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MACRO NEEDS MICRO

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Abstract

An emerging consensus on the future of macroeconomics views the incorporation of a role for financial intermediation, labor market frictions, and household heterogeneity in the presence of uninsurable unemployment risk as key needed extensions to the benchmark macro framework. I argue that this is welcome, but not sufficient for macro---and international macro---to tackle the menu of issues that have been facing policymakers since the recent global crisis. For this purpose, macro needs more micro than the benchmark setup has been incorporating so far. Specifically, artificial separations between business cycle analysis, the study of stabilization policies, and growth macro, as well as between international macroeconomics and international trade, must be overcome. I review selected literature contributions that took steps in this direction; outline a number of important, promising directions for future research; and discuss methodological issues in the development of this agenda.

JEL Classification: E10, E32, E52, F12, F23, F40

Keywords: DSGE; Heterogeneous firms; Macroeconomic policy; Market entry and exit; Monopoly power; Producer dynamics; Reallocation; Structural reforms; Trade

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1 Introduction

The global financial crisis (GFC) of 2008 and the Great Recession (GR) that followed have prompted a re-examination of the toolkit for macroeconomic analysis that should be used as benchmark for teaching, research, and policy advice.

The emerging consensus is that the new benchmark should make it possible to explain why the crisis happened, why the recovery was extremely slow, and the connection between cyclical dynamics and longer-run growth. The consensus is also that benchmark macroeconomics can no longer afford to abstract from such features of reality as heterogeneity across agents, uninsurable risk, and unemployment. The consensus points toward a framework that includes endogenous capital formation, a role for the financial sector, and a merger of the HANK and SAM frameworks.[1]

While I agree with the consensus and the promise of the HANK-SAM marriage, I think focusing exclusively on introducing financial intermediation and heterogeneous-agent unemployment in the benchmark macro framework is not sufficient.[2] I believe that even those major changes to the standard New Keynesian toolbox would leave it unable of capturing other mechanisms of first-order importance for macroeconomics. The field would continue being perceived as incapable of confronting reality (regardless of whether this perception is entirely fair or not).

Specifically, it seems to me high time for macroeconomics to move beyond the representation of firm behavior in terms of production by a constant number of symmetric firms that produce either the same good under perfect competition or a fixed range of goods under monopolistic competition between a continuum of firms. Unemployment in the aftermath of the GFC happened also because a large number of firms failed and firms that did not fail reduced the number of active production lines. Credit market freezing was central to firm failures and decisions to cut production lines. Heterogeneous efficiency across firms implied that only the most efficient producers were able to survive, but their activity was slowed down by stagnant demand. Exposure to trade became the culprit for job losses that were most often caused by technological advances and/or by labor market rigidities that prevented effective reallocation of labor across firms, sectors, and geographical areas.[3] Understanding the very slow speed of recovery since the crisis and the connection between a cyclical

[1] HANK stands for Heterogeneous-Agent New Keynesian, as in Kaplan, Moll, and Violante (2016), and SAM stands for Search-And-Matching, as in the models of unemployment that build on Diamond (1982a,b) and Mortensen and Pissarides (1994). Ravn and Sterk (2016) provide an example of tractable "marriage" of HANK and SAM.

[2] This is independent of whether HANK-SAM and financial intermediation are included in closed- or open-economy macro models.

[3] The fact that only large firms tended to survive made it easier to move toward a situation of monopsony in the labor market, with firms implementing contractual arrangements (such as non-compete clauses) that contributed to increasing labor market rigidity.

phenomenon (such as the GR) and longer-term dynamics requires us to understand also the slow-down of U.S. business dynamism that happened in the last several years. Less firms are being created, and firms grow more slowly than they used to.[4] Reduced firm entry and creation of new product lines along the recovery path generate hysteresis effects with long-run output consequences. In this light, it is no surprise that the U.S. and other economies have been struggling with low productivity growth.

I strongly believe that, if we want to provide students and young researchers with a set of macro tools that can address the most important questions of the last decade—and those likely to arise in the future—, macro needs micro (MNM—probably the sweetest acronym you will ever see in macroeconomics): The standard toolkit for macroeconomic analysis of fluctuations and policy must be extended to include producer-level dynamics of entry and exit, heterogeneity across firms, and the implications of these dynamics and heterogeneity for the macroeconomy.

This does not require the creation of a completely new set of tools: endogenous producer entry is a standard feature of endogenous growth models (for instance, Romer, 1990), firm heterogeneity and its implications for domestic versus foreign market entry and exit are standard features of trade theory since Melitz (2003). What needs to be recognized is that market entry and exit do not matter only for the long-run growth in the absence of uncertainty that growth economists usually focus on; domestic and foreign market entry and exit by heterogeneous producers do not matter only in the steady-state, balanced-trade environment most trade economists restrict their attention to. Growth is the result of entry and exit decisions that are taken under uncertainty during the business cycle. These decisions will contribute to shape the cycle, and longer-run growth will be affected by cyclical dynamics through hysteresis effects. Heterogeneity will crucially affect the allocation of resources across producers and aggregate productivity. It will be among the determinants of what the economy trades and how it responds to foreign competition.[5]

If macroeconomics aims to address the dynamics of the last decade and the economic issues that have been central to recent political outcomes, artificial separations between modeling of business cycles and longer-term dynamics must be abandoned, and the same must happen to similarly artificial separations between macroeconomic and trade modeling.[6]

[4] See Economic Innovation Group (2017) and Sparshott (2016a,b,c).

[5] Absence of entry and exit dynamics from the foundation of the basic New Keynesian framework is the result of the fact that monopoly power, but not the free entry condition, was necessary as a stepping stone to introduce price stickiness in the model. This contributed crucially to the separation between (New Keynesian) business cycle macro, growth macro, and trade theory. According to Feenstra (2003), a constant number of firms "violates the spirit of monopolistic competition."

[6] I would argue that this should be true also from the perspective of trade research—i.e., that trade economists

A growing literature has made significant inroads into the development of the type of framework I am suggesting and has built a strong case for its empirical relevance. Some results of this literature have begun informing policy advice in important ways. I briefly review the state of the art in this area below, focusing on selected contributions to model development. This literature provides the foundation for extensions of the framework in a number of directions.

The rest of the paper is organized as follows. Section 2 summarizes what the literature has already accomplished by briefly describing the main ingredients of some representative, existing models and their key results. Section 3 describes what I consider the most important directions for future research. Section 4 discusses methodological issues. Section 5 concludes.

