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Comment on L. H. Summers, "The Scientific Illusion in Empirical Macroeconomics"

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The main theme of Summers's paper is the role of empirical work in the development of economic science. Summers has written a good paper that addresses a very important problem. I agree with much of his description of the problem. Econometric studies have little impact on the development of macroeconomic theory. Theoretical model-builders often wander aimlessly in the universe of possible theoretical economies, without any guide in empirical observation.

I am less enthusiastic about the recommendations. The advice to "learn the lessons from history", or to "report relevant information and apply common sense" is too vague to be useful. Morever, every economist must agree that we should use all the relevant evidence in order to evaluate our theories. That we should try to learn from "natural experiments" is equally self-evident; data that varies is better than data that does not.

Summers is right, that the best way to guide economic theory is to establish stylized facts: results that are reasonably simple and reasonably robust across time periods and countries. The question then is: *How do we best generate and convey stylized facts that are useful for the development of macroeconomic theory?*

In general, my comment is a defence of econometric work based on "stochastic pseudo-worlds" with "representative agents". *In my view, the answer to the problems described by Summers is not that empirical work should loosen its ties with economic theory.* Instead, the most efficient way to search for useful stylized facts is to estimate equations, which have been derived from simple and precise models. A shift in emphasis is needed, however, from formal statistical tests to examination of explanatory power, robustness, and plausibility of the estimated parameters. In particular, more effort should be used to clarify which facts are and *are not* consistent with existing economic theory.

My comment consists of two parts. In the first part, I present a particular stylized fact that did have an impact on theory, and discuss why this was so. In the second part, I discuss Summers's criticism of modern econometric practice.

Table 1. *Papers which quote the real wage-employment correlation*

Phelps, E.: Introduction, in *Microeconomic Foundations of Employment and Inflation Theory*, Norton, New York, 1970.
Barro, R. and Grossman, H.: A General Disequilibrium Model of Income and Employment, *American Economic Review 61*, 82–93, 1971.
Azariadis, C.: Implicit Contracts and Underemployment Equilibria, *Journal of Political Economy 83*, 1183–202, 1975.
Lucas, R.: Understanding Business Cycles, in Brunner, K. and Meltzer, A. (eds.), Stabilisation of the Domestic and International Economy, *Carnegie-Rochester Series on Public Policy 5*, 7–29, 1977.
McDonald, I. M. and Solow, R. M.: Wage Bargaining and Employment, *American Economic Review 71*, 896–908, 1981.
Shapiro, C. and Stiglitz, J. E.: Equilibrium Unemployment as a Worker Discipline Device, *American Economic Review 74*, 433–44, 1984.
Pissarides, C.: Short-Run Equilibrium Dynamics of Unemployment, Vacancies, and Real Wages, *American Economic Review 75*, 676–90, 1985.

A Stylized Fact that Mattered

Table 1 lists some important contributions to the theory of the supply side in macroeconomics. All the papers on the list quote a particular stylized fact: the absence of a systematic correlation between real wage and employment fluctuations. This is not the place to discuss whether this stylized fact is actually true.[1] What is interesting now is that this perceived stylized fact had a large impact on macroeconomic theory. Let me therefore look more closely at this example and try to draw some conclusions of general validity.

First, it was not an accident that empirical economists examined the correlation between the real wage and employment. Empirical researchers saw a negative correlation as a testable implication of the simplest possible model of labor demand of the representative firm. I claim that, in general, the most efficient way to look for *useful* stylized facts is to start from existing economic theory and examine its implications. For example, by estimating Euler equations, we learn something about the (conditional) correlation of the rate of growth of consumption with the real rate of interest; see Hall (1978). This is useful for anyone who wants to evaluate the consumption theory that is taught to first-year graduate students, and that is standard in macroeconomic theory.

Second, this stylized fact was useful because it said something about the relevance of existing economic theory. In fact, Phelps, Barro–Grossman and Pissarides quote this result as evidence that wage and employment

[1] For references to the empirical literature, see Blanchard and Fischer (1989).

observations are not on a standard labor demand curve, while Azariadis, McDonald–Solow and Shapiro–Stiglitz take it as evidence that they are not on a standard labor supply curve.

Is this a correct interpretation of the evidence? Yes, but only under "heroic" aggregation and identification assumptions. Theorists implicitly assumed that other variables which affect labor demand (supply) are uncorrelated with real wages. It should be better to make identifying assumptions explicit and take account of other variables which belong in the equations. In this (static) model, this is equivalent to estimation of deep parameters.

Of course, it is somewhat surprising that none of the listed papers quoted the results of *estimation* of, say, labor demand functions. A probable reason is that attempts to estimate textbook labor demand functions failed, so the papers were never published.

This brings me to my third point: negative results are useful. The empirical researchers can be thought of as trying to estimate a standard labor demand curve with OLS, with negative results. We should not be surprised that this negative result is quoted by the theoretical papers; it is negative results that motivate the development of new theories. By estimating equations which are closely related to theory, we can learn what facts can — and cannot — be explained by existing theory.

