Econ Journal Watch,
Volume 5, Number 2,
May 2008, pp 199-203.

Symposium: Gender and Economics

Mr. Max and the Substantial Errors of Manly Economics

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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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