2 The State of the Art

This section reviews selected contributions to the state of the art in macro and international macro theory in which producer-level dynamics contribute to fluctuations. I focus on models that assume monopolistic competition or other forms of monopoly power, as they lend themselves most directly to providing the foundation for sticky-price extensions. The set of papers I mention is by no means intended to be a complete survey of the existing literature, and it includes much of my own work. It is the set of papers that allows me most transparently to describe how producer-level dynamics can be integrated in models of fluctuations, and to connect this to present-day questions of interest.[7]

Closed Economy

Bilbiie, Ghironi, and Melitz (2012) provide a benchmark model of fluctuations with monopolistic competition and endogenous producer entry subject to sunk costs. The model—referred to as the BGM model below—assumes that consumers derive utility from having access to a larger set of products, but the existence of entry costs implies that only a subset of the products consumers would like to have access to is actually available at each point in time. The consumption aggregator is not restricted to the familiar Dixit-Stiglitz (1977) specification, but it takes a general homothetic form. Different from earlier business cycle models with monopolistic competition and endogenous entry that assume fixed entry costs and a free entry condition that implies zero profits on a period-

should move beyond the fiction of balanced trade and the steady-state focus of their models, and they should recognize that cyclical dynamics can have effects that seal the fate of the most major trade policy decisions.

[7] By focusing on models in which firms have monopoly power, I completely omit the vast literature that builds on Hopenhayn (1992). For more references and discussion than I can cover here, see the papers I mention and references therein. For the open economy case, an extensive reading list is available at http://faculty.washington.edu/ghiro/ITMSyllabus.pdf.

by-period basis, the BGM model assumes sunk costs and a time-to-build lag: Entrants spend the first period setting up their production lines, and they begin producing and generating profits only in the following period. Free entry then equates today's sunk entry cost (which requires use of labor) to the expected present discounted value (EPDV) of profits from tomorrow to the infinite future, with discounting adjusted for an exogenous probability of firm destruction. Formally, the entry condition in the symmetric equilibrium of the BGM model is $v_t = (w_t/Z_t)f_{E,t}$, where v_t is the EPDV of profits from $t+1$ on, $f_{E,t}$ is the sunk entry cost (in units of effective labor), w_t is the real wage (in units of consumption), and Z_t is exogenous aggregate labor productivity.[8]

Firms finance their entry costs by issuing shares in the stock market, and this provides the general equilibrium link between entry decisions and the optimizing behavior of the representative household: In this model economy, investment takes the form of creation of new production lines, financed by households with their savings. The price of investment is determined by the Euler equation for share holdings. With separable, log-utility from consumption:

$$v_t = \beta\left(1-\delta\right)E_t\left[\left(\frac{C_t}{C_{t+1}}\right)\left(v_{t+1}+d_{t+1}\right)\right],$$

where $\beta \in (0,1)$ is the familiar discount factor parameter, $\delta \in (0,1)$ is the exogenous probability of firm destruction that applies to all firms (including new entrants) at the end of each period, C_t is consumption, and d_t denotes firm profits, distributed to households as dividends. Forward iteration of this equation in the absence of bubbles returns the expression for the EPDV of profits in the free entry condition. Aggregate accounting implies the standard equality between aggregate demand—the sum of consumption and investment (the price of shares times the number of new entrants, $N_{E,t}$)—and income (the sum of labor income and profits generated by the number N_t of producing firms): $C_t + v_t N_{E,t} = w_t L_t + N_t d_t$, where L_t is the amount of labor employed by the economy. The price of shares is the key, endogenously determined relative price that determines the allocation of resources between consumption of existing products and creation of new ones.

Even if the benchmark version of the model assumes that each good is produced using only labor (in linear fashion, as in the most basic New Keynesian model), the number of active producers in

[8] The presentation of the model assumes a one-to-one identification between a producer, a product line, and a firm, and I will use these terms interchangeably below. This was to facilitate relating our model to the New Keynesian literature, where individual producers in the usually assumed Dixit-Stiglitz continuum are referred to as firms. However, our preferred interpretation—consistent with relative empirical importance—is that every profit maximizing unit should be interpreted as a product line at a possibly multi-product firm whose boundaries we are leaving unspecified by exploiting continuity and the assumption that firms remain of negligible size relative to the size of the market.

any given period behaves very much like the capital stock in the simplest real business cycle (RBC) model: $N_t = (1-\delta)(N_{t-1} + N_{E,t-1})$. As this law of motion shows, the number of producing firms is predetermined and does not respond to shocks on impact, but it then adjust gradually in response to stochastic disturbances to aggregate productivity.[9]

The benchmark version of the model is simple enough that it can be literally solved with pencil and paper in log-linearized form, even though the details of the solution are not included in the published paper. There, Bilbiie, Melitz, and I use calibration to illustrate the properties of the model numerically. We show that it does at least as well (or as poorly—beauty here is in the eye of the beholder) as the basic RBC setup with respect to the familiar set of business cycle moments these models are usually evaluated against, but, in addition, it replicates successfully data properties such as the cyclicality of profits and producer entry. With translog preferences (which imply that products become more closely substitutable as their number increases), the model does a remarkable job of matching the cyclicality of the labor-share-based measure of markups in the U.S. economy used by Rotemberg and Woodford (1999).

Bilbiie, Melitz, and I set up the model intentionally to keep it as simple and as clean as possible, thus abstracting from many features of reality: For instance, we do not introduce heterogeneity across producers, and we assume that exit happens only as a result of exogenous firm destruction.[10] Absence of heterogeneity and endogenous exit implies that the model does not feature hysteresis. It also makes it possible to solve it reliably using log-linear approximation, thus obtaining results that are transparent to most macroeconomists—and no, this does not mean that we are married to log-linearization! In a nutshell, it is not unfair to characterize our model as Romer (1990) minus long-run growth and plus uncertainty, with preferences that are not restricted to Dixit-Stiglitz.

Since our paper was circulated and published, a large number of extensions and applications have been written and published by many scholars, including explorations of the role of monetary in the presence of endogenous producer dynamics in sticky-price versions of the model.[11] Once one introduces heterogeneity and endogenous exit, and assumes the appropriate externality in entry costs, the framework can generate both hysteresis and endogenous growth, making it possible to

[9] The paper also presents a version of the model in which production combines labor and physical capital.

[10] There is no fixed cost in the model in addition to the initial sunk cost of entry. This implies that, once firms have entered, they would never exit, unless hit by the exogenous "death" shock. Endogenous exit would require heterogeneity to avoid situations where all firms would want to exit. We discuss in the paper the reasons why properly calibrated exogenous exit is a reasonable approximation of reality for the purposes of our exercise.

