Financial Structure and Economic Welfare: Applied General Equilibrium Development Economics

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Abstract This review provides a common framework for researchers thinking about the next generation of micro-founded macro models of growth, inequality, and financial deepening, as well as direction for policy makers targeting microfinance programs to alleviate poverty. Topics include treatment of financial structure general equilibrium models: testing for as-if-complete markets or other financial underpinnings; examining dual-sector models with both a perfectly intermediated sector and a sector in financial autarky, as well as a second generation of these models that embeds information problems and other obstacles to trade; designing surveys to capture measures of income, investment/savings, and flow of funds; and aggregating individuals and households to the level of network, village, or national economy. The review concludes with new directions that overcome conceptual and computational limitations.

1. INTRODUCTION

This review utilizes an applied general equilibrium approach for the evaluation of financial systems and employs that framework to organize recent advances in the literature. Among other advantages, the applied general equilibrium approach to financial systems can be used to evaluate both financial sector macro policy and microfinance initiatives (MFIs), all in a common framework. For example, the approach enables researchers to quantify the welfare effects of financial liberalization policies and MFIs, even if the latter are of sufficient scale to move prices. This review thus provides direction for researchers thinking about the next generation of micro-founded macro models of growth, inequality, and financial deepening in which (limited) intermediation plays a key role, and gives guidance to policy makers targeting microfinance programs (savings, insurance, payments, credit) to alleviate poverty.

This brief introductory paragraph raises a host of closely related issues. First, what kind of applied general equilibrium models do we have at our disposal, and are they able to deal with the dual micro/macro issues? What is assumed about micro underpinnings and financial systems in computable general equilibrium (CGE) and real business cycle (RBC) models? What do we mean, in contrast, by applied general equilibrium models of the financial system? Second, why intervene at all? If markets for saving/credit and risk are complete at all levels, then at best we should be discussing distributional issues, not efficiency. What departures from complete markets seem to characterize data around the world? Third, what have we learned about policy effectiveness and the design of policies from applied general equilibrium development economics? Fourth, what do we learn about data collection and empirical methods for evaluating development polices?

An excellent review of applied general equilibrium work is Dawkins et al. (2001). Nominally this paper addresses calibration, but in the process the authors provide an informative discussion of static, micro CGE models and dynamic stochastic general equilibrium (DSGE) models. The terms micro and macro are used to characterize these literatures, respectively, and the authors do not think of either of these two approaches as an attempt to merge into a common framework. Similarly, the authors present micro literature as applied policy-oriented work and the macro literature as used for forecasting. Searching the article, the words intermediation, banks, savings, and credit do not appear.[1] Typically, neither of these types of models has been concerned with financial sector policies.

Criticism of existing models and pleas for alternative models have become more commonplace nowadays with the advent of the recent U.S. and world financial crisis. There is some relevant modeling work in both earlier and recent empirical-driven modeling literature, and this work is mentioned below. The review in Kehoe & Prescott (2007) is especially helpful in pointing out many middle-ground contributions. Still, we need more work to integrate savings and finance into macro and include models of banks. This review focuses on how new work in development economics, or what we call applied general equilibrium development economics, is addressing the integration of micro and macro, the integration of policy with forecasting, and the integration of intermediation and savings/finance. This review also candidly assesses where more work is needed. It proposes essentially a research attack strategy, with a combination of measurement and models. Needless to say, it does not propose one new applied model that answers all the questions. We are not at that point; rather, we are at the beginning of a process.

Essentially, CGE and DSGE models are complete-markets models, at all levels. By complete markets, perfect markets, or neoclassical approaches, we simply mean here that the underlying environments and financial markets are such that the two welfare theorems hold: that Walrasian equilibria are Pareto optimal and that any Pareto optimum can be supported by lump-sum taxes and transfers. Preferences can be heterogeneous, nonseparable over commodities and time, and so on. Convexity in preferences and technologies is, however, key. The assumption that markets are complete is implicit in CGE models, as they treat consumers and firms as distinct sectors from the outset. This is more than a labeling issue: Depending on the data, it can be a substantive one, and the labels are clues to how the models are put together. Likewise, representative consumer constructs either assume all households are alike or require that preferences be Gorman aggregable and that markets be complete. We thus argue that CGE and RBC models are not the natural starting points to discuss micro/macro financial sector policies.[2]

Alternatively, we do not disown the notion that some subgroups in some settings may be doing quite well, as if they had approximately complete markets (e.g., households in a village that are related through familial ties). We use the terms kinship groups or informal networks for this phenomenon. If such groups have coherence to them (e.g., markets are complete within villages and not across villages), then we can propose new building blocks or sectors for the construction of the overall model. Clearly—and we make this explicit—the assumption of complete markets and separation needs to be tested.[3] In that sense, the notion of complete markets is a starting point for empirical work, not a statement about the entire economy.

What then is the evidence that markets are incomplete in developing countries, and how do we test this? The Arrow-Debreu full risk-sharing intertemporal-smoothing benchmark and related benchmarks can be viewed as not the entire structure of complete-markets models, but rather a key, necessary implication. Relatedly, explicit financial policy interventions, natural if unintended experiments varying per-capita access to credit and saving, variation due to geography and/or instruments in the costs of accessing the financial service providers, and deliberately exogenous randomizations of MFIs tell us that some financial policies and programs move us toward and sometimes achieve these neoclassical complete benchmark standards. So the first-pass answer is not that markets, contracts, and institutions are necessarily incomplete, but rather that the combination of markets, contracts, and institutions may function, in some places for some people, as the neoclassical benchmarks might suggest, as a good approximation to complete markets. This is true largely for households in family networks in a village or for some castes, for some syndicates of family-related firms, for some shocks such as rainfall in some but not all countries, and for some geographic regions within a given country but not other regions in the same country. As the work above suggests, access/use of formal sector financial providers versus no-access/nonuse is a useful distinction, as being a client or customer of certain, specific financial institutions with risk-contingent lending and/or equity-type saving can get households and small firms close to the optimum. These findings can be used to construct sectors in the overall models.

Indeed the class of models that we thus take as the natural starting point are the so-called dual sector financial models. These are the first-generation empirically driven applied general equilibrium models of development. These models assume that markets are complete for those with access to the formal financial sector and that otherwise households as firms are in financial autarky (at best own saving and self-insurance).[4] In these models, an expanding financial sector (exogenous in some models, as if with policy variation, and endogenous in others, as with financial deepening) can explain the movement of total factor productivity (TFP), the slowdowns associated with financial repression, the speed of transitions to a steady state, and agglomeration and the geographic frequency of enterprise. Some versions of these dual sector financial models also incorporate space; distinguish which villages have financial access/use and which do not; and assume, not inconsistently with the discussion above, that the village is the Gorman aggregated representative consumer. These models are also consistent with some (but not all) of the facts of Kuznets-style decompositions, factors accounting for growth of per-capita income, and change in inequality. One purpose of this review is to provide a more complete discussion of

these dual sector models, including estimation, calibration, measures of fit, the ability to forecast, welfare gains from policy change, and anomalies (as we look forward to next steps).

With these models, one can do policy analysis and compute welfare gains. One version argues that a financial repression induced by government takeover of the banking system can induce stagnation in growth and significant welfare losses, although the impact in the population depends on wealth levels and the way repressive policies were implemented. Another version of these models computes the welfare gains from explicit efforts to increase financial access, showing that the benefits can be particularly high for the talented poor, while taking into account losses associated with increased wages. Other work deals more with micro underpinnings and evaluates smaller, microfinance interventions of randomized control trails and quasi-natural experiments. The message here is large heterogeneity in outcomes.

One lesson from all types of the applied general equilibrium empirically implemented models is that accurate measurement is crucial. Of course, investigators differ on how much to use data, if and how to cross-check such data, whether the parameters we really want are readily available, and so on (see the discussion in Browning et al. 1999, Dawkins et al. 2001, and Kehoe & Prescott 2007).

Here, with the emphasis on imperfect intermediation, the discussion of measurement focuses on the preponderance of household enterprise in developing countries, definitions and measurement of income (distinguishing accrued income from cash flow), the balance sheet and flow of funds, and finally useful aggregations. In one sense, the approach to measurement here is entirely conventional, as in CGE models. One follows the National Income and Product Accounts, but here we make the effort to go back to or construct the financial statements at the micro level. In all of this, households are viewed as firms, so there is an integrated household-as-firm sector instead of having firms, on the one hand, and households, on the other. This is a big lesson for data collection. To reiterate, neoclassical separation is possible but not inevitable, and the requisite measurement is difficult in any event.

Through the lens of this work, we comment on how measurement in subsequent surveys can be improved. This includes promoting joint surveys of households and businesses, as well as improving questions within the modules of the questionnaires. The conceptual framework and questions asked in conventional work are sometimes unclear, but we go beyond complaining about limited data and describe recent advances in measuring profits, inventory, and so on. It is easy to think that one can go back and forth between income in household budget constraints and

income in financial accounts, but the concepts are different, with implications for measuring risk and liquidity constraints, on the one hand, and measuring productivity and rates of return, on the other. Income statements in turn are connected to balance sheets, and the time difference of the latter delivers the Flow of Funds Accounts. Although savings equals investment overall, the Flow of Funds Accounts tell the true story of financial intermediation and, with a substantial households-as-firms sector, also tell us how much household saving is directly invested at home. A lesson learned is that even more effort has to be devoted to flow of funds.

A key issue is the choice of the level of aggregation, adding up households in a family network into a syndicate, or households in a village to think about villages as small, open economies in the larger national sea, as if there were complete markets within and capital market imperfections across. One creates the labels or categories depending on the application and as the research progresses. The tension between the extremes of measurement without theory and theory without data exists, but with an eclectic back-and-forth process, progress is made.

The last section of this review focuses on future steps. The first step is to figure out which financial regime is closest to the true data-generating process: not only the extremes of financial autarky versus full risk sharing, but also savings only, savings with limited borrowing, credit and insurance contracts with moral hazard, unobserved capital, limited commitment, and adverse selection, as well as adjustment and transaction costs. As noted above, households in a village connected by familial relations may get close to the extreme of full risk-sharing complete-intertemporal smoothing, but the poor with no connections prosper less. Indeed, recent work tries to integrate a degree of connectedness between informal networks (as in joint liability groups) and access to and use of agricultural development banks into the models. These groupings are not dichotomous, and one can both be in an informal network and have formal access at the same time. Likewise, there are indirect links. These kinds of models are essentially hierarchical. There is also variation across geographic regions in the best-fitting financial regimes. For example, permanent income (savings with borrowing limits) could fit best in an industrialized area, whereas moral hazard constrained insurance/smoothing could be more appropriate in a less developed agricultural area of the same country. Clearly the realism here creates many sectors: not just industry versus agriculture or north versus south, but also concerning families, villages, financial providers, and so on. There is a need for succinct labels, but there is already work making such models operational. The next generation of applied general equilibrium models is allowing these financial building blocks to vary while retaining tractability. More ambitious (but

still underway) are models of the industrial organization of financial markets, taking into account government versus private providers and, again, geography. Indeed we return in a way to where we began, with applied general equilibrium modeling and computation issues in models, but now with models that integrate the industrial organization of firms and financial intermediation.

