March 1, 2002

Professor Ananth Seshadri
Department of Economics
University of Wisconsin
Social Science Building
1180 Observatory Drive
Madison WI 53706-1393

Re: Ms. 2001163 -- "Engines of Liberation"

Dear Professor Seshadri:

I enclose a report on your paper by an economist familiar with its topic. The referee has recommended against publication of your paper in the Review, and the referee has convinced me this is the appropriate decision.

Many thanks for letting us review your work. I am only sorry my response could not be more satisfying.

Sincerely,

Orley Ashenfelter
Co-Editor
Referee's Report: Engines of Liberation
Ms. 2001163-1

The purpose of this paper is to explain the rise in labor force participation of married women in the 20th century. The idea is the invention of labor-saving devices in the home (e.g. washing machines) has something to do with the increase in participation. A computable, dynamic general equilibrium model is constructed and calibrated in such a way that the increase in participation between two widely dispersed points in time can be explained by the diffusion of such labor-saving devices. I have the following comments on the present draft:

1. This paper is part of a small, but rapidly growing literature in which “RBC-type” macroeconomists attempt to understand long-term economic change. I think it is great that macroeconomists of this persuasion are becoming interested in economic history. There is, as well, a fairly long-standing tradition of applying general equilibrium arguments to historical issues, dating back to the Jones-type models used by Temin, Fogel, and others in the late 1960s and early 1970s, and somewhat later, Jeffrey Williamson (unfortunately the authors of this paper seem unaware of this tradition). I certainly think there is potential to apply the more complex dynamic models to many important, if not all, historical issues.

2. What this literature has going for it at the moment is novelty: currently there are no (well, maybe a few) economic historians capable of writing down one of these models in the morning and solving it, say, at lunch. At some point – fairly soon, I would predict -- the novelty will wear off, and we will have to answer the question, what does a paper like this contribute to the relevant literature, namely, the long-term rise in female labor force participation? The answer, unfortunately, is very little. Basically, what we have here is the construction of a specific model calibrated in a specific way that, as such, can “explain” the long-term rise in participation (we also have some silly cross-sectional regressions), where “long-term” means between two wide dispersed dates (this is rather important. I very much doubt that the model in this paper would have much, if any, explanatory power if account were taken of the timing (i.e. decade to decade) of the long-term rise in women’s labor force participation relative to the timing of the diffusion of labor saving devices). However, there are surely many other models capable of doing exactly the same thing (for example, I bet one can construct a model of multiple sectors in which tech change alters the economy-wide productivity of female labor, resulting in a rise in participation). There is as well a previous literature – most obviously, Claudia Goldin’s book, Understanding the Gender Gap. The authors cite this work but, on the basis of the paper, do not seem to have read it very carefully. Goldin, for example, emphasizes such factors as the elimination of marriage bars, the rise of the clerical sector, the historical equality of male and female school attendance rates, a small but positive role for WW2, the role of cohorts in facilitating change and so on. To be sure, one can criticize her work as not “rigorous” enough because she uses a blend of qualitative and quantitative evidence in the context of fairly simple partial equilibrium models and because her central econometric model treats some factors as exogenous
(such as fertility) to participation when they are surely jointly determined. But for this paper to make a real contribution it needs to nest the explanation at hand – labor saving devices in the home – in a model that encompasses other explanations as well, if we really want to measure the “treatment effect” of washing machines. Otherwise all we really have is some authors showing off their technical skill. It gets old very quickly.

3. Even taken on its own terms, this paper is an incredible mess – terribly written, and terribly organized. Casual language is employed throughout mixed with high-tech language out of place in mainstream journal. Wild assertions pepper the text, historical evidence appears at random with no apparent understanding of the context, research by sociologists and social historians is berated – I could go on. In short, this paper is nowhere close to being in a state where it could be considered for publication by any economics journal, much less a major one.