[11] An incomplete list of references includes Bergin and Corsetti (2008), Bilbiie, Fujiwara, and Ghironi (2014), Faia (2012), and Lewis (2013). On fiscal policy, see Chugh and Ghironi (2015) and Colciago (2016). Some of these contributions explore the consequences of strategic interactions among firms of non-negligible size in models of oligopolistic competition. I return to this topic below.

study the questions that are of so much interest nowadays.[12,13]

We address normative issues in Bilbiie, Ghironi, and Melitz (2016), and I studied optimal fiscal policy in the BGM model in Chugh and Ghironi (2015), but I will focus next on work by Dhingra and Morrow (2014) that—although not developed in a dynamic, stochastic environment—addresses a very important question: What is the optimal amount of product variety in the presence of monopolistic competition and heterogeneous productivity across firms? Does the market equilibrium coincide with the solution to a social planning problem? Dhingra and Morrow study the conditions under which this happens. Their analysis complements what Bilbiie, Melitz, and I did in our 2016 CEPR DP, which focused on the DSGE case without firm heterogeneity.

Now consider the following: Since the results of Hsieh and Klenow (2009) and work by others on the consequences of resource misallocation across firms (for instance, Restuccia and Rogerson, 2013, and Fattal Jaef, 2016), one of the mantras we have been hearing from the policy community is that countries should implement structural reforms designed to facilitate reallocation of resources to high-productivity firms and exit by the low productivity ones. A problem that I see in that discussion is that it is often completely disconnected from discussion of why those low productivity firms exist. If they do because they are kept alive (or in an undead, zombie state) by distorting an otherwise efficient outcome, yes, reforms should be implemented that "kill" those firms and reallocate resources to the more efficient ones. But heterogeneous productivity—with low-productivity firms existing in equilibrium—may also be the efficient outcome of consumer demand of differentiated products and endogenous entry of producers that satisfy that demand. Dhingra and Morrow's paper helps us understand when this might be the case, and the discussion of reallocation in the policy debates should become very aware of their results. Then, if one combines BGM and Dhingra-Morrow, it becomes possible to study the consequences of product market reforms that facilitate entry and reallocate resources across heterogeneous firms in a dynamic model environment that makes it possible to trace the effects of reforms from their short-run impact all the way to their long-term outcomes. I will return to the topic of structural reforms below, but the events of the last decade—and the prominent role that market reforms have taken in recommended policy menus—underscore how important it has become to go beyond the static, long-run analysis of Blanchard

[12] A technical challenge that should be tackled in the case of translog preferences would be how to ensure that long-run growth would not imply a downward trend in the markup, which would be inconsistent with the evidence. But the problem would not arise with standard Dixit-Stiglitz preferences.

[13] Anzoategui et al. (2015) show how endogenous technology adoption and R&D extensive margin dynamics—which, like BGM, share key features with Romer (1990)—can result in persistent business cycle fluctuations. Comin and Gertler (2006) introduced the concept of medium-term business cycles and showed that a model with endogenous R&D and entry can replicate these lower-frequency fluctuations.

and Giavazzi (2003) and understand the effects of reforms in fully dynamic, stochastic settings.[14]

Most of the work referenced above assumes monopolistic competition among a continuum of firms. It thus lends itself naturally to incorporation of sticky prices, as in some work I mentioned. But this means it also lends itself naturally to exploration of the role of monetary policy (not to mention fiscal policy) in affecting the dynamics triggered by exogenous shocks and other policies (such as changes in market regulation) and potentially contributing to longer term effects once hysteresis is accounted for. Once firm heterogeneity is included, one can study the implications of macroeconomic policy for changes in characteristics of the distribution of productivity across firms—and this in average productivity and, if the model includes long-run growth, the long-run growth rate of the economy. These analyses, if performed, would complement the focus on the distributional effects of monetary policy implied by household heterogeneity in the HANK framework by focusing on the production side of the economy. The ongoing debate on "secular stagnation," low productivity growth, and hysteresis effects suggests that these are exercises it would be important to perform.

Finally, it is important to note that the focus of BGM and the afore-mentioned literature on monopolistic competition does not imply that attention should be restricted to this form of interaction between producers. The New Keynesian macro literature and much trade literature settled on monopolistic competition because, under assumption of continuity (or of a sufficiently large number of producers), it makes it possible to accomplish basic goals (introducing sticky prices or having welfare benefits from product variety) while avoiding the issue of strategic interactions between firms of non-negligible size. Once firms that are not of negligible size relative to the size of the market are included in the model, one needs to take a stand on their mode of competition, on why they do or do not collude, etc. Peter Neary has been advocating for a long time that trade theory should move beyond monopolistic competition and study other forms of market power.[15] In macro, Federico Etro and coauthors have been developing very interesting versions of the BGM model and of its extension to the sticky-price environment that explore the consequences of Bertrand or Cournot competition. See, for instance, Colciago and Etro (2010), Etro and Colciago (2010), and Etro and Rossi (2015a,b). The results of these papers and others in this area provide a starting point for further exploration of the implications of strategic behavior by large firms for the questions

[14] Cacciatore and Fiori (2016) make an important contribution in this direction by using a version of BGM extended to incorporate SAM frictions in the labor market.

[15] See, for instance, Neary (2016) and Neary and Tharakan (2012). de Blas and Russ (2015) explore the consequences of Bertrand competition in an extension of the widely used Ricardian model by Bernard et al. (2003).

facing policymakers today and, possibly, in the future.

Open Economy

In Ghironi and Melitz (2005—GM below), we made a start at bridging the gap between modern international macroeconomics and trade theory by incorporating the Melitz (2003) trade model in a DSGE model of international business cycles. In a nutshell, we developed a true dynamic, general equilibrium Melitz model with uncertainty.[16] The model shares several features with the BGM model described above, with two major differences: As in the original Melitz model, we assume that entrants face uncertainty about their firm-specific productivity at the time when they commit to sunk entry decisions into their domestic economies. Upon entry, producer-specific productivity is drawn from a continuous distribution (assumed to be Pareto when we solve the model). Firm-specific productivity remains fixed thereafter, but production (which, as in the benchmark version of BGM, uses only labor) is subject to aggregate, country-specific productivity shocks. In terms of the BGM model details I presented above, the value of the firm and firm profits in the symmetric equilibrium, v_t and d_t, are replaced by average firm value and profit, $\tilde{v}_t$ and $\tilde{d}_t$, i.e., the firm value and profit evaluated at an appropriately defined, market-share-weighted average of firm-specific productivity.[17]

The second key difference relative to BGM is that GM develops a two-country model in which producers decide endogenously whether to export output to the foreign market. Trade entails two types of costs: standard iceberg costs and fixed costs. Because of these fixed costs, only sufficiently productive firms—those whose firm-specific productivity is above an endogenously determined cutoff—export to the foreign country. Aggregate shocks cause the cutoff productivity for exporting to fluctuate, and thus cause changes in the composition of the consumption baskets across countries. (Average profits, $\tilde{d}_t$, thus combine average profits from domestic sales and average export profits.) As we show, the micro-level features of the model cause deviations from purchasing power parity that would be absent without trade costs.[18]

[16] While Melitz (2003) refers to the model as dynamic, general equilibrium, and characterized by behavior under uncertainty, the extent to which it indeed has those characteristics is not what macroeconomists would have in mind: Melitz (2003) focuses on a steady-state environment; the financing of sunk costs incurred by firms upon entry is not really modeled; and the only uncertainty is that on firm-specific productivity that firms face before entry in the domestic economy. We address those limitations in GM by developing a fully dynamic model in which entry costs are financed by households (as in BGM) and firms are subject to stochastic, country-specific shocks to aggregate productivity.