2. COMPUTABLE GENERAL EQUILIBRIUM AND DYNAMIC STOCHASTIC GENERAL EQUILIBRIUM MODELS

There are two strands in the modeling literature that build directly on the national accounts and related data. One literature emerged in the 1970s, namely, static CGE models such as Shoven & Whalley (1972), coming from the existence/computational work of Scarf (1973), who combined the Arrow (1964), Debreu (1959), and McKenzie (1959) general equilibrium framework with applied general equilibrium work. Kehoe & Kehoe (1994) present an excellent exposition of CGE modeling. Consumers in these models supply factors of production (e.g., labor and capital) and purchase goods from producers. Demands are generated as if consumers were maximizing utility of consumption from various sectors. For example, in Dawkins et al. (2001), the utility functions are Cobb-Douglas:

$$U^h = \prod_i (X_i^h)^{\beta_i^h}, \qquad (1)$$

where $h = 1,2,\dots$. Production functions are constant elasticity of substitution (CES) functional:

$$Q_i = \lambda_i \left(\sum_j \alpha_i^j (F_i^j)^{(\sigma_i-1)/\sigma_i} \right)^{\sigma_i/(\sigma_i-1)}, \qquad (2)$$

where $i = 1,2,\dots$. The key parameters in this model are shares for goods i in each consumer's preferences, the CES share parameters for factor j used in the production of good i, and the elasticities of substitution in the CES production functions.

More generally, the level of aggregation varies; sometimes it is as coarse as primaries, manufacturers, and services, but it is often disaggregated into subsectors. In addition, government, investment, and imports are typically added as additional sectors to handle public finance, dynamics, and trade concerns. Dawkins et al. (2001) observes that micro general equilibrium models usually have more to do with disaggregated depictions of economic systems than do the RBC models. They highlight the Whalley (1985) model of global trade, as it features 33 commodities and four trade blocs, as well as various household types and a government. Finally, they catalog some issues that have been examined with this framework: the impact of

carbon taxes, social policy in Cote d'Ivoire, the effects of the negotiations on the General Agreement on Tariffs and Trade, and the effects of the Canada–United States Free Trade Agreement (see Cox & Harris 1985, Whalley & Wigle 1990, Chia et al. 1992, Nguyen et al. 1996).

The data used to calibrate the economy typically consist of all the interindustry transactions that take place in the economy for a year, all payments to factors of production, and the final demand for goods. Assembled into a matrix, this is the input-output matrix originally developed by Leontief (1941) with fixed proportions, although contemporary treatments allow prices and substitutability. The transactions break down into blocks: intermediate inputs, final demands, and components of the value added. The transactions reported in the input-output matrix are consistent with the national income accounts. Indeed, they come from the national accounts but are meant to pick up the flow of expenditures and revenues across sectors (we return to the distinction between accrued income and cash flow below). Consequently, the data requirements can be large: trade between regions, trade barriers within regions, factor endowments and consumption by household type in each region, factor inputs, and the value of output for each production sector by region.

DSGE models in principal are not different from CGE models, but these are widely recognized as two distinct literatures. Roughly, the more the model incorporates dynamics and uncertainty, the less it is able to handle disaggregation, which is one of the strengths of CGE. One class of calibrated and implementable dynamic, stochastic models comprises the RBC models associated with Kydland & Prescott (1982) and much work that has followed. The dynastic optimization problem as set out by Cooley & Prescott (1995), for example, is to maximize the lifetime utility of the representative consumer subject to resource constraints, the flow of capital, and an exogenous shock process:

$$\max E\left[\sum_{t=0}^{\infty} \beta^t (1+\eta)^t \left[(1-\alpha)\log c_t + \alpha \log(1-h_t)\right]\right], \quad (3)$$

$$\text{s.t. } c_t + x_t = e^{z_t}(1-\gamma)^{t(1-\theta)} k_t^{\theta} h_t^{1-\theta}, \quad (4)$$

$$(1+\gamma)(1+\eta)k_{t+1} = (1-\delta)k_t + x_t, \quad (5)$$

$$z_{t+1} = \rho z_t + \varepsilon_t, \quad (6)$$

where β is a time discount factor; α and $(1 - \alpha)$ are Cobb-Douglas consumption shares, x_t is time t investment per head, c_t is time t consumption per head, k is capital per unit of effective labor, z_t is a random productivity parameter, h_t are hours supplied at time t, θ and $(1 - \theta)$ are share parameters in Cobb-Douglas technology, δ is the depreciation rate, ε_t is a productivity process error term, η is the rate of population growth, and γ is the growth rate of real, per-capita output.

This RBC work attempts to explain covariance of output, investment, and labor supply. The subsequent work of Benhabib et al. (1997) and others does incorporate two or three sectors, to break the postimpact inverse relationship between consumption and labor of a nonpermanent shock. Related work extends these to deal with depressions (Cole & Ohanian 1999, Kehoe & Prescott 2007). There is also a huge literature comparing levels and growth rates across countries (Klenow & Rodriguez-Clare 1997, Prescott 1998). These RBC, depression, and growth literatures develop and use the accounting framework of the neoclassical growth model and hence are largely representative consumer Robinson Crusoe economies without explicit financial frictions, as noted above. So either as a single consumer economy, in which case there is no need or role for financial markets, or as a Gorman aggregable economy, in which complete markets are assumed, there is nothing particularly interesting on the dimension of financial structure. An intriguing study of Chile versus Mexico (Bergoeing et al. 2007) examines financial reforms and their impact using these TFP dynamics. Although the financial sector is featured as the relevant causal factor and other factors ruled out as inconsistent with the evidence, there is no explicit modeling of the financial sector.

Still the dichotomy between CGE and DSGE models can be overdone. Moreover, some of the work in between adopts an incomplete-markets point of view. In an introduction to a special issue of the *Journal of Economic Theory*, Kehoe & Prescott (1995) emphasize these similarities and the middle ground. They also stress the necessity of tailoring models to application and how theory helps us not only as a successful approximation, but also as a way to learn from failures. The literature they review includes CGE models with at least perfect foresight dynamics, DSGE models with household heterogeneity, some search models, overlapping generations models, and RBC with nominal rigidities.

Relatedly, an important early contribution to the literature that incorporates intermediation is Alvarez et al. (1992). This work is solidly based in the national income and product accounts, explicitly has households doing the production, modifies the Flow of Funds Accounts to incorporate this and other distinctions made by the model, discusses the contribution

of intermediation to GDP, and constructs a model with banks. However, intermediation is required to go through banks, and markets are exogenously incomplete. There is a wedge between the borrowing and lending rate, due in part to the costly use of resources to intermediate. In this sense a different micro underpinning is assumed, something closer to simple borrowing/lending or the permanent income model but with borrowing constraints.

A more contemporary version of this kind of model is Christiano et al. (2007), who address business cycle facts of output, consumption, investment and add not only inflation but also net worth. It returns in a way toward CGE modeling in taking production out of the household sector. As in Alvarez et al. (1992), there is an exogenously imposed financial sector, but here lending contracts are fixed in nominal terms. Aspects from the model come from costly state verification (Townsend 1979) as used in Bernanke & Gertler (1989). Inflation is thus associated with a redistribution of wealth from household depositors to entrepreneurs/borrowers, and this has an amplifier effect (wealth effects are large in the models that follow too). Christiano et al. (2007) distinguish final goods from intermediate goods, as in the national accounts, but in addition, capital goods producers are distinguished from entrepreneurs. As the model is not calibrated by reference to a social accounting matrix, it is hard to check these assumptions. Rather, Christiano et al. (2007) naturally focus on explaining movements in the macro aggregates, the hallmark and strength of this kind of DSGE work.

In sum, CGE and DSGE models represent two distinct traditions. These implicitly assume complete markets, without testing that assumption. There is a nontrivial literature in between, some with incomplete markets imposed and some with intermediation. Again, for the most part these do not test the market structure or model around how intermediation works—at least not in developing countries. It is thus to these tests of micro underpinnings and then to the modeling that we now turn.

3. MICRO UNDERPINNINGS: TESTS OF BENCHMARK STANDARDS AND THE IMPACT OF THE FINANCIAL SECTOR

This section moves to micro data and tests of complete markets to help make some decisions about which key underpinnings should go into general equilibrium financial models. Section 3.1 reviews recent tests of the full risk-sharing complete smooth benchmark, a hallmark of complete markets, not only with consumption data, but also with labor supply, investment, and asset returns. The permanent income and buffer stock models are cousins, and these are mentioned as

well. There is variation in how well these benchmarks let us think about networks, village, and regional economies. Section 3.2 argues that policy efforts to improve financial intermediation can lead to movement toward these same benchmark complete-markets standards, achieving them in some cases. Likewise, exogenous variation through natural experiments and instruments provides much useful information on the impact of policy change and actual and potential welfare gains. Randomized trials with controls are a logical and more precise method to gauge the impact of MFIs and other policies in some settings. One must be mindful in this discussion, however, of heterogeneity in impact, the macro context in which experiments and initiatives take place, general equilibrium price and other effects, and public finance/tax subsidy issues.

3.1. Tests of Benchmarks with Micro Data

Following the view that we need to build from realistic if abstract micro foundations, we must test the building blocks of complete-markets models (or of others such as the permanent income model and the buffer stock model with borrowing constraints). Fortunately, a key part of the Arrow (1964), Debreu (1959), and McKenzie (1959) models that is operational empirically deals with the allocation of risk and the allocations of goods over time. Essentially, the allocations of complete-markets competitive equilibria are equivalent with solutions to programming problems that determine the set of allocations that are Pareto optimal. One simply maximizes a λ^i weighted sum of discounted expected utilities of all households $i = 1,...,n$ in a risk-sharing group (caste, village, members of financial institution, national economy) by choice of state/history-contingent consumption and leisure, subject to adding up constraints that determine the aggregates of consumption and labor supply, although the latter can be determined in other parts of the (unwritten) program, with production, open economy issues, and so on.

The programming problem is as follows, from Townsend (1993):

$$\max \sum_{j=1}^{n} \lambda^i \left(E \sum_{t=1}^{T} \beta^t U^j [c_t^j(\varepsilon_1,...,\varepsilon_t), l_t^j(\varepsilon_1,...,\varepsilon_t)] \right) \text{ s.t.} \qquad (7)$$

$$\left(c_t^j(\varepsilon_1,...,\varepsilon_t), l_t^i(\varepsilon_1,...,\varepsilon_t) \right) \in X_t^j(\varepsilon_t) \quad (8)$$

$$\sum_{j=1}^{n} c_t^j(\varepsilon_1,...,\varepsilon_t) \leq c_t(\varepsilon_1,...,\varepsilon_t) \quad (9)$$

$$\sum_{j=1}^{n} l_t^j(\varepsilon_1,...,\varepsilon_t) \leq l_t(\varepsilon_1,...,\varepsilon_t). \quad (10)$$

Next, one obtains first-order conditions that equate, for all dates and states, the weighted marginal utilities of these households to common Lagrange multipliers μ on constraints 9 and 10,

$$\lambda^i U_1^j [c_t^j (\varepsilon_1,\ldots,\varepsilon_t) \le l_t^j (\varepsilon_1,\ldots,\varepsilon_t)] = \mu_1(c,l) \ \ j=1,2,\ldots,n \qquad (11)$$

and

$$\lambda^i U_2^j [c_t^j (\varepsilon_1,\ldots,\varepsilon_t) \le l_t^j (\varepsilon_1,\ldots,\varepsilon_t)] = \mu_2(c,l) \ \ j=1,2,\ldots,n. \qquad (12)$$

Initially, the empirical literature assumed one good (or separability), common expectations, common discount rates, and even common specific utility functions such as CRRA (constant relative risk aversion) or HARA (hyperbolic absolute risk aversion) (Leme 1984, Cochrane 1991, Mace 1991, Townsend 1994), although the original theory (Diamond 1967, Wilson 1968) allowed substantial diversity. The implication being tested was that household consumption comoves with aggregate consumption. First differences in levels or logs remove household fixed effects, and the coefficient on aggregate consumption is common. There is now a huge literature reporting implementation of these standard tests (e.g., reviewed in Alderman & Paxson 1992, Conning & Udry 2005). Illustratively, Grimard (1997) tests among tribes in Cote d'Ivoire, Suri (2003) across villages in Kenya, and Gertler & Gruber (2002) with illness as a particular shock. On net, there are rejections, although the benchmark does surprisingly well in some circumstances. For example, in Townsend (1994), the coefficients on total idiosyncratic income relative to grain consumption are small: 0.07 or even less (although they reach 0.14 for labor income). Small coefficients on idiosyncratic income are consistent with the order of magnitude in permanent income models. Rarely, however, has the full battery of risk-sharing or other model tests allowing for groups, geography, and shocks been brought to bear in a given application, although clearly both researchers and policy makers would like to know with confidence the overall picture.

