10 For example, the fields of biology, health, and social welfare are very lopsided, medicine and nursing less extreme so but also preponderantly Democratic. See Gross and Simmons (2007, 34) and (Cardiff and Klein 2005, 247); on the Democratic tent being relatively narrow, see Klein and Stern (2005; 271-273); on the economics profession in particular, and its paucity of free-market supporters, see Klein and Stern (2007).

11 Levels of support were: Republicans (62%) and Democrats (53%); see Moore (2005).

12 National Center for Health Statistics (2006) reports the following prevalence rates in 2004 broken down by education for those aged 25 and above: 29% (less than high school), 26% (high school graduate), 21% (some college, no degree) and 10% (bachelor's degree and above).

the many schools of medicine, government, and public health.[13]

The review of over 90 studies by Scollo, Lal, Hyland and Glantz (2003) acknowledges the following support:

The VicHealth Centre for Tobacco Control is funded by the Victorian Health Promotion Foundation to conduct economic, legal, and social research in tobacco control. Dr Hyland's work was supported by the Roswell Park Cancer Institute NCI-funded Cancer Center Support Grant, CA16056-26 as a member of the Biomathematics/Biostatistics Core Resource. Dr Glantz's work was supported by US National Cancer Institute grant CA-6102. (18)

Eriksen and Chaloupka (2007) acknowledge support of the Georgia Cancer Coalition and the Robert Wood Johnson Foundation's ImpacTeen project. What about those groups? Maybe they are biased against researchers willing to arrive at politically incorrect conclusions? Take a look at the websites of the Georgia Cancer Coalition (like here) or the Robert Wood Johnson Foundation (like here) and decide for yourself.

State tobacco control agencies also fund much research, and often directly via taxes on smokers.[14] "Tobacco control programs" fund school programs, enforcement of smoking bans and age limits, counter-marketing, cessation programs, and studies that demonstrate program effectiveness. Funding appears to go not only to studies demonstrating program effectiveness, but studies showing *only* benefits from such programs, and that such programs should be expanded.[15] From 1994 thru 2005, Massachusetts state government spent slightly over $400 million ($2003) on tobacco control,[16] and California from 1988 thru 2006 spent over $2 billion ($2003).[17] We do not have a breakdown that tells us how much went specifically to fund research.

Finally, what about funds from pharmaceutical firms interested in marketing nicotine replacement therapies? It is likely that some researchers are funded directly by these private firms, or at least indirectly, when such firms contribute to various lobbies, universities, or public health groups to support their products. Bans may well enhance their markets. These firms may also be interested in selling

13 The website of *Tobacco Control* lists the ten *Tobacco Control* articles that received the most downloading during 2007 (the articles were published between 2002 and 2006). The ten articles contain acknowledgements to 12 instances of funding support: Norwegian Institute of Public Health, Roswell Park Cancer Institute, National Cancer Institute (6 times; some papers listed multiple NCI grants), Flight Attendants Medical Research Institute, Health Canada/Canadian Institutes of Health Research, Australian National Health and Medical Research Council and the US National Institutes of Health. As for the authors of the articles, most were employed at universities and most department or school affiliations contained the words health, pharmacy or medicine. Other places of employment were National Health Screening Service (Norway), Norwegian Institute of Public Health, the Roswell Park Cancer Institute, Massachusetts Tobacco Control Program, World Health Organization and Organisation for Economic Co-Operation and Development.

14 See Marlow (2006).

15 See Marlow (2006).

16 See Koh et. al. (2005)

17 See Marlow (2007).

their products to public health agencies that then dispense them to smokers. Studies may also promote acceptance of their therapies by private and public health insurers. Little concern of bias is ever expressed. Perhaps, this industry gets a "pass" in the bias debate because it is on the right side of the smoking issue.