[17] Once these changes are made, aggregate accounting implies the same equality between total demand and total income as in BGM under assumption of financial autarky. When countries are allowed to trade bonds, aggregate accounting implies a standard law of motion for net foreign bond holdings.

[18] As in BGM, domestic entry is financed by households through purchases of shares in firm equity. We assume

We show that the model sheds new light on a classic issue in international macroeconomics: the Harrod-Balassa-Samuelson (HBS) effect, or the evidence that richer countries are characterized by higher prices and an appreciated real exchange rate. Textbook theory (for instance, Obstfeld and Rogoff, 1996) assumes that the effect is caused by differences in productivity growth between traded and non-traded sectors. In our model, a completely aggregate increase in home productivity causes real appreciation because of entry and endogenous non-tradedness.[19] Thus, we provide a new perspective on the HBS effect that helps explain evidence and complements the traditional theory. Because we intentionally set up the model to allow reliable solution by log-linearization, we can delve deep into it with pencil and paper (even if, different from the basic BGM, we cannot solve it fully), and we obtain analytical results that make intuitions very transparent. Numerical examples then serve the purpose of illustrating those intuitions.

In the second part of the paper, we show that the calibrated model (with a calibration that, if anything, is chosen to match micro-level data) does at least as well (or as poorly) as the standard international RBC (IRBC) model at replicating standard business cycle moments, and it does better on some dimensions. In a follow-up paper (Ghironi and Melitz, 2007), we show that the calibrated model sheds new light on the cyclicality of net and gross trade flows while being less subject to some problems of the IRBC setup concerning the cyclicality of the terms of trade.

While Melitz and I were pleased to see the model perform at least as well as the IRBC framework (and better in some dimension), from my perspective, the real contribution of our paper (and of BGM) was to show how a mechanism that we thought important (and that evidence discussed in the papers increasingly suggested important not just for long-run phenomena) could be embedded in a macro setup without huge costs in terms of tractability and intuition, and that the exercise would shed valuable light on important questions (the HBS effect). In my opinion, BGM and GM were much more about the *mechanisms*—endogenous entry in domestic and export markets—and their implications than about the numbers generated by the specific calibrations per se. This emphasis on mechanisms is something I will return to below.

Since the publication of GM, a fast-growing literature has developed at the intersection of international trade and international macroeconomics, with contributions covering a wide range of theoretical and empirical issues. It is fair to say that this literature has done a lot to remove the artificial separation of these two fields that I mentioned in the Introduction, but more needs to be

that firms are fully owned domestically (i.e., there is no international trade in equities) for simplicity. Hamano (2015) studies the implications of international trade in equities in the GM model.

[19] All goods are *tradable* in GM, but some of them are *non-traded* in equilibrium.

done, especially in recognizing that producer-level dynamics and firm heterogeneity should become part of our benchmark thinking and toolkit.

With respect to issues that have become central in present-day policy discussions, very interesting work has developed versions of the model suitable for studying the determinants and consequences of offshoring in a DSGE environment. For instance, Zlate (2016) shows that a model with endogenous offshoring can successfully replicate a number of empirical features of U.S.-Mexico interdependence, including dynamics of firm offshoring decisions that our standard international macro models are silent about.[20] Cacciatore (2014) develops a version of GM that incorporates SAM frictions in the labor market and studies the consequences of trade integration in this framework, tracing the dynamics triggered by trade integration from the impact effect of the policy change to the long-run consequences, and studying also how trade integration affects the characteristics of the international business cycle. Trade economists mostly focus on steady-state models and the long-run gains from trade when debating the effects of trade integration. But the devil is in the dynamics! The world is never in steady state. As recent and ongoing events are making painfully clear, it is the dynamics of adjustment (or the rigidities that interfere with adjustment) that are determining electoral outcomes and the fate of proposed or existing policies (trade and others). Trade economics should move past the fiction of steady state and balanced trade when studying the effects of trade integration. Cacciatore's work is an important step in that direction.[21]

Two important developments in the trade literature that I consider especially promising for their spillovers for macro research are the study of granularity and that of global value chains (GVCs). di Giovanni and Levchenko (2012) build on Gabaix (2011) to make an important contribution. As Gabaix showed, under the appropriate assumptions about the distribution of firm size, idiosyncratic shocks across firms do not wash out in the aggregate. If the economy is "granular," i.e. it features a fat tail of disproportionately large firms, shocks to these firms become a driver of the aggregate business cycle. di Giovanni and Levchenko begin by showing that smaller, more open economies tend to be more granular than large ones. They then develop a multi-country Melitz-type model with granularity, and they show that trade integration tends to increase granularity. This is an intuitive consequence of a key property of the Melitz model: The model implies that trade reallocates market

[20] Zlate (2016) develops a model of vertical FDI, in which U.S. firms decide to offshore production to Mexico in order to produce output that is then imported back to the U.S. and sold to U.S. consumers. See Contessi (2015) for a model in which firms decide to offshore in order to serve the foreign market (horizontal FDI).

[21] Alessandria and Choi (2014) and Alessandria, Choi, and Ruhl (2014) also study the consequences of trade integration in DSGE models with endogenous entry and exit decisions. Their models incorporate additional features of producer dynamics, but they abstracts from labor market frictions.

share toward the relatively more efficient firms, which become bigger (increased market share of more efficient firms also results in an endogenous increase in average firm productivity). In a granular environment, large (more efficient) firms becoming larger implies more granularity. This poses obvious questions for the debate on the consequences of trade integration, and also for the ongoing discussions on structural reforms. It is a research area that macroeconomists should pay much attention to.

The same is true of GVCs. Fragmentation of production across borders has changed the nature of trade, resulting in increasing importance of trade in value added rather than traditional trade. Bems and Johnson (forthcoming) and Johnson and Noguera (2012) have given important contributions to our understanding of the phenomenon, and Duval et al. (2016) have explored the implications for international business cycles. GVCs have key implications for how we think about competitiveness, because exchange rate changes no longer have only the standard effect on trade of making purely domestically produced goods cheaper (or more expensive) for foreigners. The international macroeconomics of GVCs is only at its beginning, and it is an especially important research area—also for a better understanding of what we would stand to lose with trade wars.