Extensions of the basic complete-markets equilibrium framework make fewer assumptions and represent an active area of research. First, one can move to risk aversion that varies over households. This was also true in earlier work when panel data were sufficient to run regressions for one household at a time, as in Townsend (1994). Naturally, this allows the coefficient on aggregate consumption or the coefficient on common fixed effects as recommended by Deaton (1997) and Chaudhuri & Ravallion (1997) to vary across households, depending on how much of the aggregate macro risk the household is willing to absorb. Macro risk is of course a key issue when a country experiences big downturns—that is, who bears the

losses (Dubois 2001, Kurosaki 2001, Chiappori et al. 2008, Schulhofer-Wohl 2008, Mazzocco & Saini 2009). These papers pick up substantial heterogeneity in risk aversion. Chiappori et al. (2008) and Dynarski et al. (1997) find differing intertemporal discount rates. Reassuringly, Chiappori et al. (2008) relate measures of risk aversion derived in this way to those backed out from Euler equations with production data and find a positive correlation. Heterogeneity can make the full risk-sharing benchmark a more reliable indicator of the true data-generating process.

As Schulhofer-Wohl (2007) shows, if less risk-averse households' income moves more strongly with aggregate shocks, then the usual benchmark regression with homogeneity and simple time fixed effects will find a spuriously large correlation between income and consumption. Ironically, however, the benchmark can still be a good operational test under these circumstances: If the standard (misspecified) regression fails to be rejected, then the one with heterogeneity will not be rejected either. Maintained and untested auxiliary assumptions, however, can cloud the picture, so it seems preferable to incorporate heterogeneity from the outset.

One can also move to multiple goods (e.g., consumption and leisure) as in the program above. Bonhomme et al. (2009) do this by allowing partial equilibrium price risk, namely shocks to wage rates, delivered entirely from labor supply preference parameters that vary across individuals. In preliminary work, they also show that the complete risk-sharing/smoothing benchmark is not rejected in 13 out of 16 villages and 22 out of 33 networks studied. Using tests of the significance and sign of one's own wage on labor supply (supposed to be positive) and a partner's wage (supposed to be insignificant), 71 out of 194 households achieve full efficiency. Operationally, one can view this work as attempting to understand at what level one can aggregate, whether individuals within a household, households in a network, or networks in a village (and perhaps differently for different regions). Of course, this work still assumes that there is nothing special about labor in household production: All labor can be hired at actual or implicit opportunity cost wages.

Moving to larger implications, one can incorporate production and tie the empirical investment literature testing for sensitivity of investment to cash flow (e.g., Gilchrist & Himmelberg 1998) to the larger complete-markets framework. The link is the stochastic discount rate capturing the Lagrange multiplier, which is key to consumption tests. When properly incorporated, this makes it appear as if firms were risk neutral and maximized expected profits,

exactly as in CGE and originally in RBC (see Alem & Townsend 2009 for an exact derivation and application to financial sector providers as reported below). This equivalence is exploited in Felkner et al. (2009) in testing for the efficiency of rice production, in a multistage model incorporating expectations and agroclimatic data.

Related, of course, is the choice of technologies and occupations. Samphantharak & Townsend (2009b) use the Euler equation with the addition of capital assignments to technologies with varying rates of return to deliver the capital asset pricing model. In this benchmark households with homogenous risk preferences take into account the probability of disasters in which village average consumption would be low. Therefore, although utility and production are jointly determined endogenous variables at the level of the village, at the level of the household unit, with either transfers in the programming problem or prices and securities in the decentralized version, household consumption is separated from household production. For 15 out of 16 villages in the sample, higher risk (as measured by the comovement of household and village return on assets) is associated with higher average return. A stronger null hypothesis is that the coefficient in that cross-sectional regression should be the expected market return $E(R_m)$ and that null is not rejected statistically (at 1%) for 8 out of 16 villages in the sample.

There remain, however, household-specific fixed effects and significant idiosyncratic risk, neither of which should be present. Furthermore, the capital asset pricing model theory is more clearly rejected at the provincial and national levels. This would suggest that households (or villages) are in part on their own, bearing the risk of their own decisions. Hence with modest transactions costs or other imperfections, the starting point or endowment does matter, as in the allocation of land (see Townsend 1993, Morduch 1995). That is, more risk-averse households in a village (or region or nation) would choose technologies that diversify their own risk. Again, this can have huge implications for production decisions.

This relatively new approach builds on an older history in the development literature of testing for neoclassical separation (see Behrman 1999 for an excellent review, especially the summary in table 3). Largely there are rejections, in that demographics and resource endowments matter for production and labor supply. Perhaps household labor in household production is key after all, not for reasons of talent (knowing how to run one's own business), but rather because of obstacles that distort markets. However, some important exceptions do exist, such as Benjamin (1992) on efficient rice production in Indonesia. More generally, as Banerjee & Duflo (2005) argue, extensive evidence, culled from the micro development

literature, demonstrates that the assumption of optimal resource allocation from intermediation fails radically. There is enormous heterogeneity in rates of return to the same factor within a single economy, a heterogeneity that dwarfs the cross-country heterogeneity in the economy-wide average return.

3.2. Policy Change, Instrumental Variation, and Natural Experiments

There is a literature closely related to research on the efficiency of the financial system, namely, the study of what happens under policy interventions or natural experiments that arguably are meant to improve things. Does improved financial access bring more households and businesses back toward the neoclassical standards (e.g., risk sharing, efficient labor supply, and pricing of assets)?

Kaboski & Townsend (2005) study programs and policies of village-level institutions in Thailand. These are continually promoted by different ministries without monitoring or evaluation, and as a consequence, although many fail, they reappear in the data, mitigating the obvious selection issues that plague evaluations. To identify impacts, these authors use policies related to the successful provision of services, such as expansion of membership, savings, and lending, as exogenous variation in effective financial intermediation. Institutions with good policies can promote consumption smoothing and occupational mobility and can decrease moneylender reliance (presumably, with caution, an indicator of constraints). The point of the work is to figure out which policies and institutions these are.

Sometimes it is plausible that there is exogenous variation in the data related to physical access (i.e., distance to a district center), geographic clustering associated with especially active loan officers, or the history of financial institutions at the village level. Using these as instruments, Alem & Townsend (2009) create a scorecard, a rating system for all the formal, quasi-formal, and informal institutions of Thailand. The Thai government development bank (BAAC) and commercial banks are shown to be particularly helpful in smoothing consumption and, to a degree, investment. These financial institutions actually allow households and those running small businesses not only to lessen the impact of idiosyncratic shocks but also to achieve the full risk-sharing/smoothing standard. That is, assuming homogeneous preferences and common discount rates, the coefficient on idiosyncratic income shocks should not enter into consumption nor into investment, the latter holding productivity constant. The associated coefficients in these regressions are positive, but interaction with instrumented versions of access

brings the coefficients in the consumption regression down to something small and not statistically different from zero for both the BAAC and commercial banks, the primary formal sector providers of the country. Overall performance of the investment regressions is less satisfactory (rarely do coefficients sum to zero impact, and indeed they are sometimes strangely negative), although the BAAC and commercial banks continue to do well. Surprisingly, the informal sector seems more helpful in investment than consumption, and we return to that below.

Kaboski & Townsend (2008) evaluate a particular financial institution using a quasi-natural experiment accidentally conducted by the Thai government. A village-level financial institution was put in every single village of Thailand, over 70,000 villages, and capitalized by 1 million baht ($24,000 each) of government money, accounting for 1.5% of GDP. The number of households in a village varies considerably and is largely exogenous, unrelated to environment, history, or socioeconomic characteristics. There is thus variation in per-capita treatment.

There are a few exceptions to the predicted exogenous nature of village size. More generally, the government-induced unintended experiments are typically less clean than randomized interventions. It is possible that village size is correlated with unobserved features that muddy the interpretation. Extensive checks with secondary data reveal that village size is neither spatially autocorrelated nor correlated with underlying geographic features such as roads or rivers, which might be the case if village size were larger near population centers or fertile areas. Small villages did tend to be located closer to forest areas, however, and the coefficient of 0.35 (0.03) was highly significant. However, this accounts for at most 5% of the variation in size.

The analysis of the impact of the million-baht funds begins with a standard buffer stock incomplete-markets model for savings behavior under income uncertainty, based on Aiyagari (1994) and Deaton (1991), in that households can lend and also borrow, but not more than some multiple of their permanent income. This is the imposed financial constraint along with the absence of insurance, as in Alvarez et al. (1992) and Christiano et al. (2007). Although their discount rate exceeds the rate of return on liquid savings, households often maintain savings above this bound as a precautionary buffer against income uncertainty. But households in the model also have the additional option of saving by investing in an asset or project that pays a higher rate of return than liquid savings. This asset is indivisible and illiquid, paying out by increasing permanent income in the future. Furthermore, the size of the indivisible project is stochastic. Parameters of preferences and technology are estimated via general method of moments on preintervention data. Then the program is implemented and viewed as a weakening

of the borrowing constraint to increase in the model the amount of (expected) credit in the village by 1 million baht, as in reality. Again, other things being equal, the policy intervention allows villages to move to better intermediation. Simulated predictions from the model mirror other aspects of the actual data, especially a substantial increase in consumption (more than one to one with the increase in credit). Formally, the estimates from the model are statistically similar to those in the data. That is, using the combined sample, a Chow test does not detect a structural break between the actual postprogram data and the model-simulated postprogram data in any of the regressions.

Underlying these impacts is much heterogeneity. Some households are hand-to-mouth, credit-constrained consumers. Others decide to invest in indivisible projects and actually reduce consumption. Yet others with the option of easier future borrowing find themselves holding larger than necessary buffer stocks and reducing financial savings. Key unobserved variation includes the current level of permanent income and draws of potential project size. This heterogeneity is a hallmark of these micro-based models. We return to a discussion of heterogeneity below.