In an important article, "Warning: Anti-Tobacco Activism May be Hazardous to Epidemiologic Science," published in 2007 in *Epidemiologic Perspectives & Innovations*, Carl Phillips argues that much funding for anti-tobacco researchers does come with many strings attached:

> [M]uch of the funding from anti-tobacco organizations, both government and private, comes with major strings attached, often all but declaring what the conclusions of the research should be … Hardly a word is heard from that quarter about the pharmaceutical industry or others who have a financial interest in tobacco use or cessation methods, and who help fund the anti-tobacco organizations. Rather, those organizations are intent on making sure that they get to control the funding, and thus the agenda, in "their" area, and the only significant threat to this monopoly is tobacco industry funding. For example, despite the fact that anti-tobacco organizations' funds dwarf tobacco industry grants to academic researchers, no major research effort in tobacco harm reduction has been able to get fundamental funding without seeking it from the industry. (Phillips 2007, 12)

This view is consistent with a public choice view of the world.[18] The interrelations of government and academic institutions create a kind of centripetal groupthink, which allows the "right" private corporate sources of funding onto the team. Information and agenda control come from monopoly over funding and probably go a long way toward explaining the multitude of studies that claim that bans cause no harm. Consistent with this thesis is the fact that efforts have recently been made to forbid researchers at universities from receiving any funding from the tobacco industry.[19]

18 Tullock (1966) attributes many of these problems to the presence of a single buyer for research output. In this case, various government agencies and public health non-profits might be operating as a loose cartel whereby they determine the parameters of "acceptable" science and help orchestrate attacks on opposing research.

19 Blumenstyk (2007) discusses how various universities, such as public health schools at Johns Hopkins University and Harvard University, have banned researchers from accepting tobacco-industry money for research. Recently, the University of California had considered such a ban, but instead adopted a less-stringent policy whereby researchers may only accept such money following approval by a special scientific committee that verifies that any proposed study "uses sound methodology and appears designed to allow the researcher to reach objective and scientifically valid conclusions."

The File Drawer

If studies failing to show one's preferred hypothesis tend to be deposited in "file drawers," then published studies will be biased toward showing support for preferred hypotheses. Reporting biases have been discussed for many years. Leamer's (1983) paper "Let's Take the Con out Of Econometrics" describes the many biases that arise when researchers fail to report the many tests that they have conducted. Reporting a subset of all empirical runs conducted over the course of research promotes overconfidence in published results that are probably facilitated with rapid declines in computing costs and greater data availability. Ioannidis (2005) goes so far as to claim: "Simulations show that for most study designs and settings, it is more likely for a research claim to be false than true. Moreover, for many current scientific fields, claimed research findings may often be simply accurate measures of the prevailing bias" (124).

Even if researchers were indifferent to whether smoking bans impose costs, journal editors and reviewers would promote the file drawer effect when they strongly preferred one hypothesis over another. There appears great potential for this bias given that there is one journal that exists solely for the purpose of publishing papers on controlling tobacco. *Tobacco Control* bills itself as "An international peer review journal for health professionals and others in tobacco control" and is published by the BMJ Group which is a global medical publishing organization and a wholly owned subsidiary of the British Medical Association.[20] It shouldn't take readers long before they realize that the vast majority of papers published in *Tobacco Control* conclude that tobacco control measures are very effective. Many of the no-harm papers cited in the literature review by Scollo, Lal, Hyland and Glantz (2003) were published in *Tobacco Control*. And the Scollo, Lal, Hyland and Glantz (2003) review itself is published in *Tobacco Control*. Similar observations can be made for other public health journals such as the *American Journal of Public Health* and the *Journal of the American Medical Association*. Rarely do papers published in these and other journals conclude that government interventions do not effectively mitigate one problem or another. One might wonder how full the "file drawers" have become.

"Good Intentions" Bias

Smoking bans are often promoted on the basis of good intentions: *we are doing this to protect non-smoking patrons and employees; we are doing this for the health of children; we are doing this to protect addicted or uninformed smokers who cannot help themselves; we are doing this because owners do not understand that bans are good for them;* and so on.

