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Citation: Townsend, Robert M. "Village and Larger Economies: The Theory and Measurement of the Townsend Thai Project." Journal of Economic Perspectives 30, no. 4 (November 2016): 199–220. © 2016 American Economic Association

As Published: http://dx.doi.org/10.1257/JEP.30.4.199

Publisher: American Economic Association

Persistent URL: http://hdl.handle.net/1721.1/114172

Version: Final published version: final published article, as it appeared in a journal, conference proceedings, or other formally published context

Journal of Economic Perspectives—Volume 30, Number 4—Fall 2016—Pages 199–220

Village and Larger Economies: The Theory and Measurement of the Townsend Thai Project

Robert M. Townsend

I have spent close to 20 years cataloging transactions between households in Thai villages, along with a research team. Just this past summer, we documented a number of ways in which even relatively poor villages have money markets not dissimilar in some ways from New York financial markets, with borrowing and repayment passing along links in credit chains. In another project, we have been looking at month-by-month school attendance, grade level completion, and graduation for children in these villages, following them from birth to graduation. This article tells the story of how I ended up in such endeavors.

First of all, why villages? Villages can be viewed as economies—not closed and self-contained economies, of course, but spatially concentrated units that trade with other villages and with the larger regional and national economy. More broadly, a set of villages may be connected to each other in the way that households within a village are connected to each other. For me, starting with villages made sense from the standpoint of general equilibrium theory. To some, general equilibrium theory may seem abstract and irrelevant. But in villages, the entire endeavor of modeling actual economies comes to life. In particular, assumptions about endowment, technology, and heterogeneity as well as contracts, markets, and institutions can be based on measured reality. Research on village economies has become commonplace in development economics, although often not proceeding from this general equilibrium perspective.

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† For supplementary materials such as appendices, datasets, and author disclosure statements, see the article page at
http://dx.doi.org/10.1257/jep.30.4.199

doi=10.1257/jep.30.2.199

I first studied medieval villages using historical data, and then studied villages in India using data from the International Crops Research Institute for the Semi-Arid Tropics (ICRISAT). Wondering myself if the three primary villages of the ICRISAT data were representative of the entire developing world, as it sometimes seemed from the existing literature, and having a personal attachment to Thailand through my wife, I set out from our home in Chiang Mai, in the north, in the early 1990s to study villages in that area.[1]

My approach in Northern Thailand might seem on the surface to have been a traditional anthropological method. Instead, it shows how I approach research and modeling. I went with a list of questions for the villagers, supplemented with open-ended and relatively unstructured interviews, while I was eating and sleeping in the villages over sustained periods of time. I would then retreat to my home base to review what I had learned, to ponder the puzzles that emerged, and then to rethink my models and predictions. This process was iterative and Bayesian.[2] Facts and assertions were sorted through the lens of theory, and then insights from theory were incorporated into new conversations in the field. Ultimately, final questionnaires were designed and administered to a small sample of households across ten villages in three distinct areas (Townsend 1995).

Starting in 1997, I decided to scale up this endeavor to four new provinces. After fielding the survey in these new provinces as a baseline, we selected a subset of villages to receive follow-up resurveys. In 1998, realizing that more fine detail could be developed with intensive monthly resurveys, we expanded our operations to a new but smaller sample of the baseline villages. Then, seeking to gain a better picture for the whole country, we extended the annual resurveys to more provinces and to urban areas within the previously selected and newly selected provinces. We are currently at 19 years of annual resurveys, 18 years of monthly resurveys, and counting. The Townsend Thai project is arguably the longest running panel anywhere in the developing world.

Although the Townsend Thai project collects a very broad range of data, concentrating on measurement would miss a substantial dimension of what makes the effort distinctive and fruitful. After all, government statistical agencies in low-income countries now carry out a substantial number of household surveys, including the Living Standards Measurement Surveys of the World Bank, the Family Life Surveys of Rand, and other unique, specialized databases used in development studies. The key contrast is that the Townsend Thai project is at its heart rooted in

[1] Christopher Udry (1995) and I were among the first of a new wave of development economists to actually do field research, though now of course it has become standard. At the time, some prominent development economists argued against going into the field to gather data, believing this should be left to country governments or international agencies. However, there is a grand tradition in agricultural economics to be on the ground and in touch with local populations.

[2] In general, the Bayesian approach to developing models offers an interesting perspective on randomized control trials, as well. A pure Bayesian armed with a prior, and seeking only to convince herself after compiling new evidence, will not choose to randomize (Banerjee, Chassang, and Snowberg 2016).

a drive to describe village economies through the lens of economic theory—specifically, the Arrow–Debreu model of an economy in general equilibrium—which has led to questionnaires and on-site data collection methods designed with theory very much in mind.