One can put the source of some of these institutional innovations and policy variations (and others to be considered below) into their historical context, consistent with the broader view taken in this article. In Thailand, a financial crisis in the early 1980s eventually spread to one-third of all financial institutions, comprising one-quarter of total financial assets. In turn, the government's share in borrowing reached 30%. This was followed in the late 1980s by de facto and de jure financial liberalization, the elimination of interest-rate controls, and a substantial lowering of the government's share in the overall borrowing. (This episode is examined below with a structural model.) The subsequent boom of the late 1980s and early 1990s came with a dramatic expansion of the BAAC in semiurban and rural areas [providing the variation in branch locations that Alem & Townsend (2009) use as instruments], bringing variation in the credit programs of other ministries (also considered above). Unfortunately, at the same time, there was also an apparent property bubble in Bangkok sustained by offshore banking. This is a tale of two types of banking: solid domestic financial deepening and inefficient capital inflows into Bangkok. Financial sector subsidies to commercial banks and finance companies helped to hide the problem in the latter type, but the eventual currency devaluation in 1997 led to the Asian financial crisis, which indeed originated in Thailand and then spread elsewhere. Financial institution equity losses were a function of the degree of dollar-denominated debt, used as an

instrument in Vickery's (2004) evaluation of changes in net worth on the balance sheet and the bank lending channel, although related literature fails to find this in other data (Bleakley & Cowan 2009). Overall, there was a sustained slowdown in private domestic lending postcrisis. It was at this time that the government expanded social programs such as the Million Baht Fund (just featured), partly as a natural program for a populist prime minister and partly to offset criticism that commercial banks got the bulk of government assistance. The point here is that the overall evolution of the economy and its financial system leads in turn to macro policies and micro programs that then facilitate evaluation of the financial system, at least at that moment in time.

Likewise, in Mexico, reforms provide a natural laboratory, utilized in Gelos & Werner (2002), for example. A textbook repression occurred from 1982 to 1988, followed by financial liberalization in late 1988 that eliminated quotas on lending, high bank reserve requirements, and interest-rate ceilings. The authors examine the impact of these reforms on fixed investment using establishment-level data from the manufacturing sector. As measured by the sensitivity of investment to cash flow, financial constraints were eased for the smallest firms but not for larger ones.

In India, an expansion of the financial system was created by mandatory branching and licensing changes, examined in Burgess & Pande (2005). Between 1977 and 1990, the Indian Central Bank stipulated that a commercial bank could open a branch in a location with preexisting bank branches only if it also opened four in locations with no bank branches. The authors show that, between 1977 and 1990, this rule caused banks to open relatively more rural branches in Indian states with lower initial financial development. The reverse was true outside this period. The effect of the policy change was to significantly reduce rural poverty and increase nonagricultural output. However, subsequent authors have had some doubts about the robustness of these results (Kochar 2005, Panagariya 2008). Fulford (2009) uses the transitions of the dynamic buffer stock model to show that the impact that increased bank access can have on consumption can be ambiguous and depends on the timing.

3.3. Testing with Experiments and Randomized Trials

The literature on randomized trials may appear largely orthogonal to the literature on policy-induced and other natural experiments, and even further from the macro models. But there are potential links, both in placement and in evaluation. Indeed, as has been argued in Heckman &

Vytlacil (2007), the divide between structural and reduced form/experimental evaluations is unnatural. Exogenous variation provided by randomization can be helpful. [However, Heckman & Smith (1995) and Heckman (1992) also argue that some of the advantages of social experiments come with nontrivial disadvantages.] In the area of education, at least, a combination of structural and experimental methods has been brought to bear. Attanasio et al. (2009) use experiments from PROGRESA (Programa de Educación, Salud y Alimentación/Education, Health, and Nutrition Program) in Mexico to estimate parameters of a structural model that is more flexible than it could be otherwise. Todd & Wolpin (2003) and Duflo et al. (2007) use experiments in Mexico and India, respectively, to validate a model, asking if the behavior in the experiment post-treatment is consistent with what would be predicted from the model estimated with pretreatment data (see also Lumsdaine et al. 1992 on pensions). Kaboski & Townsend (2008) is an example in finance and intermediation, as argued above. More generally, Abbring & Heckman (2007) present a section on general equilibrium model and its lessons for program evaluation.

The philosophy behind program evaluations varies. In this journal series, Banerjee & Duflo (2009) stress that one of the virtues of randomized experiments, such as those facilitated by the Poverty Action Lab at MIT, is that one can estimate key elasticities. A particularly simple but highly important question is the sensitivity of loans to interest rates. Karlan & Zinman (2008) work with a consumer credit firm in South Africa in varying the advertised and the actual offered lending rates, between 7.75% and 11.75%, depending on risk class. They find that demand curves are flatter throughout most of the price range than the recent quasi-experimental estimates in the literature, but price sensitivity does increase sharply at rates above what is standard for the lender. Cole et al. (2009) estimate the sensitivity of insurance purchases to the premium. Elasticities are key parameters in CGE models, as noted above.

The results from a randomized trial of an MFI in Hyderabad, India, in Banerjee et al. (2009) are illustrative of the active randomized control trials that represent a quite active branch of the development literature. There were 120 slums, in which the Spandana program was planning to expand its microcredit program, selected for the study. A baseline survey was conducted for 20 households in each slum before the program started. Sixty slums were then randomly selected in which the microcredit program was implemented. The results paint a rather more nuanced picture of the efficacy of microfinance than what its proponents claim, although on the whole it does show beneficial effects. When the authors examine impacts separately for

households predicted to be more and less likely to open a new business, they find that households with a high propensity to become entrepreneurs spend substantially more on durable goods and reduce their nondurable consumption. Households with low propensity to become entrepreneurs, alternatively, increase their nondurable consumption. Households with existing businesses spend more on durable goods, and their business profits increase, but these households neither increase nor decrease nondurable consumption. These kinds of impacts are consistent with Kaboski & Townsend (2008), reported above.

More generally, models with underlying heterogeneity are replete with diverse impacts as well as selection effects. Even when outcomes are independent of the instrument, and the effect of the instrument is uniform on the likelihood of participation, instrumental-variables reduced-form regressions identify the local average treatment effect—those induced to participate as a result of the policy experiment. This particular treatment effect can be different in the order of magnitude and even sign from treatment on the treated and the average treatment effect, although as Heckman & Vytlacil (2001) and Heckman & Vytlacil (2005) make clear, all these measures of impact can be recovered from marginal treatment effects, given sufficient data and sufficient variation. This is fortunate, as the uniformity (also known as monotonicity) and independence conditions in Angrist & Imbens (1994) and Heckman et al. (2006) can be violated in seemingly innocuous contexts. For example, in the finance context of this review, enhanced, less costly intermediation in the randomized placement of branches can cause some inefficient households not to borrow more, at interest, but rather to exit from business enterprise and put savings in the bank at a higher return. Neither borrowing nor the likelihood of going into business is uniformly affected by the instrument. Randomization, as in wealth lotteries of mechanism design theory, overcomes nonconvexity, enhances ex ante expected utility, and serves as an instrument for the effectiveness of intermediation in a static context with incentive constraints. But this randomization fails to correctly measure impact in dynamic intertemporal settings in which levels of future promised utility matter. In static settings, levels of utility added to all outcomes do not matter, but in intertemporal settings, the level of utility promises tomorrow is a key for actions today (see Townsend & Urzúa 2009 for a discussion of these and other examples).

Returning to the general equilibrium theme of this review, Ahlin et al. (2008) argue that the ultimate success of microfinance programs may depend on macroeconomic and macro-institutional features. Their point is that understanding these linkages can make evaluation more accurate and, consistent with the goal here, help to locate microfinance in the broader picture of

economic development. They find that national-level growth has a significant and beneficial impact on MFI performance in terms of financial sustainability, default rates, and loan-size growth. This suggests that one might want to take into account the general environment in which MFIs are operating. In low-growth countries, they may be more effective than they seem, relative to MFIs in high-growth countries. Similarly, the financial depth of the overall economy is associated with lower operating costs, lower default rates, and lower interest rates, suggesting, counterintuitively, that broader competition benefits micro borrowers. However, labor force participation rates and the share of manufacturing in the economy appear to affect MFIs adversely, particularly their rate of growth in outreach, an effect that suggests perhaps that MFIs must adjust or else lose their clientele in otherwise beneficial industrialization. Alternatively, with increasing wages, running MFI at a profit becomes more difficult. One does not want to promote small businesses that subsequently turn out to be inefficient.

Not least again are direct general equilibrium effects of the interventions themselves. In a literature on the financial incentives to exit welfare, Lise et al. (2004) use theoretical modeling to evaluate a small-scale policy intervention and make predictions for the rest of the Canada that take into account changes in prices. As Banerjee & Duflo (2009) note in the context of finance and MFIs, there are many other potential effects that make extrapolation tricky, such as the crowding out of existing businesses and the inability to scale up MFI activities. Relatedly, funds that support interventions of various kinds must be raised in taxes or by other means.

However, redistribution via taxes and transfers does not necessarily mean that MFIs are inefficient. There are many efficient allocations that vary with the distribution of income. From the second fundamental theorem of welfare economics, again under convexity and other assumptions, any Pareto optimal allocation can be sustained as an optimum with lump-sum taxes and transfers. This is true even with information impediments and other obstacles to trade (Prescott & Townsend 2006). Thus transfers, or subsidies, can be consistent with efficiency if we are careful in thinking about them. In Thailand, for example, the BAAC receives a relatively small subsidy to cover its operations, specifically an income-recompense transfer that compensates for the insurance it provides in extending overdue loans or covering regional shocks (Townsend & Yaron 2001). Conceptually, this is somewhat akin to giving the subsidy to households directly via lump-sum transfers, then letting them buy financial products from the financial sector provider at market prices. Financial institutions break even, but financial access and services reach the poor. This argument is inexact and dangerous insofar as it ignores the

distorting effect of the below-market interest rate, or the subsidy to engage in credit, and this may matter if there is a nontrivial elasticity, i.e., lower interest rates causing higher loan demand. Relatedly, household recipients of the cleaner and theoretically more desirable lump-sum subsidy might not demand financial products at all.

4. MODELING WITH IMPERFECT MARKETS AND AN EXTENSIVE MARGIN

With the general economic equilibrium in mind, and now armed with more micro evidence, we return to the primary directive, measuring and modeling the financial structure of actual economies when markets and institutions are potentially incomplete. As this agenda is reasonably far along in Thailand, we present it here as a leading exemplar of how to proceed and contrast it with what we know from other economies.

4.1. Fact Finding and Quasi-Analytic Diagnostics

An auxiliary step, halfway between micro and macro, is to assemble reasonable summaries of salient historical patterns of growth, inequality, and poverty reduction. Much like many other Asian countries, Thailand has displayed high average rates of growth (6.2% GDP growth per year on average from 1950 to 2003), increasing and then decreasing inequality (as in the Kuznets curve), and substantial reductions in poverty (from 48.3% of the population falling under the poverty line in 1976 to 8.9% in 2002). Much to the point here in this review, there is significant financial deepening: Thailand starts at a relatively low level of financial intermediation and then expands.

We juxtapose this with Mexico. In contrast with Thailand, Mexico has experienced relatively slow growth in GDP (3.2% per year between 1989 and 2006), modest reductions of inequality (from a Gini coefficient of gross household income of 0.487 in 1989 to 0.459 in 2006), poverty that has been reduced only recently and remains relatively high (from 22.4% of the population in extreme poverty in 1992 to 20.3% in 2002) (López-Acevedo et al. 2004), and limited (although increasing) levels of financial deepening (approximately 24% of households had some contact with the financial sector in 1989, compared with 49% in 2006). We thus have the key questions: Can a single model explain these differences? Do we need different models for different questions, countries, and so on? This is the great present challenge.

One summary of the data, and one that leads into modeling, can be obtained from micro socioeconomic data, namely, decompositions of the change in per-capita income and the change

in inequality. The change in mean income is thus decomposed into two parts, one from the changes in population shares p^k and the other from growth within subgroups μ^k:

$$\Delta\mu = \sum_{k=1}^{K} \overline{p^k}\Delta\mu^k + \sum_{k=1}^{K} \overline{\mu^k}\Delta p^k, \quad (13)$$

where Δ denotes the difference over time, and the upper bar denotes the average over time.