The following quotation is the abstract of a 31-page *Journal of Economic Sur-*

20 See Tobacco Control's website: http://tobaccocontrol.bmj.com.

veys literature survey of tobacco-control studies:

> This survey focuses on government efforts to curb the use of undesirable goods, notably tobacco products. We synthesize the economics literature and examine the effectiveness of government curbs on tobacco consumption through non-price controls (such as bans on cigarette advertising, health warnings, and workplace smoking bans) and price measures (or higher prices through higher taxes). This literature review is unique in that we do not merely aim to provide a summary of the literature. Rather, our main focus is to draw conclusions from the literature regarding the effectiveness of alternate policy measures across countries in checking smoking and to provide directions/suggestions for extending the scope of government intervention to other tobacco products. (Goel and Nelson 2006, 325)

The survey contains 103 separate references and never addresses the fundamental issue of why it is that we need these anti-smoking remedies or how they know that private markets fail to effectively deal with smoking. The authors appear to have no reservations about pursuing interventions as long as they correct "undesirable" behaviors such as smoking. As the authors say, their purpose is to "provide directions/suggestions for extending the scope of government intervention to other tobacco products."

There may be a bias toward not going much farther than "good intentions," especially among a reference group consisting mostly of people who do not smoke, do not own a business, and romanticize the ability of governments to improve society. So, it might be pretty easy to persuade many that bans don't impose costs. It's also likely that many of us would prefer to never experience smoking again. These biases probably predispose many of us to not scrutinize evidence claiming that bans yield benefits with no harm.

SECOND-HAND SMOKE: SUSPECT SCIENCE

The following recent quotation summarizes the view that second-hand smoke isn't a major health problem.

> While there is ample evidence that chronic exposure to secondhand smoke increases the risk of cardiovascular disease, and therefore heart attack risk, and there is some suggestive evidence that acute exposure to secondhand smoke may present some danger of risk to individuals with existing severe coronary artery disease, there ap-

> pears to be no scientific basis for claims that brief, acute, transient exposure to secondhand smoke increases heart attack risk in individuals without coronary disease, that it increases such risk to the level observed in smokers, that it can cause atherosclerosis, that it can cause fatal or catastrophic cardiac arrhythmias, or that it represents any other significant acute cardiovascular health hazard in nonsmokers. (Siegel 2007, 24)

This summary comes from a paper entitled "Is the Tobacco Control Movement Misrepresenting the Acute Cardiovascular Health Effects of Secondhand Smoke Exposure?" in *Epidemiologic Perspectives & Innovations* in 2007. The author, Michael Siegel, is Professor of Social & Behavioral Sciences at Boston University's School of Public Health and, on his own website, he writes of himself: "He has been active in promoting smoke-free bar and restaurant policies throughout the country and has served as an expert witness in several major tobacco litigation cases."[21]

Siegel's (2007) paper discusses what he refers to as wild claims regarding adverse health effects of second-hand smoke of many pro-ban advocates and discusses how he has been personally attacked for criticizing such claims.

> The general approach has been to attack ad hominem, rather than to directly confront the arguments being made. For this reason, I have come to the impression that the tobacco control movement does not allow room for any difference of opinion, and that those who dissent with any aspect of the prevailing wisdom must be discredited, attacked and silenced. I sense a rather McCarthyistic element in the tobacco control movement. Whether the scientific arguments I have made are valid or not is up for question and debate; the unwillingness of the movement to be willing to entertain a discussion of the validity of its scientific claims, on the other hand, is a dangerous element in a public health movement. (Siegel 2007, 20)

The claims that Siegel is referring to are contained in his blog tobaccoanalysis.blogspot.com and interested readers might be rather surprised at the degree to which Siegel describes the "junking" of epidemiology. In brief, Siegel (2007) has been vehemently attacked for criticizing views of leading anti-tobacco activists on the extent of health risks to non-smokers. Siegel fears: "The dissemination of inaccurate information by anti-smoking groups to the public in support of smoking bans is unfortunate because it may harm the tobacco control movement by undermining its credibility, reputation, and effectiveness" (24).