Minor comments:

1. p. 1. Section 1.1 should be labeled “Stylized Facts”
2. p.2, bottom, sentence “In 1890 ... year.” How do we know this?
3. p. 3, line 3, sentence “Ninety-eight ... 1900”. How do we know this?
4. p. 4, fn. 4. Should I believe this source? Why?
5. p. 5, tables. On the previous page we were talking about the early 20th century. Now we have data from the middle. Why is it relevant?
6. p. 7, par. 1. The evidence from Vanek, which is well known, suggests that, among married women who were working, there has been little change in the amount of housework per week over the 20th century. A really useful bit of economic history would be to investigate whether this fact is more stylized than real. The authors, however, accept the data at face value, pointing out that there still was a decline in average hours devoted to housework because of the compositional shift (married women who were working spent less time at housework, and “today” more married women work). This just misses the point.
   Why is it the there has been no decline in housework among married women who work? There are many possible explanations – here’s one off the top of my head. Married women who worked outside the home in the early 20th century were of two types: (1) they had dissolute, disabled, or even missing husbands, but older children at home capable of taking up the slack while they worked. Or they took in boarders, and were able to combine labor force participation of a sort with housework (2) they were highly educated, married to highly educated men, and had always worked – elites – with more than enough money to purchase the services of domestic servants. Indeed, I suspect what this paper is really about is the decline in the use of domestic servants among the upper middle classes, analogous to the decline in the use of typists among upper middle class economists, in favor of word processors.
7. p. 8, line 1, sentence “It seems ... participation.” Goldin, I think, comes to a different conclusion.
8. p. 9. The table from Durand is really just dumb. Surely the authors are aware that there is a vast literature on this issue in labor economics. However, the authors do
not seem to be aware of the problems with Durand's use of such evidence from the 1940 census; see, in particular, T.A. Finegan and R. Margo, Journal of Economic History, 1994.

9. pp. 10-17. Aside from the fact that the presentation of the model is not appropriate for the AER, the "insight" here does not need this much ammunition - or, giving the authors the benefit of the doubt, they haven't persuaded me that the model adds much. What is the insight? A married woman can do one of two things. She can stay at home doing the laundry by hand, or she can go to work, pooling some of her husbands' income and her earnings to buy a washing machine. These purchases are assumed to be lumpy, so maybe she will go to work a few periods before buying the machine to build up some savings (why she remains at work after the machine is purchased is not totally clear to me. Perhaps the user cost of the machine is such that it is necessary to do so). Married women and their husbands differ in ability, so the rich adopt the machines first. However, as noted above, rich married women had another option, historically - purchase the uses of a servant. And nearly everyone, at least in cities, had the option of sending laundry out (to the nearest Chinese laundry). This model, in other words, is not complicated enough, because it fails to consider important substitutes for home production. Perhaps what really matters is the relative price of washing machines, where relative is in comparison with servants' wages or the price of washing at the local laundry.

10. p. 17, line 16, sentence "Hence ... direction." Except, I don't think it was "non-neutral". It tended to be the sort of technical change that lowered the marginal productivity of "strength" in favor of "brains". Again, see Goldin.


12. p. 23. Footnote 17 is just offensive.

13. p. 23, line 3, sentence "It seems ... Revolution." Huh?

14. p. 28, top. A regression like this might not be out of place in an undergraduate term paper. But why should professional economists in 2002 take this seriously? If the authors want to use cross-sectional type evidence, they should do so seriously. In this regard, perhaps they might consider looking at the 1918-19 or 1934-36 BLS cost of living studies, which may contain usable micro-data.

15. p. 28, line 13, "The price ... sector." Why should anyone believe this? Just because the authors say so?

16. p. 29, line 3, "Second ... here." I don't follow.

17. p. 31, top. Caselli and Coleman is already published.

Final comment: I do hope the authors continue to think about applying dynamic GE models to historical issues. I also hope that they spend more time with knowledgeable economic historians - or, if none are close at hand, persuade their colleagues to hire one.