The Theil-L entropy index I, our measure of income inequality, is also additively decomposable into within-group inequality WI and across-group inequality AI as follows:

$$I = WI + AI, \quad (14)$$

$$WI = \sum_{k=1}^{K} p^k I^k \quad \text{and} \quad AI = \sum_{k=1}^{K} p^k \log\left(\frac{\mu}{\mu^k}\right), \quad (15)$$

where I^k denotes the inequality within subgroup k. The within-group inequality WI is a sum of the subgroup inequality I^k's weighted by the population shares of subgroups. The across-group inequality AI is a sum of log inverse of relative incomes, again weighted by the population shares of subgroups. Due to the additive nature of the Theil-L index, we can also apply the above discrete product rule to the inequality change over time as follows:

$$\Delta I = \Delta WI + \Delta AI, \quad (16)$$

$$\Delta WI = \sum_{k} \overline{p^k}\Delta I^k + \sum_{k} \overline{I^k}\Delta p^k, \quad (17)$$

$$\Delta AI = \underbrace{\sum_{k} \overline{p^k}\left[\overline{\frac{\mu^k}{\mu}} - 1\right]\Delta\log\mu^k}_{\text{Divergence}} + \underbrace{\sum_{k} \left[\overline{\frac{\mu^k}{\mu}} - \overline{\log\frac{\mu^k}{\mu}}\right]\Delta p^k}_{\text{Kuznets}}. \quad (18)$$

The change in total inequality is decomposed into the change in within-group inequality ΔWI and the change in across-group inequality ΔAI. The within-group inequality change ΔWI is further decomposed into the change in subgroup inequality and the change in population composition as in Equation 17. The across-group inequality change ΔAI is decomposed into two components as well: the change in relative income gaps across subgroups (the first term in Equation 18) and the change due to the population-composition changes (the second term in Equation 18). Note that the Δln term approximates the growth rate of average income of subgroup k; hence the first term in Equation 18 captures the inequality change due to the differential growth rates across

subgroups weighted appropriately. The methodology comes from Mookherjee & Shorrocks (1982) and Jeong (2008).

For Thailand, for example, occupation transition, increased education, and financial access account for 21%, 25%, and 20% of income change adjusted by adult-male equivalent household size (the residual is change within categories) between 1976 and 1996. Note that financial access is included in this list. Again this is a key extensive margin in the micro data. Likewise, 7%, 47%, and 27% of inequality change can be accounted for by population shifts across these categories, the Kuznets effect (Jeong 2008). Over periods when Thailand liberalized its financial system (1986–1992), this reaches 6%, 38%, and 35% for occupation transition, education transition, and financial access, respectively.

In Mexico, population changes in terms of occupation transition, education, and financial access account for 11%, 81%, and 42% of change in gross household income, respectively (not adjusted by adult-male equivalent household size) between 1984 and 2006 [ENIGH (Encuesta Nacional de Ingresos y Gastos en Hogares) surveys before 1992 have not been harmonized by INEGI (Instituto Nacional de Estadística y Geografía) with updated census information]. These patterns are somewhat similar to Thailand. Composition effects from education are higher than in Thailand, likely a result of a large expansion of primary and secondary education in Mexico during this time period. In terms of inequality change, population shifts across categories (the Kuznets effect) for occupation transition, education transition, and financial access account for −20%, 33%, and 4% of inequality change, respectively. This means that population shifts in terms of occupation transition worked against the 0.026 increase in the mean log deviation of income in Mexico between 1984 and 2006.

Another type of decomposition, in cases obtained from macro data, is TFP: the change in output not attributable to change in factor inputs. TFP measurement is a hallmark of the RBC-type work, as noted above. In that work TFP is estimated as a residual and is a key exogenous stochastic process in the models. The difficulty is getting inside the residual and relating it to intermediation. Models such as those described below—models that have substantial integration with intermediation, savings/finance—are needed. Jeong & Townsend (2007) use occupation choice with an exogenously expanding financial sector, something that increases efficiency, to explain 79% of the change in Thai TFP.

4.2. Applied General Equilibrium–Dual Sector Models as the Starting Point

Motivated by these facts and decompositions, and some of the tests reported above, we write down a model not only with aspects of imperfect financial markets but also with an expanding financial sector: a transitional model of growth and inequality. The model's underpinnings are estimated with micro data. Then the mode is simulated and the results compared with actual dynamic data.

As in Lloyd-Ellis & Bernhardt (2000) (LEB hereafter), this model features two sectors, with one sector perfectly intermediated at a market-clearing interest rate and the other sector in financial autarky. In the latter, households choose each period to maximize end-of-period earnings by choice of wage work, subsistence agriculture, or business enterprise. Setting up a business requires some fixed costs, and these vary with the talent of the household. Thus there are wealth effects in occupation choice and capital utilization in this sector. Given an equilibrium wage rate w, wealth, talent pair type (b, x), a cost-of-living parameter v for households in urban areas (entrepreneurs and wage earners), and subsistence agricultural income γ, a household chooses its occupation to maximize his total wealth W:

$$W = \gamma + b \text{ for subsisters} \qquad (19)$$

$$= w - v + b \text{ for wage earners} \quad (20)$$

$$= \pi(b, x, w) - x - v + b \text{ for entrepreneurs,} \qquad (21)$$

where

$$\pi(b, x, w) = \max_{k,l}\{f(k,l) - wl - k - x\} \text{ s.t.} \qquad (22)$$

$$0 \leq k \leq b - x. \quad (23)$$

Although an extreme case of imperfect intermediation, the financial autarky sector captures the lack of separation between production and consumption decisions. This problem is solved for each household one at a time.

In contrast with the financial autarky sector, the intermediated sector households with low returns can save by putting their resources in the banking sector, and households with high returns can borrow from the same banking sector. An interest rate clears this market. Labor can migrate across the two sectors, and there is a common market-clearing economy-wide wage.

Part of the model displays neoclassical convergence: Savings makes its way into increased capital, leading over time to diminished returns on capital (lower interest rates) and higher marginal product of labor (eventually higher wages). Alternatively, occupation choice is constrained by wealth for those without intermediation, so for a time the rich get richer, exacerbating inequality via an increasing of number of the still relatively few entrepreneurs and a sustained magnitude of profits relative to wages, which are kept down by the larger number of workers.

Giné & Townsend (2004) and Jeong & Townsend (2008) estimate the underpinnings of this model in a number of ways, simulate it, and examine the goodness of fit. The estimation sections of these studies use the selected sample of micro data for those not in the financial sector. Essentially, the mapping between initial wealth and subsequent transition into business contains information on key underlying parameters of preferences and technology. The authors use retrospective Townsend Thai micro data and, with some strong assumptions, cross-sectional socioeconomic survey data. Giné & Townsend (2004) go on to use the structure of the model at these estimated parameters to simulate macro paths. The weight on the intermediated sector expands exactly as in the data, as if it were expanding exogenously. A mean squared error criterion is used as a measure of goodness of fit (and used to calibrate a few remaining parameters).

Giné & Townsend (2004) then use the structural model at its estimated parameters to back out counterfactual variables: what would have happened at the macro and micro levels if the financial infrastructure of Thailand had not expanded. The model thus allows one to compute the gains or losses for each measured wealth and inferred talent cell of the data. When the wage is higher than it would have been without an expanding financial infrastructure, then there can be general equilibrium effects: specifically losses from enhanced intermediation for those that would have been entrepreneurs regardless (as they now have to pay higher wages). The largest gains are for those poor but talented households who are constrained on the extensive margin and can become entrepreneurs with some credit, but who would be wage earners otherwise. There is also smaller gain for richer households who quit business with relatively low returns and save their money in banks instead. The mean welfare gains overall for those who benefit from the liberalization are between 76,840 in 1997 baht and 83,444 in 1997 baht, depending on assumption made about the (endogenous) distribution of wealth. This corresponds to 1–1.09

times the 1997 average household yearly income. However, the losses for entrepreneurs who suffer from increased wages are a similar order of magnitude relative to their income.

A second model makes the expansion of the financial sector endogenous. This is a linear coefficient, AK, constant returns model, much as in Greenwood & Jovanovic (1990) (GJ hereafter). Households maximize discounted expected utility

$$E\sum_{t=0}^{\infty}\beta^t \frac{c_{jt}^{1-\alpha}}{1-\alpha} \quad (24)$$

subject to resource constraint $k_{jt} = c_{jt} + i_{jt}$ with portfolio share Φ_{jt} for the risky asset at date t, so the next period beginning-of-period wealth k_{jt+1} can be written as

$$k_{j,t+1} = \left[\Phi_{jt} \left(\theta_{t+1} + \varepsilon_{j,t+1} \right) + (1 - \Phi_{jt})\delta \right] i_{jt}. (25)$$

Here banks (or mutual funds) are more explicit. For those in the financial sector, intermediation provides the advantage of sharing the risk of idiosyncratic shocks and better information on macro shocks. Both are part of the role of banks. The assumption for Thailand is justified by the earlier micro evaluations, e.g., the role of the BAAC and commercial banks in achieving the full risk-sharing benchmark. Households decide on how much to save, i_{jt}, and, as noted above, how much of this to put in safe technology with return δ and risky technology with return $\eta_{j,t+1}$. The latter is an additive combination of aggregate, economy-wide and idiosyncratic, household-specific shocks. However, the banking sector can completely smooth these idiosyncratic shocks, as if in a perfect mutual fund (complete markets).

Here intermediation comes at a cost, both a fixed, extensive margin cost, parameter q (of establishing branches or learning to use the financial system), and a marginal cost, the transaction-cost-induced markup between borrowing and lending rates. This is the spread in Alvarez et al. (1992), for example. Therefore, some low-wealth households are in financial autarky with value function v^0, choosing investment and a balance between safe and risky projects according to the following functional equation, deciding as well next period whether to stay out of the financial sector or enter, to get value v^1. These value functions are defined as

$$v^0(k_{jt}) = \max_{i_{jt}, \phi_{jt}} \left\{ u(k_{jt} - i_{jt}) + \beta E_{\eta j,t+1} \max\left[v^0(k_{j,t+1}), v^1(k_{j,t+1} - q) \right] \right\},$$
$$(26)$$

$$v^1(k_{jt}) = \max_{i_{jt}} \left\{ u(k_{jt} - i_{jt}) + \beta E_{\varsigma_{t+1}} v^1(k_{j,t+1}) \right\}. (27)$$

This model features a key wealth threshold below which households would choose not to pay to enter the financial system. But over time, as the distribution of wealth shifts to the right, households pass the threshold, and financial participation increases on average. Thus there are wealth effects moving the extensive margin, but the entire distribution of wealth is driving the picture, not just average net worth.

This model's micro parameters (e.g., risk aversion, discount rate, rates of return) are calibrated in Townsend & Ueda (2006). They are estimated using socioeconomic survey data in Jeong & Townsend (2008). The parameter estimates from these diverse methods are quite similar. Both papers then go on to simulate dynamic paths, drawing aggregate and idiosyncratic shocks in a large number of Monte Carlo runs. The (stochastic) calibration exercise looks at confidence interval banks, not at each variable and date one at a time. This strategy is taken because the model exhibits a transitional growth path toward distant steady-state and stationary relationship between financial deepening, inequality, and growth that are not present currently (we discuss this further below).