A rather public fight is ongoing between Siegel and Stanton Glantz, with

21 The webpage containing the quotation is here.

the latter one of the leaders of the tobacco control movement and co-author of papers quoted here several times. As quoted in a recent *Boston Globe* article, Glantz states the following about Siegel:

> I view him as a tragic figure—he has completely lost it. His view is that everybody in the tobacco control movement is corrupt and misguided except for him. You have to be careful what you say to preserve credibility in academic circles, and he is not doing that. (Glantz qtd. in Beam 2007)

Carl Phillips, an epidemiologist at the University of Alberta, has also written about personal and financial attacks leveled against him, as he, too, has been vocal about what he perceives as junk science in the study of environmental tobacco smoke (ETS):

> There is little doubt that inhaling smoke in unhealthy, but equally clear evidence shows that we can only demonstrate disease risk from ETS for those at the highest level of exposure. The evidence about health effects of smoke and the legitimate aesthetic objection to involuntary ETS exposure are quite sufficient to justify prohibiting indoor smoking in public places, though clearly insufficient to justify public policies that prohibit voluntary low-level ETS gain. The activists involved, many of whom hold titles that indicate that they should behave as scientists and academics, appear unconcerned about subverting science to further their worldly agendas, hurting the careers of honest scientists, driving students away from politically controversial fields, attacking the principles of free academic research, and threatening the reputation of epidemiology as a field. (Phillips 2007, 7)

UCLA medical researcher James E. Enstrom also writes of extensive personal attacks following publication of his 2003 *British Medical Journal* paper that found little relationship between ETS and tobacco-related mortality. Enstrom (2007) documents his many claims that anti-tobacco activists have not properly presented evidence on effects of second-hand smoke and at times have also misrepresented evidence. The paper compares "current ETS epidemiology in the U.S. with pseudoscience in the Soviet Union during the period of Trofin Devisovich Lysensko" (2). Enstrom highlights Stanton Glantz as a major source of attacks.

INTIMIDATION AS A STRATEGY

Intimidation appears to be part of the strategy used against those who do not fully support anti-tobacco activists. In the Internet age it is pretty easy to learn about the lives of researchers. Recent inspection of SourceWatch's entry for UCLA epidemiologist Enstrom, just mentioned, reveals the following summary:

> Enstrom is a controversial figure who has accepted funding from the Philip Morris tobacco company and the Center for Indoor Air Research (a tobacco industry front group), and subsequently published research that contradicted scientific consensus about the health effects of secondhand tobacco smoke … (SourceWatch, link)

I recently read a paper published in a prestigious economics journal to remain nameless that called into question some of the claims of tobacco control activists. I enjoyed the paper and sent a quick email to one of the authors stating that I thought they had done a good job and sent along a suggestion where they might extend their research. Within a few hours I received an email back from one of the authors, but clearly the email was not intended for me to see. In it, one of the two authors wrote to the other author that *"I don't think we should get involved with this guy because I hear he has very deep connections with Phillip Morris."* I figured out pretty quickly that the email was supposed to be for their eyes only, but, by mistake, was sent to me as well. I had no doubts that they had located my listing under SourceWatch. I immediately sent the authors a reply email that simply read something like: *"Thanks for the quick reply and I appreciate your honesty,"* to which I received another email from one of the authors within a few minutes that read something like: *"I am very sorry for that, I hit the wrong button on my email program. We have to be very cautious about our work as we experienced very strong personal attacks to this paper we just published."* I ended the exchange with something like: *"Welcome to the club, I am not surprised, and best of luck."*

One more personal story appears appropriate. I have recently been publishing articles in peer-reviewed journals that question the influence of tobacco-control spending on cigarette consumption and smoking prevalence.[22] I have not received grants of any kind for this research from any tobacco-related entity. In fact, my grants from the tobacco industry ended quite awhile back, when I finished my work on smoking bans. Fortunately, I have been able to obtain data on tobacco-control programs from the Internet and, unlike smoking bans, lack of funding is not much of an obstacle to this research effort. My research finds that public spending exerts little effect on smoking consumption and prevalence, despite continual statements by anti-tobacco activists that spending not only signifi-

22 Marlow (2006), Marlow (2007), Marlow (2008A) and Marlow (2008B).

cantly lowers smoking, but programs are critically under-funded. I am sure these conclusions are not well received by anti-tobacco activists, even though I would have thought they would be interested in re-allocating funding in ways that might reach their objectives of eliminating smoking.

