Comment on "Laboratory Experiments with an Expectational Phillips Curve," by Jasmina Arifovic and Thomas J. Sargent.

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1. Laboratory experiments in macroeconomic environments

Experiments like the ones undertaken in this paper are rare. The conventional wisdom has been that controlled laboratory environments are infeasible for macroeconomic questions, since we are attempting to understand how a very large number of individual households and firms interact to produce the prices and quantities we observe in the data. While this conventional wisdom is, of course, true at some level, there is more to the story. There are good reasons to take experimental macroeconomics very seriously. Mainly, we need to obtain laboratory confirmation of predictions from *simple* models before we can hope to correctly infer what forces are at work in large, industrialized economies.

We economists often write down simple models in an effort to get some core economic intuition concerning topics of interest. The literature begun by Kydland and Prescott (1977) and continued by Barro and Gordon (1983), Rogoff (1985), Walsh (1995) and many others is one outstanding example. In these simple models, we presume to know how human beings confronted with the environments we construct would act. Laboratory experiments can help us to understand whether such presumptions are warranted or not. And, in fact, the current paper calls those presumptions into question.

The literature begun by Kydland and Prescott (1977) is perhaps the most influential for central bankers during the last 25 years. It provides the leading explanation of why there is so much inflation among the industrialized countries of the world today. The word "credibility" rolls off of the tongues of seemingly everyone connected with monetary policymaking. In this literature, the presumption that the economy's participants would coordinate on the time-consistent, high inflation, Nash equilibrium of the model has been almost axiomatic. Only the recent challenges of Cho and Matsui (1995, 1999), Sargent (1999), and, less formally, McCallum (1995) and Blinder (1998) have even questioned this assumption. That is why the current paper provides such an important service: The main finding is that the Nash equilibrium is not consistently sustained in the laboratory. What

is sustainable, and how to interpret it, presents us with a challenge.

2. Three theories

The environment considered in this paper can be viewed as encompassing three theories. The most natural one is a basic version of the Kydland and Prescott (1977) economy, in which time inconsistency problems lead to a Nash equilibrium characterized by an inflation bias. I want to caricature this theory as simply predicting that the Nash equilibrium will be sustained in the laboratory implementation, perhaps after some transition dynamics that are not in the neighborhood of the Nash outcome.

The existence of a Ramsey outcome characterized by a distinctly lower inflation rate facilitates interpretations of the experiments in terms of Cho and Matsui (1995, 1999) or, perhaps closer to the intent of the authors, Blinder (1998) and McCallum (1995). These theories suggest that the Ramsey outcome could be sustained, but the details either do not exist or are not implemented here. Thus the link between these theories and the actual laboratory results is somewhat tenuous. According to McCallum (1995, pp. 208-209), "... the central bank [could] ... recognize that its objectives would be more fully achieved on average if it were to abstain from attempts to exploit ... temporarily given expectations." Possibly, the human subjects playing the central bank in the laboratory could make such a leap of faith and simply play Ramsey. I want to caricature this group of theories as predicting sustained Ramsey outcomes in the laboratory, possibly in conjunction with some initial transition dynamics.

Finally, the environment here has many of the ingredients of Sargent (1999), where the policymaker's use of a misspecified model and an approach to learning characterized by the discounting of past data leads to a system in which both the Nash equilibrium and the Ramsey outcome are visited on a recurrent basis. The Sargent (1999) dynamics involve a relatively long time spent in transition from the Ramsey outcome to the Nash equilibrium, while the time spent in tran-

sition from Nash to Ramsey is relatively short. The details of these dynamics are sensitive to parameter choices. In addition, the assumptions concerning the knowledge available to the government and to the private sector differ in the laboratory experiments relative to Sargent (1999). Thus it not that clear what the predictions from Sargent (1999) actually are and whether they map clearly into the experimental design examined here. Nevertheless, I want to caricature the Sargent prediction as one where the laboratory systems display considerable time, beyond initialization time, in transition between Nash and Ramsey, and more so from Ramsey to Nash than from Nash to Ramsey.

3. Results

The main results that I want to focus on are as follows. The laboratory systems tend to spend a good deal of time in the neighborhood of the Ramsey outcome. There is evidence that systems sometimes achieve the Ramsey outcome, but then "backslide" or creep toward a Nash equilibrium. In general, the Nash equilibrium is not often observed in these experiments. Based on these results, which theory is best supported by the laboratory data?

The most striking finding is simply that the Nash equilibrium is not observed on a sustained basis. Thus the basic Kydland and Prescott (1977) prediction is disconfirmed in the laboratory. If such results hold up in future experiments, it will be a crushing blow to the leading theory of why we have observed so much inflation in industrialized economies during the post-WWII era. How many papers have been written assuming, in similar environments, that the Nash equilibrium could be sustained? Those assumptions are simply not supported by the laboratory data assembled here.

It is not as clear as the authors suggest that we can effectively distinguish between the two remaining theories based on this data. Sargent's (1999) theory has wide-ranging predictions. A relatively long, sustained period at the Ramsey outcome could be consistent with this theory; not observing much time at a

Nash equilibrium could also be consistent. The slow transitions toward a Ramsey outcome are not consistent. As for the Blinder (1998) and McCallum (1995) positions, it is not clear how they can account for the non-Ramsey outcomes observed in this data.

It seems clear that to frame these questions more appropriately, one needs to obtain more detailed predictions from a specific version of Sargent's (1999) model, and implement that version of the model in the laboratory. The rapid escapes from the Nash equilibrium, for instance, are relatively rare events in Sargent (1999), and I am not sure we should expect to observe one in experiments of this length. With a more specific implementation of the Sargent model, one could calculate an expected time to escape and use that to interpret the data. Similarly, backsliding from Ramsey also takes a good deal of time. But, again, one could obtain a more detailed prediction from a specific implementation of Sargent, and then compare that prediction with the data.

Experiments 9 through 12 involved a better compensation scheme for the policymaker. These systems spent much less time in a neighborhood of the Ramsey outcome. This seems like a significant finding, and the authors should discuss it in more detail.

4. Some alternative experiments

Experiments beget experiments. Many more laboratory implementations of the Kydland-Prescott model need to be executed before we can be fully convinced that the Nash equilibrium is not the right prediction. To get a better approximation to the point of view emphasized by Blinder and McCallum, the authors may want to consider experimental designs where subjects playing the policymaker role are familiar with ideas from the monetary policy games literature. This seems to be part of McCallum's (1995) critique. To more closely match the spirit of the Sargent (1999) model, where policymaker learning is crucial, experimental designs which involve pitting a policymaker against a robotic "rational expectations" private

sector might be interesting, instead of letting both sides learn as in the current implementation. Such experiments would also have the benefit of being cheaper and easier to run. In general, future research might focus on more general setups that have exact counterparts in the lab.

5. Conclusion

This is a problematic paper from the perspective of the monetary policy games literature. It brings empirical evidence to bear on the predictions from widely-used, simple models, evidence which does not square well with traditional interpretations of the theory. I do not think that the results here are definitive, but on the other hand we economists are not so successful that we can afford to ignore evidence from controlled experiments.

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