Fourth, if stable correlations can be established, this is certainly very useful. Theory warns us, however, that many correlations are unlikely to be stable. For example, the correlation between the real wage and employment should depend on the relative importance of supply and demand shocks, which has varied across time periods and countries.

In the same way, we should not expect the correlation between the change in nominal money supply and real output to be stable. Standard theories tell us that the correlation should be different, depending on the monetary policy pursued. The same logic applies to a VAR or an error correction model relating money and output: they should not be stable across policy regimes. Thus, we may search in vain for simple and stable correlations, even if essential elements of preferences and technology are stable.[2]

Fifth, theory is needed in order to decide what empirical variables should be looked at. Neither the "real wage", nor "employment", are unambiguous concepts. Recent empirical applications of labor union models have been careful to distinguish the product real wage, which is relevant to the firm, from the consumption real wage, which is relevant to

[2] Friedman (1986), who searched for stylized facts using atheoretical methods, found that the correlation of M1 growth with economic activity was significantly positive for the periods 1947–65 and 1965–82, but not for the whole period 1947–82. Such instability may very well reflect a change in monetary policy in the late 1960s.

the worker. Some studies suggest that while there is little correlation between fluctuations in real wages and *hours* worked, there is a positive correlation between the real wage and the *number of* persons employed; see Alogoskoufis (1987). To measure labor input, differences in human capital (marginal product) between different workers should be taken into account; see Kydland and Prescott (1989). These issues matter, and someone who takes theory seriously will worry more about them than someone who is searching for stylized facts without the benefit of theory.

Summers's Criticism

I now turn to Summers's specific criticisms of empirical work based on representative agent models. The issue is *not* whether people are rational and forward-looking or whether the economy is in a rational expectations equilibrium. These issues are separate from the one raised by Summers: whether it is useful to estimate equations derived from simple models with representative agents.

(i) Uninformative Tests

I agree that formal statistical tests of tightly specified models (e.g. Euler equations) are often uninformative. On the other hand, purely statistical rejections should not be held against these models. The models are obviously not true and we should expect them to be rejected statistically. They may still be very useful.

To evaluate such models, we should focus on reasonableness of parameter estimates, explanatory power, and on the quantitative importance and nature of the rejection. How well does the theory fit the facts? What correlations are/are not consistent with the theory? What events can/cannot be explained by the theory?

The reason why many papers in this tradition are uninformative is that the authors do not spend enough effort on such an evaluation. The paper by Mehra and Prescott (1985) is more informative than the one by Hansen and Singleton (1983), not because Mehra and Prescott are less rigorous, or because they use a less formal testing procedure, or because they look at means rather than covariances. The important difference is that the deviation from theory reported by Mehra and Prescott *can be understood*, while Hansen and Singleton tell us nothing about *why* the restrictions were rejected. But this could have been done.

(ii) Robustness

Empirical results are often met with scepticism, not only because of the well-known data-mining problem, but also because it is very easy to make serious mistakes when carrying out empirical work. Results may be affected by outliers, unexplained trends, special events, changes in data definitions or institutional changes, not to speak of typos in the program! If empirical work is to have an impact, much more effort should be devoted to making empirical papers persuasive.

The best way to convince me that a result is not an artifact is often to present the data in some informative diagrams. But diagrams should be used as a *complement* to formal statistical analysis. While diagrams convince us that the results of formal tests make sense, formal tests tell us whether what we think we see in the diagrams is statistically significant.

(iii) Identification

I have difficulty understanding Summers' view of identification. He criticizes Bernanke for making implausible identifying assumptions, but Modigliani's consumption function is not scrutinized in the same way. He seems to suggest that the pragmatic economist could "learn the lessons of history" without worrying about identification. Anyone with a basic knowledge of econometrics should know that this is not true.

Whenever we want to answer *economic* questions, we have to make identifying assumptions. Such identifying assumptions can only be motivated by reference to a structural model. Only after we have postulated a "stochastic pseudo-world" can we answer the economically interesting questions about cause and effect. Identifying assumptions are always doubtful, and can always be criticized. Whatever economic conclusions we draw are conditional on the identifying assumptions, and alternative interpretations of reality are possible. Thus, we can never hope to "prove" any economic claims with econometrics.

In my view, the important criticism of most papers in the VAR and error-correction traditions — including "structural VAR" models — is that they *obscure* the issue of identification. Instead, we need clear statements and motivations of the identifying assumptions; see further Cooley and LeRoy (1985).

(iv) Statistical Technique

In principle, there is no conflict between statistical technique and common sense. It should be possible to take economic theory seriously, take statistical issues seriously, take data problems seriously and establish useful stylized facts. The real problem is that doing all these things is beyond what most of us can manage. Like Summers, I think that too little time is spent understanding the broad patterns in the data — compared to the time spent on sophisticated statistical techniques.[3]

[3] Many so-called "applied" econometric papers read as if to say, "Look! This fancy estimator can be calculated with real numbers" — as if anyone doubted that.

On the other hand, statistical issues sometimes do matter. I conclude that doing good empirical work is very time consuming. There is no quick and easy way to do it. Good empirical work is most likely to result from cooperation between an economist who knows econometrics and an econometrician who knows economics, where both have an interest in learning about reality.

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