Moreover, I have a passion for realistic assumptions and panel data. Structure imposed on a model should be consistent with measured reality, and predictions should be free of functional form and distribution assumptions, as in nonparametric econometrics, in so far as possible. Both are ideals of course, and compromises must be made as practical considerations arise, but at least we are clear about the goal. The Townsend Thai project, in pushing this version of the structural approach, and in keeping the surveys running on the same households over time, might be said to have anticipated 20 years ago the current dynamic approaches to modeling household behavior.

In the following pages I will describe this underlying theoretical vision of the village as embodying an Arrow–Debreu economy. I review my earlier work on village economies, to set the stage for what I was seeking to accomplish with the Townsend Thai project. After a synopsis of the evolution of the Townsend Thai project, I describe research based on this data: the extent of risk-sharing within and across villages; how obstacles to trade vary across regions and by urban/rural status; taking advantage of natural experiments such as the effects of a Thai government program to provide microfinance; and using the longitudinal nature of the data to look at patterns like trends in returns and inequality. The concluding sections of the paper consider some of the advantages of long-term panel studies, the influence of this study on other research efforts, and some horizons for future research, both with these data and more generally.

Village Economies in Theoretical Perspective

An "economy" in the language of general equilibrium theory consists of a specification of the fundamental objects, as in Debreu (1959), Arrow (1964), and McKenzie (1959). This includes an underlying commodity space which includes consumption goods and other outputs; land, labor, capital and other inputs; locations; the passage of time to recognize dynamics; and states of the world to capture uncertainty. These commodity spaces can be chunky, as with indivisible goods (think of savings and loan rotating credit associations, which are winner-take-all, or an investment that requires a minimal scale which limits entry). Commodities can be transformed into one another as in production, or moved from one point in space to another at a cost, as in the transport of goods.

After the commodity space comes a specification of the preferences of agents, such as the assumption that households may maximize expected utility. Ownership comes from endowments, including factors of production (land, labor, and capital) and access to the technologies of production, storage, and trade. All of these can be measured, in principle.

The tools of general equilibrium theory also allow us to take a view of a village in partial equilibrium, with some balance of payments constraint on its relation with the outside world. The village economy can include fiat money which is valued as social currency, although typically not by the way it is used in the village, but rather by outside, economy-wide considerations. We can just extend the commodity space and include this currency as another good. Generalizing, one can include a potentially limited array of outside assets or liabilities, such as savings accounts or credit from external lenders.

One can then consider the allocations that are observed against a benchmark of economic efficiency. In the case of private information and/or other obstacles such as limited commitment, allocations will not be first-best, but rather will be, or at least should be, constrained Pareto optimal. To continue with the village metaphor, such allocations can be determined by maximizing a weighted sum of utilities of individuals and households in a village, subject to a variety of constraints: resources constraints (or open economy village-wide budget constraints); incentive compatibility constraints having to do with private information and moral hazard; constraints having to do with limited commitment; and so on. This maximization subject to constraints is a mathematics problem. Indeed, one can refer to this problem in a shorthand way as "the planner's problem," although the concept of a "planner" is really just a stand-in for the math problem that delivers the outcomes of the community of the whole.

In terms of implementation, one can suppose that allocations in a village will be achieved in a decentralized fashion, as with complete markets. But one need not make any such assumption. For example, the trading of Arrow–Debreu securities (that is, claims with payoffs contingent on the states of the world) is not necessary. Other institutions and mechanisms are allowed, and indeed are often one of the more interesting aspects of investigation. For positive economics, prediction comes from the premise that social forces will seek to move to an allocation in which some can be made better off while not making others worse off. For normative economies, if the allocation is not constrained optimal, then Pareto improvements are possible, hence providing natural guidelines for policy. There is some tension between the positive and the normative approaches, but the overall perspective is that models should be taken seriously, and they will talk back to us with conclusions that were not necessarily obvious a priori.

It may seem peculiar to some readers that a data-gathering project and accompanying empirical work should begin with an Arrow–Debreu theoretical perspective, but of course, drawing a distinction between theory and econometrics is fallacious. Koopmans (1947) offered a prominent statement of their connection in his "Measurement without Theory" essay, where he critiqued the National Bureau of Economic Research project of business cycle measurement (as discussed in Burns and Mitchell 1946). His main argument for the use of economic theory was that it allows economists to make predictions, especially counterfactual predictions to evaluate potential policy. In a more recent comment along the same lines, Rubin (2005, p. 3) notes: "There is no assumption-free causal inference, and nothing is

wrong with this. It is the quality of the assumptions that matters, not their existence or even their absolute correctness. Good researchers attempt to make assumptions plausible by the design of their studies."