Although Townsend & Ueda (2006) match the average movements in financial deepening, inequality, and growth, the match is not perfect. In particular, the original GJ financial-deepening model does not explain the Thai slowdown in the early 1980s nor the upturn in the late 1980s, as mentioned above, but in subsequent work, Townsend & Ueda (2010) bring a financial repression into the model. This is a key financial sector policy and hence is at the heart of this review. Repressive policies are modeled as a distorting tax on savings, much like the marginal intermediation costs, but the tax is policy induced and wastes resources. This is calibrated by the government's share in lending relative to the private sector, something that varies from zero to over 60%. The government is also imagined to waste 5% of resources. An alternative experiment raises the cost of establishing branches. With these policies varying again according to a calibrated version of the interventions that we see in practice, lowered at the time of the subsequent liberalization, the policy-modified model fits the observed repression/liberalization episode: The Thai economy stalls out in the early 1980s, with financial deepening going completely flat and then exploding in the late 1980s.

One can back out the distribution of welfare gains at estimated/calibrated parameters from the eventual liberalization. These gains vary by wealth. Liberalization benefits the rich or the middle class, depending on the policy experiment. The aggregate compensation is approximately 27% of the aggregate wealth for the case of a reduction in the variable cost, i.e.,

elimination of the wedge or tax in intermediation. It is approximately 14% for nonparticipants and 44% for participants. Entry cost reduction gains (more branches) are in contrast relatively small: 2.0% overall, 3.9% for nonparticipants, and 0.0% for those already in the financial system. Although the calculation shows that welfare gains are sizable, growth effects of financial liberalization are not found. This is actually consistent with the empirical literature that shows mixed evidence so far. For example, in a sample of eight developing countries, Bandiera et al. (2000) show that financial liberalization is not associated with an increase in savings. Jayaratne & Strahan (1996) find that deregulation of intrastate bank branches in the United States raised the state-level GDP growth but did not increase bank lending. Based on cross-country panel data, Galindo et al. (2002) find an industry-level growth enhancing effect, but Abiad & Mody (2005) report mixed evidence on both aggregate and industry-level growth effects from financial liberalization. There is a literature on the effects of capital account liberalizations looking at growth effects. There are also conflicting views. Some stress positive effects on growth rates (e.g., Bekaert et al. 2005), and others stress negative effects, raising the probability of crisis (e.g., Stiglitz 2000). Kaminsky & Schmukler (2003) show that both views can coexist: Liberalization may create crisis in the short run but may be beneficial for long-run growth (see also Tornell & Westermann 2005).

These steps have been repeated in Mexico, where a calibrated version of the same GJ model can capture trends before and after the 1994 peso crisis, but not the crisis itself. Earlier work (Alem 2007) found the same problem in Peru. This GJ model cannot deal with disintermediation. Models of crisis are still needed. We take this up briefly in the last section.

It should be stressed that both the LEB and GJ models are models of transition. This is in contrast to the more standard steady-state analysis (see detailed discussions in Townsend & Ueda 2006). That is, one typically compares across countries as if they were in steady state (see Erosa & Hidalgo 2007). Buera & Shin (2008) compare and contrast steady states to transition dynamics to see if (*a*) financial frictions slow down the economy's progression toward stationary equilibria; (*b*) inequality affects the speed of convergence to stationary equilibria; and (*c*) economic resources are initially misallocated due to financial and nonfinancial frictions, e.g., productive entrepreneurs possessing insufficient resources. Buera & Shin find that financial frictions and the initial wealth distribution have significant and persistent effects on the transitional dynamics of the aggregate variables, although again the paper's initial condition is itself a steady-state system that is shocked with reforms. That is, economic development is modeled as a transition from a

steady state with financial and nonfinancial frictions to one where nonfinancial frictions are absent.

In contrast, the LEB and GJ estimation and calibration exercises above view all the data as coming from transitional dynamics, and a starting point can be quite far away from any steady state. Initial conditions are calibrated from observables: the fraction of the population participating in the financial system and the initial distribution of wealth. There are of course several other transition models of this kind (e.g., Acemoglu & Zilibotti 1997, Greenwood & Smith 1997), but virtually none of these other models has been taken to data with estimated or calibrated parameters. Moll (2009), Banerjee & Moll (2010), and F. Buera & B. Moll (unpublished data, 2009) emphasize the fundamental tension in fitting. When talent is somewhat persistent, then, even with credit constraints, endogenous savings from forward-looking behavior remove distortions in the distribution of TFP in the long run. Indeed, the economy can move there quickly. This is anticipated in Karaivanov (2003) and its effort to fit to the Thai data. Banerjee & Moll (2010) find that something similar to large and high-frequency shocks to productivity, different degrees of patience, or alternative financial constraints is needed to slow things down. The occupation-choice LEB model is likely vulnerable to such forward-looking behavior, were savings to be endogenous; transitions are slow in the model as it stands, but that is likely due to myopic savings, whereas if agents were forward looking, they would save more and grow faster. The financial-deepening GJ model is forward looking, yet quite slow. These better-fitting dynamics seem to come from either the entry costs of intermediation or related policy distortions (i.e., costs or wedges on the extensive, rather than intensive, margin). Speeds of transition can be estimated with long panel data, but standard regression analysis as if data came from a stationary distribution can go wrong.

We do learn something about possible next steps from the successes and failures of these occupation-choice and financial-deepening models. Jeong & Townsend (2008) offer a comprehensive review and comparison of the horserace between two models; for example, the LEB model picks up trends and fluctuations in growth and in inequality and the upturn in the late 1980s, all without any aggregate shocks. It predicts the eventual large increase in wages of unskilled labor. But it produces too few nonparticipant entrepreneurs, and their very high earnings, and in this sense the model overdoes the distinction between financial access and nonaccess. It also fails to deliver observed comovements across groups, and the gaps across groups are too large. Finally, it fails to fit via the Kolmogorov-Smirnov criterion for the overall

distribution of income. The GJ model does well with the trends of income, inequality, and financial deepening; the increasing gap between financial sector participants and nonparticipants; and the overall shape of the income distribution. But it misses the catch-up of nonparticipants after 1992 and, relatedly, overpredicts poverty. Both models fail to relate movements of aggregate patterns of growth and inequality to the growth patterns of the richest group, participant entrepreneurs. Both models underpredict inequality within groups, although a comparison of the LEB model with the GJ model shows that adding more categories is not necessarily a remedy for this.

The criterion for goodness of fit in some of these statements is as yet somewhat subjective. Alternatively, Jeong & Townsend (2008) do extensive robustness checks, varying parameter values within two standard error bands of point estimates. For the LEB model, results are sensitive to two of the five parameters of the production function, those determining the marginal product of labor. Otherwise, orders of magnitude of the changes in aggregate dynamics from perturbing other parameters of the production function, the distribution of talent, the savings rate, and returns to subsistence agriculture within 10% deviation bands are small. For the GJ model, income growth and financial expansion are sensitive to parameters that determine the rates of return to investment—in particular the risky investment—and are less sensitive to the parameters of risk aversion, entry cost, and the idiosyncratic shock.

The LEB and GJ approaches, with endogenous selection into occupation and/or the financial sector, respectively, have been taken down to the regional and cross-village level in the work of Felkner & Townsend (2009). The LEB occupation-choice model takes village-level data from the Community Development Department in Thailand and has as a key variable the fraction of households in a village engaged in enterprise. The model is consistent with the incidence of enterprise that we see on the actual map: observed increased agglomeration of enterprise activity over time, more subtle patterns of contiguous geographic convergence, and stagnation in areas left behind. But the model captures this only if the cost of entering business varies directly with distance to district towns and market infrastructure (as is estimated from village-level data). The model simulations take the spread of the financial sector from village to village, as if it were exogenous, as we see it in the data. A village without access to the outside financial system has everyone in financial autarky. A village with financial access has borrowing and lending as the model assumes. A village-level simulation of the GJ financial-deepening model treats each village as a unit with either perfect financial markets, if intermediated, or none, if not. The

simulation reveals what must be taken as targeting, if not a policy distortion, as otherwise model predictions are way off. That is, in reality, the BAAC expands into distant villages with poor infrastructure while the model overpredicts intermediation near towns. It is implausible that entry costs into the financial sector fall with distance, although that is implied by taking the model as it is to the data. This suggests that industrial organization models and a distinction between private and public sector financial providers will be needed (see below).

5. MEASUREMENT AND THE NATIONAL INCOME AND PRODUCT ACCOUNTS

With CGE, RBC, and these dual financial sector models in mind, we now return again to micro data. In this section, however, we focus more on measurement, trying to get at some of the distinctions suggested by these models. One goal is to build up our knowledge of key building blocks for the next generation of model construction.

5.1. Production in the Household Sector

As noted above, there is a framework underlying the national accounts and hence CGE models that is the basis of thinking about how economies are put together. Some new RBC models use this same framework. Essentially, with few exceptions, households do not originate product; i.e., they do not produce anything directly themselves. (The exceptions include owner-occupied housing, the hiring of domestic servants, and, as emphasized below, proprietorships.) Rather, households own factors of production and thus provide labor, capital, land, and other factors to firms that produce. Firms utilize inputs as well as intermediate goods, and, netting out the costs of the latter, value added is remitted back to households as factor payments. Goods and services are purchased by the government, which also serves to tax households. Exports, imports, and the foreign sector are considered below.

Yet in developing countries it is understood that there is substantial production in the household sector, not only in farm proprietorships, but also in nonfarm unincorporated enterprises. In some areas, this sector can dominate all the others. In Thailand, for example, the sum of farm and nonfarm proprietary income was 55% of national income in 1990. As late as 2004, corporate profits made up less than 20% of national income. In the United States in 1929, proprietary income represented 17.4% of national income, as compared with 11.8% of national income comprising corporate profits.

Still, there is always the temptation to think that "ma and pa" stores cannot add up to anything significant and that development and industrialization take off when there are domestic,

if not foreign, factories in place. Surely these bigger firms are what we have in mind in the CGE models. Foster & Rosenzweig (2004) discuss the role of larger factories in India not as emerging from small underpinnings, but rather as coming in from other parts of the country, seeking lower wages. In contrast, as emphasized above, the transition of households into enterprises can dictate a large part of the movement of macro TFP, again, the change in output not attributable to change in factors. There is some tension here. But even granting that in advanced industrialized economies the small-scale sector is relatively unimportant, it is key to understand the reasons why this is not the case in developing countries. In other words, to understand the transition, or the lack of a transition, from developing to developed, it is important to determine why the labor input is owner utilized in self-employed enterprises, as oppose to hired out to larger factories. Not studying proprietors would be akin to ignoring important symptoms.

Unfortunately, the more typical distinction between household and business surveys, with the latter missing household-based production, limits even the best research. A widely cited study of TFP in India and China (Hsieh & Klenow 2007) allows firm-level heterogeneity, as well as potential distortions or wedges in output sales and in input purchases, as in Restuccia & Rogerson (2008). Hsieh & Klenow use a standard model of monopolistic competition with heterogeneous firms (essentially Melitz 2003) to show how these distortions, which drive wedges between the marginal products of capital and labor across firms, lower aggregate TFP. The production functions assumed are constant returns to scale and either Cobb-Douglas or CES, depending on the sector. The data come from business surveys from India and China. The India Annual Survey of Industries data provide a census of all registered manufacturing plants with more than 50 workers and a random one-third sample of registered plants with more than 10 but less than 50 workers. The data for Chinese firms, the Annual Survey of Industrial Production, contain a census of all nonstate firms with more than 5 million yuan (approximately $600,000) in revenue, plus all state-owned firms. The authors estimate that approximately 30%–60% gains in productivity could be accomplished were these distortions removed. Yet even with special attention to the smaller end in India, one suspects there is a high mass of unmeasured firms, and product, on the left tail in both countries. The point is that the estimate of Hsieh & Klenow may be way off; we do not know. If small firms with high returns are constrained, then removing obstacles may have even bigger effects.

