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Discussion

A number of participants raised questions concerning the model's calibration, especially in relation to the parameters μ_b, μ_h, and μ_k, which control the pledgeability of bonds, housing, and capital, respectively. The first was José Scheinkman, who echoed a comment made by Jean Tirole that in practice, T-bills are much more pledgeable than is suggested by the authors' baseline value of 0.45 for μ_b. He explained that even during the recent crisis in Greece, money markets were accepting assets as collateral with much lower haircuts. He also argued that it might be inappropriate to use the ratio of home equity loans to home equity as a target for calibrating μ_h, because that target does not adequately reflect households' ability to obtain credit at the margin. For example, a historically low value for that target may just reflect a low demand for credit; if so, then the value of μ_h should be larger than the authors' baseline of 0.06.

Randall Wright defended the authors' calibration of μ_b by arguing that even if some agents face lower haircuts on bonds (higher values of μ_b), other agents may not. Many households may hold bonds indirectly through intermediaries such as pension funds, for example, and may not be able to easily access those assets for use as collateral. Since their model features a representative household, a lower value for μ_b is needed to help account for this heterogeneity. Still, he agreed that modeling the heterogeneity more explicitly would be interesting as well. He also expressed interest in using microeconomic data, such as those collected by the Bank of Canada and the Federal Reserve Bank of Boston, to improve the calibration of their model.

Venky Venkateswaran responded to Scheinkman's suggestion that the ratio of home equity loans to home equity might be low due to

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a low demand for credit. He argued that such a hypothesis must be reconciled with the fact that households hold a large fraction of their wealth in assets with returns close to zero. In the model, this behavior can be explained if low-return assets have more favorable pledgeability characteristics; however, this would imply that households' demand for credit may not be as low as Sheinkman suggests.

Michael Woodford suggested calibrating the pledgeability parameters by observing how the average return of each asset varies with inflation, rather than by trying to observe the extent to which each asset is pledgeable or is being pledged. He also pointed out that the model implies the existence of a particular monetary aggregate that should not generate the types of anomalies often encountered when estimating money demand functions. According to the model, this aggregate should be a weighted average of all the assets in the economy, with weights equal to the same parameters that govern the pledgeability of each asset. Even without direct evidence on the pledgeability of individual assets, therefore, he argued that the model nevertheless makes a tight set of predictions that would be worthwhile to investigate further.

Wright agreed that one valid approach would be to construct a monetary aggregate along the lines suggested by Woodford, and then proceed to estimate money demand functions. However, he argued that a preferred approach would be to jointly estimate an "interrelated asset demand system," analogous to the standard interrelated factor demand systems that have often been estimated for firms as functions of input prices. However, Woodford noted that the model still makes the strong prediction that all the assets are perfect substitutes in the way that they affect the supply of liquidity in the economy. Wright agreed, but replied that the pledgeability parameters themselves might somehow be endogenous.

Venkateswaran responded to a question raised by Jean Tirole during his prepared remarks concerning the degree to which the authors' choice of 1.3 for the markup in the Kiyotaki-Moore (KM) market affects their conclusions about the equity premium. He explained that their calibration of the markup does not directly affect their findings regarding the equity premium. The equity premium in the model is generated primarily by the fact that bonds yield a real return (r_b) of only about 2 percent, relative to a real return (r) of 5 percent on illiquid bonds, so μ_b must be high, based on the first-order condition (ignoring taxes): $(1 + r_b)(1 + \mu_b i) = (1 + r)$.

Sydney Ludvigson asked the authors to comment further on their calibration of μ_b in relation to μ_k, in particular as it relates to the fact that the spread between average equity returns and average returns on illiquid corporate debt has been found to account for most of the equity premium. Venkateswaran acknowledged that he had not checked the data on corporate bonds, but explained that since $\mu_k = 0.17$ and $\mu_b = 0.45$ in their baseline calibration, the pledgeability of corporate bonds would need to lie somewhere within that range. However, he noted that if Ludvigson's fact is correct, then their calibration strategy would imply that the pledgeability of corporate bonds would have to be somewhat closer to that of T-bills (μ_b).

Giuseppe Moscarini encouraged the authors to think more about the role of banks in their model. He suggested a mechanism by which their model might generate a "collateral multiplier": inflation can drive households to buy more housing capital and increase the value of houses, and then banks might react by creating more inside money, reducing their reserves, and lending more. He also noted that the authors would probably need to calibrate their model to measures of money other than just M1 or M2 in order to pursue this mechanism further. Wright explained that the pledgeability parameters stand in for the banking sector in the model, but nevertheless agreed on the importance of making banking more explicit.

Another thread of the discussion centered on a plot shown by Varadarajan Chari during his prepared remarks, which showed that the correlation between inflation and the long-run component of the capital-output ratio is not strongly positive if those series are computed using a different (but in his view equally appealing) approach. Woodford noted that the capital-output series used by the authors is very high during the late 1970s and early 1980s, but asked whether the alternative series computed by Chari is still unusually high during this time period. Chari explained that according to his series, the capital-output ratio is somewhat higher during that time period, but does not rise quite as much as the series that the authors use, and that the peak of his series is also shifted to the right.