Coming to interdependence between trade and macroeconomic policies, inroads have been made by Bergin and Corsetti (2016) and Cacciatore and Ghironi (2012—CG below).[22] Bergin and Corsetti (2016) show how accounting for extensive margin dynamics can reconcile the traditional preference of policymakers for boosting manufacturing competitiveness with the incentive to appreciate the terms of trade embedded in New Keynesian open economy models since Corsetti and Pesenti (2001). In CG, we develop a version of the GM-Cacciatore model that incorporates sticky wages and prices, and we study the consequences of changes in trade integration for the optimal conduct of monetary policy, as well as the role of monetary policy in the dynamics triggered by a possible return to past levels of tariffs. Interdependence between trade and monetary policy is all over the map in policy discussions and documents—just think of the role of the creation of the Single Market in the run-up to the euro. The New Keynesian open economy literature has studied the consequences of openness for optimal monetary policy and alternative exchange rate regimes in models in which openness is characterized by changes in the degree of home bias in consumer preferences or parameters of technology. But to the extent that openness is the outcome of trade policy actions, proxying policy by varying a parameter of preferences may be very misleading: After all,

[22] See also Cooke (2014, 2016). Hamano and Pappadà (2017) focus on the interaction between monetary policy, producer dynamics, and external imbalances.

those are the famous structural parameters we would like to keep invariant to policy. Embedding trade microfoundations in the international macro framework makes it possible to perform a deeper analysis of the consequences of changes in trade policy for monetary policy. This is what we make a start at doing in CG.[23]

3 What Next?

The discussion in the previous section hinted to a number of research directions that I consider promising for the future. In addition to those, there are four directions that I view as especially relevant for future macro theory research. Two—financial intermediation and household heterogeneity—have already become part of the emerging "consensus future" of macroeconomics. I briefly discuss below how research in these areas would connect naturally to issues related to producer dynamics and heterogeneity. Existing research already yielded results that could be used to introduce these areas of ongoing work at the end of a first-year, Ph.D. macro sequence—at least, that is what I would do if I taught the second semester (or the third quarter) of such sequence. The other two research directions I focus on build and expand on themes I mentioned in Section 2.

Financial Intermediation

The work I reviewed in the previous section makes strong simplifying assumptions with respect to the role of financial markets. Entrants finance their sunk entry costs by issuing equity in frictionless stock markets (except for an assumption of extreme home equity bias that prevents international equity trading in most open economy models). There is no role for financial intermediation and associated frictions. Reality reminded us brutally in the last decade of the possible consequences of abstracting from these features. Moreover, empirical work had already documented the importance of bank finance—and the consequences of changes in the characteristics of banking markets—for the dynamics of producer entry and exit already before the GFC (for instance, Cetorelli and Strahan, 2006).

Stebunovs (2008) made a start at modeling the results of this empirical literature in a version of BGM in which firms must borrow from intermediaries with monopoly power that compete in Cournot fashion over the number of loans they issue. He showed that balancing the portfolio

[23] In Barattieri, Cacciatore, and Ghironi (2017), we use a small open economy version of CG to study the macroeconomic consequences of tariff shocks. We show that the model replicates empirical evidence on the responses to such shocks, and we evaluate whether protectionism can be beneficial by raising inflation when the economy is mired in a liquidity trap. Our conclusion is negative.

expansion effect of extending more loans with the profit destruction externality that producer entry imposes on all producers results in intermediaries erecting a financial market barrier to entry in the form of a mark-down in the bank's valuation of an extra loan (i.e., an extra productive unit) relative to the perfectly competitive benchmark.

Formally, the Euler equation for financing of entry in the BGM model is replaced by an Euler equation that determines the value of an additional producing firm at time $t+1$ in the portfolio of loans extended by a financial intermediary:

$$q_t = \beta E_t \left\{ \left(\frac{C_t}{C_{t+1}} \right) \left[\left(1 - \frac{1}{H} \right) d_{t+1} + (1-\delta)\, q_{t+1} \right] \right\},$$

where H is the number of financial intermediaries that compete in the market. The number H plays a similar role to that of the elasticity of substitution across products (θ) in the familiar continuous model of monopolistic competition: With endogenous output, the assumption $\theta > 1$ is necessary to ensure strictly positive output in equilibrium. The case $\theta \to \infty$ corresponds to perfect competition. Here, $H > 1$ is necessary to ensure that intermediaries finance a positive number of entrants: If $H = 1$ (absolute monopoly in the banking market), the Euler equation above implies $q_t = 0$, and the economy is starved of entry and, eventually, production. If $H \to \infty$, the banking market becomes perfectly competitive, and the Euler equation that determines q_t becomes that of perfectly competitive finance, as in BGM (except for the difference that q_t is the value of a firm producing with certainty at $t+1$, and thus $(1-\delta)$ multiplies only q_{t+1}). As long as H is finite, the Euler equation implies a mark-down of q_t relative to the perfectly competitive scenario.[24]

The entry condition $v_t = (\dot{w}_t / Z_t) f_{E,t}$ of BGM is replaced by $q_t = \{\dot{w}_t / [(1-\delta)\, Z_t]\}\, f_{E,t}$. A reform that increases competition in local banking markets (such as the scenario explored empirically by Cetorelli and Strahan, 2006) causes H to rise and boosts entry of non-financial establishments by narrowing the gap between q_t and its value under perfect competition.

Notz (2012) shows that Stebunovs' results hold also in a model in which the financial contract is a more standard debt contract and does not assume that intermediaries extract all the profits of the firms they finance in repayment of their loans. More recently, Bergin, Feng, and Lin (2014—BFL) develop a version of BGM that builds on Jermann and Quadrini's (2012) to incorporate financing constraints and a mix of equity versus bond finance. BFL show that their model replicates several

[24] This is akin to Hayashi's (1982) result that monopoly power results in a mark-down of the marginal valuation of capital relative to its average valuation in capital accumulation decisions by firms. See Cacciatore, Ghironi, and Stebunovs (2015) for an open economy extension of Stebunovs' analysis.

features of data in response to financial shocks.

These examples illustrate how finance can be embedded in models with producer dynamics, but much more work in this area is needed, especially to address the implications of borrower heterogeneity (here, heterogeneity across producers), asymmetric information, and the open economy dimension. We still lack a consensus model of producer heterogeneity and financial frictions that would allow us to address the role of financial intermediation for misallocation of resources across producers with market power. Manova (2013) built on Melitz (2003) to develop a benchmark model of financial frictions and trade with heterogeneous firms. She highlighted how financing requirements associated with trade can result in trade participation by a smaller set of firms in the presence of frictions, and her work provided the theoretical foundation for the explanation of the "Great Trade Collapse" of 2008-09 that highlighted the drying up of trade finance as the key source of the collapse. However, we still do not have a consensus framework that embeds financial frictions and a role for intermediation in a dynamic international macro model with uncertainty. More research in this area is needed in order to address a number of interesting positive and normative questions.