Some Village Research Antecedents

Thinking about villages in the context of Arrow–Debreu economies was the basis for my studies in economic history and in various countries. Because these predecessors shaped my thinking about the Townsend Thai project, it is helpful to consider them briefly.

The orientation of my work on the medieval village or estate economy was to explain observed institutions through the lens of theory, or establish puzzles that could not be so explained. The theory part is clear from the subtitle to that book, "A study of the Pareto mapping in general equilibrium models" (Townsend 1993). The book focused on risk and its many sources, both aggregate and idiosyncratic. For example, idiosyncratic risk was a force behind one of the most salient institutional features: fragmented land holdings. A typical farmer could hold 50–60 spatially separated strips of land scattered through the village, as a hedge against risk. Yet despite low cross-crop and cross-plot correlations that apparently drove this fragmentation, the variation in aggregate yields was so high that episodes of starvation occurred approximately every 12 years. Storage, either in the bin or as seed in the ground, had an incredibly low return. This research offered a calibration of a macro model that could rationalize the observed patterns of crop variability, carryovers, and planting decisions. Labor supply came in large part from duties and works supplied to the lord of the manor as a function of landholdings. We have no record of actual consumption, and thus no ability to link consumption directly to landholdings. However, land division was apparently a more useful method of sharing risk than a system of transfer payments to offset idiosyncratic risk. This is built into a moral hazard model that seeks as part of the math problem to limit transfers and thus showed how the number and location of initial "endowments" of land would matter. The model predicted a substantial part of the high degree of observed land fragmentation. This theoretical basis for decisions about location, and the condition of plots, became a basis for considerable subsequent work, some of which is touched on below.

The measurement used in this study of the medieval village came from historical material, like the accounts of the Bishop of Winchester, reported in Titow (1972) and other sources gathered and studied in McCloskey (1976). It would not be misleading to refer to many of the sources as government administrative data. I used virtually all available data to calibrate the models.

Following this work, I turned to village India to bring this analysis of risk and insurance to a more contemporary context. In this research (Townsend 1994), the focus was on taking the theories of Wilson (1968) and Diamond (1967) of the optimal allocation of risk bearing to the consumption and income data gathered

by the International Crops Research Institute for the Semi-Arid Tropics (ICRISAT). For an efficient outcome, aggregate risk at the village level—in the form of what is left of crops after intertemporal smoothing—should be shared among households. Put more dramatically, controlling for the aggregate shocks, the idiosyncratic variations in household-specific income should not influence consumption at all. Analysis of the panel data revealed that this benchmark for efficient outcomes could not be rejected in many specifications, and in the cases where it was rejected, the coefficient on how idiosyncratic household income affected consumption was typically small. The implication was that desperately poor households in undeveloped villages achieve, or come close to achieving, a high degree of risk-sharing, as if the markets (or other social institutions) for risk-sharing were complete, or almost complete. This remarkable finding and the resulting controversy helped to fuel interest.[3] The view here is that the extent of risk-sharing should not be based on a theoretical presumption, but instead should everywhere and always be treated as an empirical question.

In the early 1990s, the ICRISAT data were one of the few panel data sets available anywhere in the world containing consumption and income data for the households living in villages in low-income countries over time. In those early days, a researcher wanting access to this data was required to visit the Institute near Hyderabad, India, and from there proceed to visiting the sampled villages. The journey could be arduous, but it did teach researchers about the potential importance of context—and sometimes there were serendipitous events that shape careers.

For example, during one of these trips, co-author Ned Prescott discovered a fascinating institution: cropping groups in which multiple tenants jointly farm the land of a single landowner. Theory was developed to understand this corporate form and its internal incentives, as well as how it could coexist with both single tenancy and a spot market for bullocks (Townsend and Mueller 1998; Mueller, Prescott, and Sumner 2002; Prescott and Townsend 2006). This same theory can be applied in many other contexts, including complex financial institutions and the role of agency in New York's financial markets. This experience with cropping groups reinforced the idea that data collection and measurement in village economies needed to include contracts and institutions, which in turn needed to be spelled out in baseline forms with possible variations for labor employment contracts, credit, land rental, and so on. It also showed that data is needed on the environment, on allocations as outcomes, and on these contracts and institutions.