Venkateswaran pointed out that in the theoretical model, the capital-output ratio is only affected by quantity adjustments, because the relative price of capital is fixed at one. In the data, however, the capital-output ratio might be moving partly due to a quantity effect and partly due to a relative price effect. He interpreted Chari's plot as demonstrating that both effects have played an important role over the sample. Moscarini agreed that the increases in the capital-output ratio from 1973 to 1974 must be due to changes in relative prices, because the changes during that year are too large to be driven by changes in quantities.

Chari argued that the difference between the two series may actually be more complicated than that. He briefly explained how the Bureau of Economic Analysis constructs the series on nominal private nonresidential fixed assets: they begin with nominal investment at subaggregated levels (e.g., laptops), then use a perpetual inventory method together with a price deflator for each asset to construct a real asset series, and finally add together all the real asset series and multiply by an aggregate price index. He expressed concern about the repeated use of different price indices in this procedure, and argued that the chained series he employed in his calculations is closer to the original measure of real assets.

Simon Gilchrist suggested an alternative method of uncovering the relation between inflation and the capital-output ratio, which does not depend on treating the capital-output ratio as observable. Given observations on the long-run effect of inflation on the real interest rate, it is possible to compute the model-implied effect of inflation on the capital-output ratio that is consistent with those observations. Jonathan Parker remarked that there are several mismatches between the theory and the data. First, he argued that as a result of chain weighting, the measure used by Chari would not even be guaranteed to be stationary in a well-defined model. Second, he explained that the theoretical exercise seems to imply that there should only be one (common) trend in capital and output, but in the data these two series are likely to have different trends.

Wright interpreted the two different measures as being qualitatively similar, but quantitatively different, with Chari's measure being more volatile. He also remarked that when the capital-output ratio increases, it does so partly because of increased investment, and partly because inflation can lower output. He then explained that other empirical studies have found that housing is also correlated with inflation. Originally, he had speculated that there might be some substitution between capital and housing, but such substitution would imply that capital should decline in response to inflation, contrary to their finding. In this model, however, both capital and housing increase in response to inflation. This occurs through a Mundell-Tobin effect: following increased inflation, households choose to divest themselves of currency and accumulate other assets, including both housing and capital. He therefore claimed that the mechanism in their model does a good job capturing the broad observations present in the data. He also argued that the empirical issue raised by Chari is not a problem for the theory. In their baseline calibration, capital is fairly pledgeable. If instead capital is not pledgeable, it will not respond to inflation, which would be in line with Chari's findings. Chari was not convinced. He argued that based on his measure, one might be led to write models where the steady-state capital-output ratio does not change with inflation. In his view, the evidence is mixed, and much more ambiguous than the paper makes clear.

Wright also offered some general comments in response to other concerns raised by Chari and Tirole during their prepared remarks. He began by clarifying that the paper is not meant to provide a new methodological contribution. He recommended that readers consult Williamson and Wright (2010) for more details on the methodology behind the paper. Instead, the focus of the paper is on its substance. Specifically, they show that inflation is not superneutral through a particular version of Mundell-Tobin effects, and attempt to explore the quantitative implications of that. For now, they have focused on the long-run properties of the model, but he agreed that the next step would be to do a more thorough time series analysis in this type of model.

Next, Wright argued that the theory of the second best is quite important in many macroeconomic contexts; for example, in determining what actions the monetary authority should take when the tax authorities are operating to satisfy independent objectives. The model is not meant to be a deep theory, but predicts that households are underinvesting in capital when realistic capital tax rates are included in the model. Therefore some inflation will be beneficial, since it increases the demand for capital. However, he disagreed with Chari that a goal of their analysis should be to understand how to conduct monetary policy over the business cycle. Instead, he emphasized that it is more desirable for policy to have good operating characteristics over the long run: for example, avoiding episodes like the recent recession, or stagflation. He argued that open market operations are not a panacea for the business cycle, and that inflation can affect consumption, investment, and possibly employment in the long run.

Wright also expanded on an externality emphasized by Tirole in his prepared remarks, which is referred to in the paper as the "paradox of liquidity": at the level of the individual household, it may be optimal to relax borrowing constraints by increasing investment in pledgeable capital. If all agents behave in this manner, however, the rate of return on capital can fall enough to tighten credit conditions.

Finally, Wright explained that the broad contribution of their paper can be understood in relation to the models developed by Kiyotaki and Moore (1997) and Bernanke, Gertler, and Gilchrist (1999). While both models present interesting mechanisms, he argued that they only have limited quantitative success unless many additional frictions (such as sticky prices) are also introduced. In this paper, the authors take the idea of credit constraints introduced by Kiyotaki and Moore (1997), but place the central friction on households rather than on firms. In his view they find that many of the features in the data become easier to understand as a result. He therefore concluded that the paper represents a step in an interesting direction.

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