Heterogeneous Households

Kaplan, Moll, and Violante (2016) introduce uninsurable employment risk in the New Keynesian model to address the inability of the framework to study the distributional consequences of monetary policy. Ravn and Sterk (2016) show how the framework can be tractably combined with search-and-matching frictions in the labor market. The SAM model has been incorporated in a number of international macro and macro papers with producer dynamics, some of which I reviewed above. However, to the best of my knowledge, these models abstract from uninsurable employment risk, with implications for their results and the range of questions they can address. Introducing meaningful household heterogeneity in models with producer dynamics will make it possible to study the connection between distributional issues and firm dynamics.

For instance, evidence shows that, on average, exporting firms are larger and more productive than firms that serve only the domestic market. The evidence also shows that, on average, exporters pay higher wages than non-exporters. How do changes in trade policy that affect job creation and destruction by exporters and non-exporters affect the distribution of income on impact and along the dynamics toward the new long-run steady state? How does this interact with macro policy and the exchange rate? How do uninsurable employment risk and household asset accumulation shape the effects of large policy shocks such as the possible dismantling of NAFTA or the withdrawal of

the Federal Reserve from international financial regulation arrangements? And what is the effect (if any) of limited household participation in financial markets (a possible interpretation of market incompleteness) on the dynamics of firm entry and exit? IN BGM and GM, firms finance their entry costs by issuing equity purchased by the representative household on the stock market. The extent to which households participate in the stock market may have implications for the extent to which different firms rely on alternative sources of finance, and it may importantly affect the consequences of shocks and macro policy actions for both the distribution of household income and the distribution of activity across firms. In the light of ongoing events and policy discussions, these are interesting, important questions that our macroeconomic framework should address.[25]

In addition to these two research areas, I view two directions of study as especially important to build an overall framework of analysis suited to tackle present-day (and future) positive and normative issues: One is the consequences of granularity, networks, and strategic interactions between firms; the other is interdependence across policies, within and across countries, and policy-regime change. However, I would reserve covering work in these areas for second-year, field Ph.D. courses.

Granularity, Strategic Interactions, and Networks

The benchmark macro model with monopolistic competition assumes a continuum of measure-zero producers that interact with each other in non-strategic fashion. Producers respond to aggregates but not to individual competitors. This is true even in models with heterogeneous producers, such as frameworks that allow for heterogeneous productivity across firms. But the research by Gabaix (2011) in a closed-economy environment and its extension by di Giovanni and Levchenko (2012) to the consequences of international trade highlight the importance of allowing differences in firm size to have meaningful implications for the consequences of idiosyncratic shocks across firms. Moreover, once we begin entertaining the idea that firms in our macro models should no longer be measure-zero entities, the assumption of non-strategic monopolistic competition becomes less and less tenable. In small open economies, policymakers pay attention to the decisions of individual large firms that represent a disproportionate portion of the economy in taking their decisions. Even in large economies, expansions or contractions of industry giants at the center of large networks of transactions, and interactions between such key firms, have ripple effects that can propagate to

[25] Research should also address the extent to which SAM is a satisfactory model of unemployment for the purpose of addressing these and other questions, or whether Michaillat's (2012) version with rationing unemployment should be preferred.

aggregate consequences and non-negligible spillovers abroad.

Integrating these mechanisms in macro models will be important to answer a number of questions. For instance, as I discuss below, structural reforms designed to increase product market flexibility have become part of the policy menu invoked by policymakers to improve economic performance in a number of countries. But how do reforms impact economies (domestic and foreign) in the presence of granularity, networks, and strategic interdependence between large firms (and, sometimes, between these firms and the policymakers themselves)? Although there is a growing literature on granularity, networks, and strategic interactions (some of which I briefly mentioned above), we simply do not know enough in this area, and we need to know more.[26]

Similarly, we do not know enough about the implications of GVCs for macroeconomic policy, structural reforms, or even the dynamic consequences of changes in trade policy. The establishment of GVCs has resulted in fragmentation of production into networks that cross multiple country borders, with product components or unfinished products often crossing a given border repeatedly before the finished product is available to consumers in its final destination. As hinted above, this implies that the standard notion of the competitiveness effect of exchange rate changes is no longer valid. We need a dynamic model of roundabout production across country borders to begin understanding the consequences of different macro policies and exchange rate arrangements in this environment, and we need dynamic models of GVC formation—say, a dynamic, stochastic version of Antràs and Chor (2013)—to study these questions more deeply and to understand the consequences of market reforms and changes in trade policy. The threat that established GVCs may unravel if protectionist pressures led to trade wars makes the need for this research all the more urgent.

Policy Interdependence and the Dynamics of Policy-Regime Change

The difficulties facing policymaking since 2008 have highlighted the importance of multi-pronged approaches to tackling crises and persistent recessions. Calls for policy packages have become a mantra for policymakers at the highest level.[27] In many instances, calls for multi-pronged policymaking are combined with exhortations (or promises) to engage in stronger international coordination of economic policies (for instance, see G20, 2016, and Lagarde, 2016a,b).[28] The menu of

[26] On granularity, see also Carvalho and Grassi (2017) and di Giovanni, Levchenko, and Mejean (2014, 2017). The literature on networks includes Acemoğlu et al. (2012), Baqaee (2016), Carvalho (2010), and Grassi (2016).

[27] For recent examples of calls for appropriately designed policy packages, see Draghi (2016a), G20 (2016), Lagarde (2016a,b), and Praet (2016).

[28] Draghi (2016b) contains an explicit call for research on "interdependence in interdependence."

this multi-pronged approach usually includes monetary policy, fiscal policy, and structural reforms, where, depending on the situations, the latter include reforms of financial, labor, and product markets. Macro-prudential policy is also often added to the menu.

This rich menu of policies, and their interdependence within and across countries, raises questions about how these policies interact with each other—again, within and across countries. Evaluating the possible benefits of coordinating policies across countries—or across policymakers within a given country—requires attention to specifying policymaker objectives, strategy spaces, and asymmetries across countries (or policymakers) that can impinge on the possible gains from coordinating policies. I discuss below some recent work on the interaction of monetary policy and structural reforms to which I contributed. Although this work yielded valuable insights, the analysis of product and labor market reforms that it performs is based on assuming that characteristics of market regulation in reforming economies (think of euro area countries) are exogenously (and fully credibly) adjusted to U.S. levels, taking the United States as a benchmark for flexibility. No stand is taken on whether this is optimal for the countries involved, and not even on whether existing levels of regulation are optimal for the United States. The topic of optimal structural reforms (like that of optimal trade policy in a dynamic general equilibrium setting under uncertainty) is a major open area for research. When one recognizes that reforms interact with macroeconomic policy (as Draghi, 2015, and other policymakers clarified in unequivocal terms), one needs to address the implications of strategic interactions between governments (in charge of regulation) and central banks. And when one recognizes that policies have spillover effects across country borders, it becomes clear that we need to understand international interdependence within and across policy areas, and within and across borders. This is an area where we have barely begun scratching the surface. Past theoretical literature explored interdependence between monetary and fiscal policies within and across countries (Dixit and Lambertini, 2003; Eichengreen and Ghironi, 2002; or Beetsma and Jensen, 2005, in a New Keynesian framework) and between monetary and trade policy (Basevi, Denicolo', and Delbono, 1990), but much more work needs to be done to yield results that can provide a more reliable road map for the understanding of positive and normative issues in this area.

