Macro’s Newfound Interest and This Paper. Macroeconomics has become interested in market power. A series of studies over the past few years has documented a set of possibly interrelated, broad-based, and decades-long trends: increased market concentration, higher profit rates, higher measured price-cost markups, decreased investment rates, reduced firm entry and factor market dynamism, and a fall in labor’s share of income. If one wanted to offer a single, plausible-on-its-face explanation for these trends, it would be reasonable to argue that there has been a broad increase in market power among producers in the economy. This interest in market power extends beyond just product markets. Characterizing the role of monopsony, especially in the labor market, is an active research area as well.

However, there are potential alternative explanations for many of the trends described above. These include a growing role for intangible capital in production, increases in product market substitutability due to the expansion of trade or decreases in consumer search costs, and other shifts in production technologies that have increased returns to scale. Moreover, a set of studies has offered evidence for these mechanisms—in case studies, certainly, but in more broadly scoped empirical settings as well.

I view the goal of the Covarrubias, Gutiérrez, and Philippon paper as trying to bring together and make sense of those many data patterns and conflicting stories. On the theory side, the paper shows how a commonly used class of models captures many of the proposed explanations for the aforementioned data trends, and it uses such models to point to possible empirical tests to discriminate among these explanations. On the empirical side, it applies these tests in an attempt to identify the most likely explanation for the data trends. (Though as I note below, the collage of statistical analyses ultimately tells a multi-causal story, so in the end I might say the paper points toward the more likely explanations).

Comment 1: This Is a Useful Class of Models. This paper employs a very useful class of models, with substantial “empirical bang for the theoretical buck.” Key elements of this class include heterogeneous-type producers, a demand system that allows for product differentiation and interactions among firms that hold market power (this power is parameterized or microfounded depending on the setting), a zero-cutoff-profit condition that selects on heterogeneity (i.e., a firm must be of sufficiently high type to operate profitably), and free entry among ex-ante homogenous potential entrants that pay a sunk entry cost to learn their type. The combination of the zero-cutoff-profit and free entry conditions endogenously determines the equilibrium set of producers. This structure allows considerable richness in the patterns of industry outcomes that it admits as possible while remaining tractable and transparent.

Comment 2: The Problem with Concentration. It is heartening to see the paper’s recognition of problematic nature of using concentration to measure competition. As the field of industrial organization realized several decades ago, concentration is a market outcome, not a market primitive. Depending on the setting, concentration can be associated with less competition or with more. For example, under Cournot competition among heterogeneous-cost firms selling a
homogenous output, the share-weighted price-cost margin is proportional to the Herfindahl-Hirschman index of concentration. In this case, concentration is associated with less competition (higher price-cost margins and lower welfare). On the other hand, in the differentiated-product class of models used in this paper, increases competition due to greater product substitutability lead to more concentration but lower price-cost margins and higher welfare. Thus concentration is not just a noisy barometer of competition; we do not even know which end of the barometer should be pointed up. Nor is this just a theoretical issue. Hundreds of studies have found that increases in substitutability (whether through reduced costs of trade, transport, search, or switching) resulted in both lower margins and greater concentration, as the more capable firms become larger in response to increased substitutability. Even studies within the newer macro market power literature have found, at least for some sectors, simultaneous concentration and productivity growth.

This causal inference problem holds generally but is perhaps especially dangerous if one is making comparisons across different markets, when there is considerable scope for variation in primitives to drive concentration and competition in multiple directions. If one wants to study differences in competition, one should look at the primitives.

The paper recognizes this issue and nicely elucidates it with its conceptual framework. It then frames the empirical patterns as indicating mechanisms working in both directions. The paper argues that early in the sample period (before the 2000s), increases in concentration reflected greater competition. On the other hand, the new millennium saw further concentration associated with changes in primitives that reduced competition. I would characterize the underlying evidence as suggestive but not dispositive; still, it certainly points the literature in useful directions.

**Comment 3: The Macro of Markups.** Given the problems with using concentration to measure market power, a natural question is how one should then measure them. Price-cost markups are the textbook definition and measure of market power; as such, I view them as the most direct measure of that power.

The use of markups highlights three quantitative issues with results found so far in the macro market power literature: the alignment of aggregate price, markup, and cost growth; the relationship between markup, profit share, and scale elasticity; and the fact that factor market power implies same marginal-product-to-marginal-cost wedge often used to measure markups.

I describe the first two issues in detail in Syverson (2018) and as such refer interest readers there. In short, one involves the seeming inconsistency between historically low inflation growth on the one hand and unusually high markups growth and either steady or perhaps unusually high growth in costs. Given that the latter two sum to the first, there is a question as to how these relative trends all add up, so to speak. The other issue is that there is a very general relationship that should hold at the firm level among the markup, pure profit’s share of income, and the scale elasticity. Because these objects are often measured using at least somewhat independent data and sources of variation, this relationship offers empirical discipline that I
believe the literature would find useful. The authors address this relationship in the final section of the paper.

The third issue involves the relationships among product market power, factor market power, and a common measure of markups. This measure, the ratio of the estimated marginal product of a variable input to its revenue share, is a standard metric of product market power in the literature. This is because this ratio equals the price-cost markup of output under the assumptions of imperfect competition in the product market and a perfectly competitive market for the variable input. However—and here is the inference problem—the same ratio equals the monopsony markdown in the wage of the variable factor if the product market is perfectly competitive and producers have market power in the factor market. If firms have market power in both the product and factor markets, then the ratio mixes these two effects in its sum. Therefore reading the ratio as reflecting solely product market monopoly or factor market monopsony could misattribute one for the other. Moreover, even when recognizing that the measured ratio may reflect both market power effects, additional variation and empirical metrics are necessary to separately quantify each component.