Indeed, in preliminary work, F. Buera & R.M. Townsend (unpublished data) estimate from country censuses that small firms in India with one to four employees constitute 45% of

employment and 83% of all enterprises. Including firms with five to eight employees adds another 15% and 11% to these numbers, respectively. Similarly, Mexico has a very large number of small firms, as a substantial proportion of Mexican household heads are self-employed (21% without paid employees, as compared with 6% of household heads who own businesses with paid employees, as calculated using the 2006 ENIGH data).

For Thailand, P. Kerdpairoj (unpublished data) provides key information. There are two surveys for that country that focus exclusively on manufacturing enterprises: the Household Manufacturing Survey, which covers all enterprises with less than 10 workers, and the more standard Manufacturing Industry Survey, which covers manufacturing businesses with 10 or more workers. Picking 1999 as an example year, approximately 20,808 large-sized manufacturers are present in Thailand. Over 70% are located in urban areas, and they are highly abundant in Bangkok and the Bangkok vicinity and are moderately abundant in the central region. This fits the stereotype of large urban firms, perhaps with little connection to household production. Large manufacturers employ altogether approximately 2,023,020 workers, although even here we discover that the majority of the so-called large manufacturers employ only 10–19 workers. In contrast, 1,188,482 small manufacturers are present in Thailand. Only 12.07% of small manufacturers are in urban areas; that is, the majority is located in rural areas. This does not fit the stereotype; these are small nonurban firms run by households, even from their own homes. Total employment in household manufacturing is 2,439,841, so even though the Household Manufacturing Survey units often employ only one or two family workers, largely unpaid total employment exceeds that of the larger firms in the Manufacturing Industry Survey. The point is that household manufacturing cannot be dismissed as a trivial fraction of this middle-income economy. That is, the data go against what the researcher's prior expectations might be.

Another way to look at this is to go back in time. Again, this can be illustrated from Thailand (Townsend 2010), where many of these pieces are being put together. A 1980 study from the Ministry of Industry on formal business establishments finds that 63.3% of all firms had fewer than 10 employees, even when tiny rice and saw mills, ice-making, and printing businesses are excluded. The picture grows more dramatic as one recedes into history. Akira (1989) finds that the number of establishments in Thailand with 10–49 workers was 62.5% in 1970 and even larger at 84.3% going back to 1963. More contemporary data suggest that some small firms have high rates of return (Pawasutipaisit & Townsend 2010), as if they were constrained.

5.2. Concepts of Income Accuracy of Measurement from Questionnaires

Data from any given survey are, of course, limited to the questions asked, and accuracy has to do with the way they are asked, among other factors. Surveys of manufacturers typically fail to ask about the situation of the owners as households, even if such firms are closely held. Typically, World Bank Living Standards Measurement Study (LSMS) surveys and Family Life Surveys do better on the household side in recognizing substantial production. Of course, these distinctions between firm and household would not matter if there were genuine separation of production from consumption and labor supply, and if firms and households answering the questions were able to maintain the distinction economists have in mind. Clearly, this is not easy. Even as economists, we may be unsure where to draw the line: how to think about production of consumption goods at home using purchases inputs, multitasking, and the use of time and more generally how to classify activities, e.g., child care and nurturing the household garden. (In principal the latter two are investment and production, respectively, but quite hard to measure.) Regarding finance, if households have borrowed from a bank, would they distinguish loans used in production within household enterprises from loans financing the purchase of consumer durables? It would seem better to face the difficulty squarely and think about households as firms from the outset. Then, as Deaton (1997) notes, an overall accounting framework is needed to organize the answers to the requisite myriad questions of the questionnaires.

Samphantharak & Townsend (2009a) go back to the building blocks for measurement in the national income accounts, namely the financial statement of income for firms. This is a key feature sometimes unemphasized, that the prime building blocks are financial accounts applied at the level of individual entities. The authors apply financial-accounting concepts systematically to households in a high-frequency long-duration panel, the Townsend Thai monthly data. Many issues are thought through and resolved: tangible and intangible assets, liabilities, wealth, gifts and transfers, multiperiod production activities, inventories, livestock aging, loan repayments, barter transactions, owner-produced consumption, and other intrahousehold transactions. For example, is an incoming remittance an insurance indemnity or the purchase of equity in home enterprise? How do we treat rice not sold but used to feed chickens, which lay eggs sold in a restaurant managed by the household? Should rice and other in-kind transactions be incorporated into the statement of cash flow? The authors draw explicit attention to the issues and the need to make consistent decisions. Similarly detailed measurement is done in Collins et al. (2009),

although the focus there is more on financial transactions and cash flow than the measurement of income, for which the questions are relatively simple and aggregated.

For income and product, the distinction between accrued and cash-flow bases matters much for measuring productivity versus liquidity. In the accrual basis, as in standard corporate accounting, expenses are subtracted at the time of sale of product; i.e., the return from an enterprise is sales minus purchases made to produce that product, whereas with the cash-flow-basis entries are organized according to contemporary revenues and costs—money in and money out. These distinctions are crucial in high-frequency data when we want to study both liquidity (the protection of consumption and investment from cash-flow fluctuations, and the financing of short-run business operations by the household) and the long-run underlying financial situation (the effectiveness of the household as a business in using its assets in productive activities to generate income, and the rates of return on assets and credit relative to alternative uses).

In other words, if we really want to get at the cash-flow implications of revenues and expenses and/or to get at liquidity and financing issues, then perhaps we should be using the statement of cash flow all along. One gets from accrued income to cash flow by adjusting for income-related transactions that do not have cash-flow implications: trade credit, inventory valuation, depreciation, and so on.

Although models are written down with explicit equations, it may be unclear which concept (cash versus accrual) to grab from data, if available. Worse yet, the two may be comingled. The budget constraint in models seems, naturally enough, to limit the sum of all expenditures and outflows to the sum of all revenues and inflows. That is essentially what is captured in the statement of cash flow. Alternatively, under the accrual concept of income, savings plus gifts constitute an increment in wealth, and the corresponding budget equation would tell us now about wealth management, how the increment is allocated across an increase in assets and/or decrease in liabilities. Over short horizons, with high-frequency data, these two measures can be quite distinct from one another. Rightly so, as, again, the concepts they are trying to capture are quite distinct: smoothing cash-flow fluctuations versus portfolio allocations (Samphantharak & Townsend 2009a). Ignoring the issue or averaging the data is likely to present a blurry or inaccurate picture of the distinctions that matter so much to us as economists and policy makers. A firm can be constrained in cash flow yet have very high returns (perhaps there is a policy remedy in providing short-term credit or debt rollovers), or a firm might have low return but manage cash flow well (perhaps these firms ought not to be in business at all, and their

existence reflects underlying inefficiencies, such as absence of savings vehicles to intermediate funds). Of course, if all financial statements and national accounts were done on a cash flow rather than accrual basis, then there would be no such distinction, but then the variability in cash flow clouds the picture of returns. However, most income statements are on an accrual basis, and this is one possible source of the discrepancy between micro and macro data

To draw from data the distinction between cash flow and accrued income, it matters how the questions in the surveys are asked. The LSMS agricultural module, one of the better survey instruments around, asks about inputs used over a specified cropping season and the amount spent, equating the two. But for some households and producers, these are not equal. If the households used inputs held in previous inventory, then expenditures during the specified season might be recorded as zero. Likewise, inputs purchased during the season may not have been used on the plot. Revenue raises similar timing issues. The LSMS agricultural module asks about production during the past 12 months or the past cropping season and also about sales of any of that specific product, but sales from product inventory are typically not asked about, or at least are not clearly distinguished.

Treatment of inventory and the potential mismeasurement of profits—and hence income and product—is the more general issue, and this is the subject of continuing work. De Mel et al. (2009), intent on measuring rates of return and the effectiveness of alleviating credit constraints, find that asking questions on revenues and expenses without consideration of the timing mismatch between sales and the associated purchase of inputs is a major cause of a significant difference between reported profits (i.e., asking only one question, What were your profits?) and the alternative of calculating profits from a series of more detailed questions on revenues and expenses separately, constructing the sum of revenues minus expenses as the total.

Again, measuring profits well is the key. And again, inventory is a special problem. For nonagricultural, merchandising-retail-business households such as local convenience stores, keeping track of the in transactions and out transactions of business inventory is quite difficult, if not impossible. That is, with heterogeneous types of inventory and a large number of transactions, it is difficult to know exactly what in the inventory is being sold and thus its cost basis (i.e., when acquired at what cost). Because of this, it is unknown whether the households selling at current prices are achieving a gain or a loss. De Mel et al. (2009) suggest as a remedy that it is better to ask for revenues and for the average markup of sales over input costs. That is,

with estimated markups, the data analyst can compute directly the gains and losses between acquisition and sale (i.e., capital gains and losses).

Of course, the development literature is not alone in its emphasis of households as firms. Other papers on family business include Bertrand & Schoar (2006) and Bertrand et al. (2002). These treat firms as households in thinking about management, family interconnections, and tunneling (where family-connected firms siphon the profits of successful firms in a family group away from nonfamily equity holders of those successful firms, by making them appear less successful). A U.S.-based macro/finance tradition focuses on equity in proprietorships as a portfolio choice (Heaton & Lucas 1996, Moskowitz & Vissing-Jorgensen 2002) and/or observed inequality as driven by the high returns of these business owners (e.g., Quadrini 2000, Castaneda et al. 2003, De Nardi 2004, Cagetti & De Nardi 2008).

The U.S. Survey of Small Business Finances, a survey of firms with less than 500 employees, recognizes the interrelationship between household and business in collecting demographic information. The survey includes data about the owner, as well as household net worth, although again it is somehow imagined that the household can separate business and household balance sheets. For example, the respondent is asked if a cash account is used for personal and businesses purposes. If only a portion of the cash is used in the business, then the respondent is asked to report only the business portion.

To reiterate, all these data issues matter for judgments about whether the financial system is operating well and how to model obstacles to trade, so it is quite worrisome that the more substantial, smaller segment of the economy may not be in the included in the data at all, or that output and profits are not measured well if they are. More generally, skewness in the distribution of firm size or dispersion in Tobin's Q is sometimes taken as an indicator of the existence of financial constraints and whether financial reforms have improved matters. So it has to be measured well. Abiad et al. (2004) find that liberalization improves efficiency (reduces Tobin's Q dispersion), although financial deepening does not, *ceteris paribus*.

Claessens & van Horen (2009) presents a cross-country study of financial frictions in the institutional context. Another partial remedy is to look at the robust implication of financial constraint such as firm size, rather than profits or productivity. Cabral & Mata (2003) argue that the evolution of firm size in Portugal has to do with financial constraints. Fernandes & Pakes (2008) find only limited evidence for capital market distortions across states in India relative to

apparent labor market distortions. More generally, measurement of firm size as it relates to financial efficiency is an active area of research.