The same is true of policy-regime transition: Theoretical macro analysis (or international macro analysis) most often compares different policy regimes "in a vacuum," without addressing the actual issue of how the economy transitions from one regime to the other, and what policymakers should do to ensure an orderly transition. In international macro, we have a vast literature on exchange rate crises and a more recent literature on sudden stops as the result of occasionally binding constraints,

but we do not really have a consensus way of modeling orderly versus disorderly regime transition. While I could rattle off a bunch of consensus references on crises, I cannot come up with a consensus reference for modeling no-crisis endogenous regime transition, and what ensures that the transition would indeed be a no-crisis one. I think this should be an important area for future research, and I think this is the area where we may need the most significant departures from existing methodologies for macro modeling.

I am optimistic that developments in theoretical and computational tools have put us in a position to make significant progress in the near future along all four research directions I described, and I think we should encourage our Ph.D. students to pursue them.[29] A better understanding of the economy and formulation of better policy advice will follow from a deeper understanding of producer dynamics, of the interaction of these dynamics with finance and the behavior of heterogeneous households, and of how strategic decisions of large firms shape policymaker responses and/or are shaped by them.

4 Methodological Issues

From a methodological standpoint, I believe that the path to progress lies in not being dogmatic and in recognizing that different types of models can be useful for different purposes, as Blanchard (2017) argued. Within such flexible, non-dogmatic approach, DSGE models can serve very important purposes for theoretical analysis and the application of theory to questions of positive and normative nature, including policy evaluation exercises.

To return to a theme I mentioned above, analysis of policy packages (be it positive or normative) requires models to include all the features that are key to disentangling and understanding the effects of different policies, and how they interact with each other.[30] DSGE models have the potential to fulfill this task successfully. By building on the appropriate level of microfoundation,

[29] The four research areas I focused on are by no means the only ones I consider important for the future development of macroeconomic theory. An example of another new area of research that I consider very important and that I think we should mention in second-year field teaching is immigration. Given recent events and evidence, I believe it is important that international macroeconomists start moving beyond the assumption of immobile labor we usually make and start exploring the implications of labor mobility. Dmitriev and Hoddenbagh (2012) and Farhi and Werning (2014) made a start at this in their models of monetary unions, connecting the literature that employs the basic New Keynesian setup to Mundell's (1961) seminal work on optimum currency areas. Mandelman and Zlate (2012) went one step further in their modeling of immigration, treating it as an entry decision subject to sunk costs. They embedded this mechanism in a model of U.S.-Mexico interdependence and showed that the model does a remarkable job of replicating cyclical patterns of immigration from Mexico into the United States and of remittances from the latter back to Mexico. Mandelman and Zlate (2016) extend the model to incorporate task trade and skill upgrading to study the role of offshoring and immigration dynamics in shaping observed U.S. labor market polarization. Much more work in this area is needed.

[30] Some of the material in this and the next paragraph repeats points I made in Ghironi (2017b).

they stand the best chance of disentangling the various channels through which the policies that are called for are transmitted and interact with each other. By being dynamic, the models can help us understand the differences between short- and long-run effects of different policy actions—and how different parts of policy packages can complement or substitute for each other over time. By being stochastic, the models recognize that policy operates in an uncertain environment, where consumers, firms, and policymakers take their decisions without perfect knowledge of the future; that the effects of reforms can depend on business cycle conditions, and reforms themselves can alter the characteristics of the business cycle. Finally, general equilibrium implies that prices and quantities are jointly determined by the constraints and optimality conditions of the model, with no imposition of a-priori assumptions on how policy should affect any price or quantity.

Importantly, the defining characteristics of DSGE modeling that I just mentioned (microfoundation—even if, strictly speaking, there is no M in DSGE—, dynamics, uncertainty, and joint determination of prices and quantities by the model's constraints and optimality conditions) do not necessarily include rational expectations and reliance on exogenous productivity shocks as the sole source—or even as a source—of cyclical fluctuations. DSGE analysis does not *require* the most standard Euler equation that ties expected growth in the marginal utility of consumption to the ex-ante real interest rate, nor does it require all those ingredients (or solution techniques) for which DSGE research has become the object of a barrage of criticism from academics, bloggers, and journalists. We may want to use some or all of those ingredients and techniques because, after all, models are never meant to be photographs of reality, and it is useful to establish benchmark, transparent results in simplified frameworks that can then guide our understanding of the implications of working with more realistic assumptions. But nothing in the DSGE approach constrains us to using any of those ingredients. Even the level of microfoundation we want to embed in our models is ultimately a decision that must be taken based on the balance between complication, clarity, and empirical plausibility of assumptions *and* results. Related to this point, I am not advocating in this paper that all models in macro and international macro from now on should include *all* the building blocs I discussed above (producer entry, firm heterogeneity, openness of the economy, financial intermediation, HANK, and SAM). The choice of what to include in any given model should still be guided by the questions we want to address: Our framework and approach—and our teaching—should be flexible enough that building blocs can be added or subtracted depending on the question of interest and the goal of our analysis—whether it is purely theoretical or more applied.[31]

[31] I most strongly reject the criticism that DSGE models are designed based on "cherry picking" the facts to be

As I pointed out above, my view on the balance of micro and macro that we should incorporate in DSGE analysis is that macro needs (more) micro than the established benchmark has been incorporating, with a focus on producer dynamics and interactions to supplement the increasing attention to financial and labor markets. Note that this is important not just for the sake of microfoundations and elegance: It is important for the models to address key features of real world dynamics, to fit the narrative of policymakers, and to avoid potentially misleading results.

Interdependence across policies is an excellent example of what I have in mind: As ECB President Mario Draghi began his campaign in favor of structural reforms designed to increase the flexibility of product and labor market in the euro area, macroeconomists became naturally interested in the question whether implementing such reforms during a recession and when the central bank is constrained by the zero lower bound (ZLB) on interest rates can be especially costly. The question was initially addressed by Eggertsson, Ferrero, and Raffo (2014—EFR below) in a paper where they use the off-the-shelf basic New Keynesian framework and model reforms essentially as exogenous cuts to price and wage markups. EFR concluded that reforms boost external competitiveness and improve the external balance, but they can be very costly at the ZLB because of their deflationary effect. The EFR article was an important starting point for discussion of the effects of reforms during recessions and at the ZLB, and it received a lot of attention in the media, in the policy community, and among academics. Economists at policy institutions started using their much richer DSGE models to simulate the effects of reforms modeled in the same way. But this modeling approach completely abstracts from any of the product and labor market dynamics that policymakers have in mind when talking about structural reforms. It is sufficient to read the opening statements to Draghi's press conferences or his speeches to pick up countless references to entry barriers, product market competition, job creation and destruction, and features of micro-level behavior that the EFR modeling approach abstracts from.