Comment 4: Churn. Much of the empirical work in the paper involves the helpful collection and presentation of patterns that have been shown elsewhere in the literature, including in earlier work by the authors. One of the more novel empirical results here regard competition dynamics. The paper documents several notable trends since the late 1990s: a decline in the hazard rate of one of the top four firms in an industry (whether measured by revenues or market value) falling out of that position; a rise in the persistence of sales and market value ranks and shares (among all firms, not just the top four); and in a separate paper by the authors, a reduction in the responsiveness of entry to Tobin’s Q.

I agree that exploring the dynamic implications of the market power hypothesis is a potentially very fruitful research channel, and results of this type might bolster the case for market power. However, there is reason to be careful about drawing strong conclusions from the particular results in this paper. They are obtained from Compustat data, which of course comprises publicly listed firms. As is well known, publicly listed firms in the U.S. have experienced certain trends that are notably different from the broader universe of firms. In particular, the number of listed firms has been in secular decline since peaking in the latter half of the 1990s. The implications for the paper’s empirical tests on churn are apparent. Listed firms are an increasingly selected and, likely, increasingly stable set of firms. It is therefore perhaps not surprising that the dynamism of this sample (and entry in particular) would have declined, even if market power effects were absent. Indeed, it is interesting that the time series patterns of churn obtained in the paper show a similar rise-and-fall pattern over the past 30-40 years that the number of publicly listed firms did.

All that said, it is useful to remember that there is separate evidence in the literature that dynamism has fallen even among privately held firms. However, that also seems to reflect an
ongoing trend since at least the early 1980s, which means it has run through both the increase and decrease in churn measured in the paper.

**Comment 5: Sutton’s Endogenous Sunk Costs.** The paper treats sunk costs as exogenous, and argues increasing sunk costs are best explanation for several patterns in data. A sunk cost story that might be an even better description of what might be going on is the endogenous sunk cost of Sutton (1991, 1997).

In Sutton’s framework, incumbents employ an “escalation mechanism” to raise the sunk costs that a firm must pay to enter the industry. If this mechanism is potent enough, then little or no additional entry may occur despite substantial market expansion. In the limit, the number of industry firms can be bounded from above even if the market grows several times over. In Sutton (1991), the escalation mechanism is advertising and marketing. Suppose brand capital is necessary to compete effectively in a market. If brand is sufficiently responsive to investment, entry may stall even as the market continues to grow. An example of this might be the carbonated soft drinks industry, where two firms have dominated even as total consumption grew several times over. The duopolists’ continuous brand investments (to the tune of billions of dollars per year), interacted with consumers’ particular responsiveness to brand effects in this market, have combined to keep out any entrants. In Sutton (1997) the escalation mechanism is R&D and vertical product improvements. Here, commercial aircraft is an example industry where R&D-driven endogenous sunk costs have limited entry into an ever-expanding market.

It is plausible that network effects or lobbying might be other kinds of escalation mechanisms. The paper discusses lobbying in particular. Pursuing these and related phenomena as possible shapers of equilibrium market structure would be a worthwhile use of research effort.

As with the relationship between the markup, profit’s share of income, and the scale elasticity I mentioned above, endogenous sunk costs are another mechanism that might connect what the paper’s framework treats as independent. Here, these connections might be especially strong among the intensity of intangible capital in production, the scale elasticity ($\nu$), and sunk entry costs ($\kappa$). For example, network effects are often linked to intangible capital, can create a type of scale elasticity, and surely raise sunk entry costs for later potential entrants.

**Comment 6: Multi-Causality.** While emphasizing increasing entry cost story as the primary explanation for the patterns described above, the paper takes a bit of a turn in Section 4 and acknowledges, conceptually and empirically, that ultimately the patterns may be multi-causal. This is wise. It seems quite plausible that the observed trends reflect a combination of greater intangible intensity, changing product-market substitutability, greater scale economies, and higher entry costs (all with potential implications for market power, though in possibly different directions). Equally important is that the relative contribution of each can vary across sectors. Again, the multi-causal nature may not just be coincident effects. The connections mentioned above may tie them together directly.
The paper conducts a principal components decomposition to address this issue empirically. This is a very useful step, as it reduces the dimensionality of outcomes in informative ways. And while factor analysis can sometimes lead to not-so-helpful ex-post theorizing, the authors avoid drawing too strong of a conclusion about specific drivers in specific instances.

I think what will ultimately be required is a series of case studies that put faces on the facts. I realize readers might roll their eyes at someone who has worked in the industrial organization literature calling for more case studies, but I recently spoke at an industrial organization conference and told them they need to do more macroeconomics. Perhaps we can meet in the middle and learn something from each other.

To conclude, I believe the empirical case is not yet definitive that across-the-board increases in market power have led to the several trends noted above. However, my priors have moved relative to five years ago, and this paper has helped nudge them. To warrant stronger conclusions, I believe market power and alternatives need to be better quantified, not just qualified, and patterns of heterogeneity in such effects more richly characterized.

References