One of these papers, which I will not name, was accepted for publication after going through a fairly lengthy review period in which I made many good changes to the paper. A few weeks after its acceptance, I received an email from the editor of the journal stating that he had received word from someone that my paper did not acknowledge the fact that I was someone with strong ties to the tobacco industry and that it was unethical to not disclose this conflict to readers. Again, the source was probably SourceWatch's faulty information. The editor asked if this was true. I replied that, although I had received grants in the past for work on smoking bans, that I had not received any funding for this or related work on tobacco-control spending. I then added that I wasn't really keen on adding a disclosure that read that *"I had received grants in the past from the tobacco industry, but not for this paper"*. The editor agreed, and to his credit, said he was satisfied with my response and that no further action was necessary.

WHO WOULD FUND SUCH RESEARCH?

I have suggested that tobacco firms have aimed their funding toward research that focuses on how private markets work, while non-tobacco funding sources aim their funding toward research that shows that government intervention is effective. Further, I have suggested that ideological sensibilities differ between these two different research paths, and that still other biases might be in play. However, I doubt that the two opposing predispositions regarding the proper role of government somehow balance out. In the smoking ban literature, it is clear that there are many more studies showing that smoking bans inflict no harm on any business and, as it turns out, most of these studies are funded by dollars outside of the tobacco industry.

We should address the following question: Who else would fund studies that seriously examine how private establishments manage diverse preferences regarding smoking? Pro-ban researchers never collect such data nor give much consideration to private market efforts. Given the pervasive factors—the costly nature of data acquisition, the paucity of free-market researchers, the preponderance of non-smokers, the seductions of "good intentions," and the many funding sources available to those who prefer government intervention—very little research on private markets is conducted. If not funded by industries adversely affected by such intervention or think tanks interested in free markets, even less research would be conducted. Moreover, given the apparent scarcity of journals that have editors, officers, and referees interested in uncovering how well private markets manage resources, it's not particularly surprising that little research on private markets is conducted. I can just imagine how my proposals for conducting smoking-ban studies would have been received if I had submitted them to the many funding sources available to anti-tobacco activists.

An interesting view is expressed in Phillips (2007) regarding the strings attached to research funding:

> Anti-Tobacco activists have long coasted on the cigarette industry's misdeeds regarding producing illegitimate research. The substantial and deplorable misdeeds from thirty or forty years ago are well documented, and it is clear that the industry has been guilty of many of the same crimes against epidemiology practiced by anti-tobacco activists today: One result of that guilt coming to light is that claims by the industry are widely discounted, making them little present threat to honest science. Despite this, anti-tobacco activists are still trying to attribute epidemiology that they do not like to the (largely nonexistent) influence of the industry in the field. Another result of the guilt is that the industry's every move is carefully watched, making tobacco industry funding a professor's dream: the funder does not dare say a word to try to influence the research. (Phillips 2007, 12)

This account certainly accords with my own experience. No one was looking over our shoulders, or attempting to manipulate our research. The overriding issue was to publish peer-reviewed work and gain credibility. But, as discussed many times in this article, activists have not shied away from claiming that all research connected in any way to the tobacco industry must be incorrect and even deceitful. Thus far, this strategy has largely been effective in protecting them from coming to terms with opposing research.

All studies, whether sponsored by private or public monies, deserve scrutiny and a degree of skepticism. But the more central ethical principle of scholarly discourse should be, rather, what Wayne Booth (1974) calls "the rhetoric of assent," that is, an initial readiness to suppose that the other guy or gal is also fair minded and trustworthy, and deserves being heard out. I should not dismiss studies funded by anti-tobacco sources, and others should not dismiss studies funded by the tobacco industry. It's not a matter of either 100 percent trust or 100 percent skepticism. It is an initial inclination toward assent combined with a degree of fair and responsible skepticism after you've heard the other guy or gal out.

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. Go to May 2008 Table of Contents with links to articles

Go to Archive of **Character Issues** Section