Is it important to include such features in the model? The answer from the work I did with Cacciatore and Fiori (Cacciatore, Fiori, and Ghironi, 2016—CFG below) and with these authors and Duval (Cacciatore et al., 2016a,b) is a strong yes. In this work, which provided the model foundation for the discussion of structural reforms in the April 2016 issue of the IMF's *World Economic Outlook* and for the advice the IMF has been giving since, we show that incorporating

matched, and the implication that other modeling approaches would be superior by not being subject to this problem. *Every model* will only explain the behavior of the variables it incorporates. *Every model-builder*, within each modeling approach, is engaging in cherry picking, and *every model* will fail on some empirical dimensions that some of us may find very relevant. The only model that avoids this problem is called reality.

micro-level dynamics has important consequences for results, and that the ZLB should not in itself be a reason to delay reforms, at least of some types. In CFG, we show that implementing reforms in an environment of exceptional macro policy expansion is a way not only to smooth short-run costs of reforms, but also to bring long-run benefits closer to the present, as Draghi (2015) argued. Of course, our models abstract from many relevant micro-level features, and they make assumptions that the anti-DSGE crowd views as mortal sins. But I view the type of nuanced policy advice our models helped the IMF give since last April as a clear success of this DSGE work, and many other successes are out there that the critics fail (or simply refuse) to acknowledge.[32]

So, yes to DSGE, and yes to micro.

Within this approach, I believe the most productive way to proceed, especially for teaching and academic research, should be to focus on *mechanisms* rather than ad hoc tweaks to the mathematical specifications of preferences and technologies or adjustment costs. Failures of models to address important features of what we aim to explain should be studied by asking ourselves what mechanisms are missing from the framework for it to succeed—and by mechanisms I mean deeper features than most tweaks to preferences, technology, and/or the introduction of adjustment costs. The same argument applies to shocks: When a model fails to explain a set of data I am interested in given a set of shocks on which I (more or less) believe the empirical literature has told me what I should expect, my preferred approach is to ask what deep mechanism(s) is the model missing rather than adding shocks all over the model in order to "force" it to fit the data. It is this deep-mechanism-driven approach that led me to develop the agenda with Melitz and those with Cacciatore and others based on the incorporation of producer dynamics. By focusing on mechanisms and keeping the framework as simple and "clean" as we could, we were able to develop models such that, even if analytical solution is not feasible, one can go deep enough into the model with pencil and paper that the intuitions for results become quite transparent.[33]

Of course, this is not to deny that there is a role for tweaks and adjustment costs. We used adjustment costs ourselves in Cacciatore et al. (2016a,b) because of the IMF's interest in a quantitative model (and we used an ad hoc shock to the Euler equation for bond holdings in Cacciatore et al., 2016b, to push the economy against the ZLB), but, in academic research and for teaching pur-

[32] For an interesting contribution to the debate on structural reforms that accounts for firm heterogeneity and endogenous producer exit, see Hamano and Zanetti (2017).

[33] Put differently, one can incorporate micro-level dynamics into macro models without necessarily turning them into black boxes or "kitchen sinks." The same was accomplished by much DSGE literature by many scholars in other areas. This literature therefore is not subject to Blanchard's (2017) criticism of existing DSGE research, and it has already been doing what Blanchard recommends DSGE modelers should be doing. See Ghironi (2017a) for more discussion of this point.

poses, the cleaner framework should be preferred, with numerical results (which are qualitatively the same as in the more quantitative model) intended more for illustration than for close empirical relevance.

Thus, even taking as given this preference for cleaner models that may miss several features of the data, there should be no dogmatic preclusion against the use of model tweaks in both academic and (especially) policy research, and there should be no preclusion against the use of what Wren-Lewis (2017) refers to as SEMs (structural econometric models), or of "toy" models (in the language of Kocherlakota, 2016), be they microfounded or not.[34] If a SEM or a toy model make it possible to address the question of interest, my view is that we only stand to learn from comparing their results to those of different types of DSGE models (the "clean" variety I prefer and the more quantitative setups that have become predominantly associated with the DSGE label since Christiano, Eichenbaum, and Evans, 2005, and Smets and Wouters, 2007). When results are similar, we will perhaps feel more comfortable about them, and when they differ, we will have new research questions to ponder.[35]

So, macro needs micro to talk about phenomena that are very relevant to explain reality and address policy questions, but, just as important, we should teach our students to be flexible and non-dogmatic, and we should equip them with the tools to tailor the modeling approach and the specific models they use as researchers to the purposes of their research.[36]

5 Conclusion

Over twenty years ago, Paul Krugman wrote: "I would like to know how the macroeconomic model that I more or less believe can be reconciled with the trade models that I also more or less believe. [...] What we need to know is how to evaluate the microeconomics of international monetary systems. Until we can do that, we are making policy advice by the seat of our pants" (Krugman, 1995). Answering that call for research at the intersection of international trade and international macroeconomics is as important now as it was then. In fact, this paper essentially argued that the scope of Krugman's call and the answer to it must include also research that does not focus on the

[34] Baldwin and Krugman (1989) is a great example of how to use non–explicitly-microfounded toy modeling to incorporate producer dynamics in the analysis of classic international macro questions. Mehra and Prescott (1985) is an excellent example of a DSGE, microfounded, toy model that yielded very important insights.

[35] Agent-based modeling is another research area that should receive more attention. Hommes and Iori (2015) presents a collection of recent applications of this approach. See also Assenza and Delli Gatti (2013).

[36] This also requires avoiding the "Don't read, just write" approach to dissertation research that some pursue. As a field, we will be less likely of locking ourselves into any box if our students have been exposed to and encouraged to read on a "variety of boxes" (and how to manipulate them) on the way to their job market papers.

open economy dimension of macroeconomics. Macro—whether international or not—needs micro: MNM!

This paper has summarized several existing contributions to answering this call, it has outlined key next steps in this program, and it has discussed methodological issues for this agenda. Contrary to the doom-and-gloom view of macroeconomics that dominates newspaper articles and popular blogs, and that has been put forth also by some very notable scholars, I believe that macroeconomics did not regress in the last 30 years; that it did commit mistakes, but it also delivered a number of important, valuable results; and that there is a bright future for the field if we can avoid being dogmatic—and if the sociology of the journal publication business does not stymie many promising efforts.

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