5.3. Balance Sheets and Flow of Funds

The financial statements that underlie the national income accounts include not only the statement of income, but also the balance sheet. This is a measure of assets, liabilities, and owner equity at a point in time. When differenced over time, it becomes the savings investment account, upon which the Flow of Funds Accounts are constructed. The Flows of Funds Accounts examine net savings and are typically segregated into sectors: firm, financial, foreign, federal government, and state and local government. Household enterprises with positive retained earnings, or net income exceeding consumption, save somewhere: in their own enterprise via real capital asset purchases, in cash, or in financial assets. Changes in financial assets are typically emphasized in the flow of funds across sectors, although we also care how much of one's own investment comes from one's own savings. In principal, reasonably accurate numbers could be obtained for flows within and across sectors. In practice, however, there is often an inconsistency between the numbers that come from household surveys, occasionally but not always used in the construction of national income and product accounts for consumption, and the numbers obtained from the official national accounts themselves. In Thailand, for example, household savings are substantially less in the household surveys than in the national accounts. One problem with surveys is that they tend to undersample the richer households who have the bulk of the savings. Low net savings in household surveys is also consistent with buffer stock smoothing; that is, savings are negative when households experience adverse shocks. Deaton (1997) presents an excellent discussion of these issues. Srinivasan (2003) details the various surveys used in India to measure not only household savings, but also household investment, and notes again that in the India national accounts both of these are essentially residuals, yet large. The point is that errors in these would likely matter much to the entire India national picture.

Again, one's own investment and accumulated cash when subtracted from total savings leave a residual that is, by definition, intermediated with financial instruments, as measured in the flow of funds across sectors. The channeling of savings to borrowers either through savings in banks or through lending in informal markets is crucial to understanding how the financial system in a given country is put together. In this sprit, Hanna (1994), for example, provides for Indonesia a stimulating review of the sequencing of macro/financial reforms and correlations with savings, credit, and investment. This also distinguishes private and government sector

banks. Green & Murinde (1999) offer a review of flow-of-funds research in developing countries, which, as they emphasize, is revealing of the interrelationship between financial sector development and the real economy. More generally, we want to know if intermediation and flow of funds, when measured properly, are largely local and limited by geography and kinship, for example, or if they span across unrelated individuals and diverse geographic areas (see below).

5.4. Aggregation and Key Subunits

Financial accounts are typically constructed at the level of households or firms (or as we argue just above, this should be households as firms). This allows aggregation up to various possible levels or categories, as the analyst might wish. One can add up and consider firms and/or households in a family network, for example. As noted in Bertrand et al. (2008), most corporate finance models of firm behavior study the typical U.S. corporation, defined as one firm with a large set of dispersed shareholders. But La Porta et al. (1999) find that approximately 25% of the firms in their sample are members of pyramids. In a pyramid, an ultimate owner uses indirect ownership to maintain control over a large group of companies. This kind of structure raises distinct questions about firm behavior. These pyramids are especially relevant in developing countries. For example, Bertrand et al. (2002) propose to follow the flow of funds through a pyramid in India by tracking the propagation of exogenous shocks to different firms. Related is the interest in networks, both in theory and practice (from the theory perspective, see Genicot & Ray 2003, Jackson & Watts 2005, Bramoullé & Kranton 2005, Ambrus et al. 2008; for empirical work, see Fafchamps & Lund 2003, Leider et al. 2009). Detailed data at the level of financial accounts, as in Samphantharak & Townsend (2009a), can help enormously with these issues.

Paweenawat & Townsend (2009) takes those accounts at the household/firm level and aggregate up to construct village- and county-level income and product numbers. One might think of this as a kind of CGE formulation, but in this work villages are the natural unit, potentially with much better markets and institutions within than across. That is, the authors treat villages as small open economies in the larger regional and national sea. This in turn allows for a focus on trade flows, the balance of payments, capital mobility, and the interrelationship between the real (trade) and macro (financial) aspects.

Ricardian comparative advantage and Heckscher-Ohlin-Vanek factor proportions models successfully explain some key aspects of these data. Villages and regions tend to export products in which they have a comparative advantage or use relatively more of abundant factors. Over

time, as the Thai national economy opens up with lower transport costs or better means of transportation, factor prices move in predictable directions; e.g., interest rates decline over time in capital-scarce, labor-abundant regions. Alternatively, the real wage in Thailand is increasing in all regions. Nor is there any increase in specialization. If anything, the addition of business income seems to diversify portfolios. Most village-level current account surpluses are matched by financial saving, but there are some real capital movements. The interrelationship between the real and financial sides of the picture, combining macro/financial and micro/trade as in modeling north and south financial frictions, is the focus of Antras & Caballero (2007), work that builds upon Rajan & Zingales (1998) and Manova (2008).

Alternatively, Donaldson (2009) successfully uses a Ricardian-based model to study disaggregated, internal trade flows and smoothing in India, following the international work of Alvarez & Lucas (2007) and Eaton & Kortum (2002). The bottom line in all of this is that geography and key subunits do seem to matter for understanding how a given economy is put together and utilizing both data and models in applied general equilibrium work.

Of course, the accounting framework is not perfect, neither in the measurement nor the underlying theory. On the measurement side, it is sometimes desirable to disaggregate down to the level of the individual within an apparent household. Family members do sometimes draw distinctions across owned versus shared assets, for example, and there is a substantial body of work asking whether households are unitary (Duncan 1993, Lundberg & Pollak 1993, Alderman et al. 1995, Chiappori & Ekeland 2006, Bourguignon et al. 2009, Chiappori & Ekeland 2009). Individual ownership of assets may matter for within-household divisions of consumption and labor supply behavior. And if individual wealth changes, then, without commitment, bargaining weights can change, undermining the conception of the household as unitary. Certainly, migration in and out of households creates huge issues in the appropriate sampling frame even at the household level, although tracking surveys and analysis, as in the Indonesian Family Life Survey (Sikoki et al. 2009) and the Kenyan Life Panel Survey (Baird et al. 2008), have made great strides.

6. NEXT STEPS AND FUTURE DIRECTIONS

A key building block in all these models is the specification of the financial regime. We mention full risk sharing on the one extreme and financial autarky on the other. Some incomplete-markets models adopt at most the permanent income model or, more limited, natural credit limits and

buffer stocks. Ideally the assumed financial regime should be close to the true data-generating process; i.e., micro underpinnings should be tested.

Kinnan (2009) develop a nonparametric test of inverse Euler equation implications for limited commitment models, in which lagged marginal utility is a sufficient statistic for the present consumption, and for alternative financial regimes. She finds that unobserved output seems to play a key role. Attanasio & Pavoni (2009) estimate and compare the extent of consumption smoothing in the permanent income model relative to that in a moral hazard model with hidden savings, finding evidence for the former. Paulson et al. (2006) compare moral hazard, as in Aghion and Bolton (1997), to limited commitment, as in Evans and Jovanovic (1989), for which wealth plays a role, along with talent, in both models in determining whether a household enters into business, and at what scale. A key step in this work is the use of the methods of Vuong (1989) to compare across nonnested regimes. Karaivanov et al. (2009) generalize these linear programming methods so that they work with panel data and a wider variety of mechanism design regimes along with the others: autarky, savings only, borrowing and lending, moral hazard, unobserved capital, and full risk sharing/smoothing. There appear to be differences across regions; for example, the more limited borrowing/saving financial regime fits the best in the central region near Bangkok, whereas something less constrained, the moral hazard financial regime, fits the best in the more rural northeast. Karaivanov et al. (2009) use these same methods taken to Spain to evaluate differences in effective financial regimes and how they vary with the financial provider, distinguishing across single-banked firms, multiple-banked firms, and older firms with no bank use at all. The last category turns out to be a group of well-capitalized family-related businesses. Likewise, in the Thai data, as with the earlier tests for full insurance, family-connected households in a village do better, coming closer to complete risk sharing than the overall sample.

This leaves open of course the connection between the formal and informal sectors. C. Kinnan & R.M. Townsend (unpublished data, 2009) show that being indirectly connected via a chain of family members or through gift giving to the BAAC or commercial banks works to smooth consumption against idiosyncratic risk as well as being a direct client. For investment, it appears that social sanctions play an additional role, helping to leverage outside funds. Other work tries to integrate formal and informal sectors into the same theoretical model. In addition to the 1997 household survey, Ahlin & Townsend (2007b) use a BAAC group borrowing module and examine the decision of whether to borrow as an individual or in a group, testing some

(static) models in the literature. Two of these models highlight moral hazard problems that joint liability lending and monitoring can mitigate (Stiglitz 1990, Banerjee et al. 1994). Another model focuses on an environment of limited contract enforcement and the remedy of village sanctions (Besley & Coate 1995). A fourth model shows how adverse selection of borrowers can be partially overcome by joint liability contracts (Ghatak 1999). Ahlin & Townsend (2007b) find that the Besley & Coate model of how social sanctions can prevent strategic default performs remarkably well, especially in the low-infrastructure northeast region. The Ghatak model of peer screening by risk type to overcome adverse selection is supported in the central region, closer to Bangkok

Other work attempts to identify and distinguish obstacles as well. Karlan & Zinman (2010) distinguish ex ante selection from ex post moral hazard, although they are unable to isolate the effect of classic adverse selection (by risk type alone). Ahlin & Townsend (2007a) find evidence for classic adverse selection using both a structural model and astructural empirical work. The model delivers preferences for the choice between individual loans versus joint liability loans and predicts that higher correlation across project returns would make joint liability borrowing more likely to be chosen. Ashraf et al. (2006) work with Green Bank in the Philippines and find evidence that time-inconsistent preferences generate demand for a committed savings product.

The next generation of applied general equilibrium models will allow these financial building blocks and obstacles to vary while retaining tractability. Moll et al. (2009) embed occupation choice into a model with generalized financial regimes as linear programs, so that a variety of financial regimes can be chosen, potentially different in different regions of the country, as estimated in the data. That is, the impediment to trade might be moral hazard in one instance or collateral/default in another. The strategy for computation here is, as in Buera & Shin (2008), to take paths of wages and interest rates as given, solve the subproblems for the local subeconomies, then gauge excess demand for capital, and finally compute market-clearing prices via an iterative algorithm.

Yet more ambitious but still underway are models of the industrial organization of financial markets. Moreno (2010) uses a matching model to understand observed heterogeneity in Mexico in both financial sector providers and firm borrowers, and the impact of a financial liberalization. Assuncao et al. (2009) take geography and expansion into account to compare the clustering and strategic patterns of government versus private sector providers.

We return at the end of this review to the overall framework and where we want to go. These are general equilibrium models in which there are decentralized markets and information problems, networks and groups are endogenous, firm size interacts with finance, and everything is priced. Prescott & Townsend (2006) allow two types of agents, rich and poor in endowments of wealth, with the possibility of both single-agent firms (proprietorships with a moral hazard problem) and multiagent firms (worker/supervisor pairs with internal monitoring to mitigate moral hazard). Limited commitment can also be accommodated. The set of Pareto optimal allocations and their associated decentralized competitive equilibrium are computed with the Dantzig-Wolfe (1961) algorithm. This brings us full circle to earlier work, e.g., Mansur & Whalley's (1982) use of the Dantzig-Wolfe algorithm. The contemporary application here is an applied general equilibrium model in which types of firms, firm size, the assignment of workers, and financial flows are all integrated and codetermined. Although solutions to such prototype models can be characterized as key parameters are varied, such models have not been taken to data. Yet clearly computation methods can lead to subsequent advances in applied general equilibrium modeling, using theory and data. This has been the case in applied general equilibrium since its inception.

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[1]The terms finance and financial appear only in the footnote acknowledging financial support for the research. The term savings is mentioned only in terms of wide-ranging estimates of elasticities.

[2]Many economies with underlying nonconvex environments can be turned into linear economies with linear programs, so economies with information and other problems can be analyzed with standard tools such as the two welfare theorems. But the neoclassical separation fails in the models. Wealth matters for consumption and production, for example.

[3]Likewise, there exist in the literature other models with financial sectors and intermediaries. These typically assume incomplete markets. We argue that those underpinnings need to be tested as well.

[4]Other markets for wage labor are not in autarky, however.