My work on Northern Thai villages added to this historical and contemporary experience (Townsend 1995). The selected villages are described in the language of technology and endowments: crops and their varieties; plots and their location, slope and soil characteristics; variability in crop yields; and mismatches in the timing

[3] A parallel literature developed around the same time in the United States, with some accepting the null and others rejecting it, using different datasets and different methods. For examples, see Altonji, Hayashi, and Kotlikoff (1992), Attanasio and Davis (1996), Mace (1991), Cochrane (1991), and Altug and Miller (1990).

of good and bad years. Responses to shocks included labor supply and potential borrowing and lending. In other villages, I was left with risk-response variations across households which suggested that Pareto improvements would be possible, which made me think more deeply about policy. In sum, the Northern Thai village study reinforced the importance of measurement at the village level, and fruitful ways of doing that measurement. It pushed me toward the idea of a further, larger-scale but comparative study of villages near to one another, in small sampling areas, but with variation across provinces that differed in development and openness. It made me determined to do this in a way that held potential to contribute to economic policy.

Synopsis of Townsend Thai Project

To begin, we conducted an initial, baseline survey in 1997 that included villages from four provinces: two in the relatively poor agrarian Northeast and two in the developed Central region near Bangkok. The selection of provinces was based on a pre-existing socioeconomic income and expenditure survey by the Thai government, so that we could take advantage of existing government data. The idea, roughly, was to use the cross-sectional variation as a substitute for the passage of time; we had no idea then that the project would last for so long. Within each of these four provinces we chose 12 tambons (a small subcounty region) per province. The tambons were chosen at random but with an environmental stratification (as discussed in Binford, Lee, and Townsend 2004). The idea was to end up with a sample in which there would be idiosyncratic regional shocks, so that risk-sharing could be better tested. Landsat imagery classified the types of ground cover. The idea was to distinguish environmental variation from historical institutional variation by having a constant underlying environment for clusters of villages. Finally, four villages per tambon were selected randomly.

Within each village, households were selected at random from rosters held by the headman. The 1997 household survey thus totaled 2,880 households (15 households for each of 192 villages). There are also survey instruments for the 192 headmen, as well as for 161 pre-existing village-level institutions (such as production credit groups or rice banks) and 262 joint liability groups (that is, they underwrite the risk of each other's loans) for customers of the Bank for Agriculture and Agricultural Cooperatives (BAAC). There are as well 1,920 sets of soil samples with measurements of organic matter and cation exchange capacity (which is the soil's ability to hold on to nutrients) from 10 of every 15 households per village. The first collection of these data was in April/May of 1997, constituting the baseline survey for rural areas.

When the unanticipated Thai financial crisis erupted in July of 1997, we wanted to assess the impact of what seemed like an aggregate shock. Thus, we began in 1998 the first of many subsequent rural annual resurveys in four tambons in each of the original four provinces, chosen at random from the original baseline sample. These surveys are done regularly, once a year, with teams of hired enumerators.

Also in 1998, an additional tambon per province was selected for fielding an intensive monthly survey, starting in August of that year, to get the details on labor supply, use of cash, and many other features that are only possible to obtain accurately with frequent recall, high-frequency data. The subsample was chosen to be consistent with the aforementioned environmental specification—that is, with similar environments across villages—but with variation in a priori formal and informal institutions (as they appeared in the 1997 baseline instrument). The detail of the monthly crop production data is a revealing example, used in Felkner, Tazhibayeva, and Townsend (2009) to assess the impact of climate change. We have measurements of short-term inputs (seed/seedling, fertilizer, pesticides, herbicides, hired labor and exchange labor, and rented capital equipment), and outputs (harvests). We distinguish production by stages (planting, maintenance, harvesting) and have obtained, as noted earlier, measurements of the soil as in the annual data and also weather shocks (measured with village-level rainfall gauges, temperature, and soil moisture readings). For the monthly environmental and socioeconomic data, gathered continuously throughout the year, we use local enumerators who are in permanent residence in the area and who use the above-mentioned anthropological-type methods when doing interviews.

The scale of the survey expanded, so as to be more nationally representative: two more provinces were added in the South in 2003 and two more in the North in 2004. Subsequently, one province was dropped in each region: one in the South became too dangerous because of an insurgency, and one in the North was dropped for budget reasons. An urban baseline and subsequent annual urban resurvey were added beginning in 2005 in all six remaining provinces, so as to compare urban neighborhoods to rural villages within in the same province, and to think about a province as a regional economy. In 2013, we added monthly surveys in urban areas in the original four provinces. While we do not have as long a duration for any of these latter surveys, we have achieved a larger scale. As of the last reporting period, the Townsend Thai project includes 3,890 households.

This synopsis emphasizes the breadth and depth of the Thai project data. However, the importance of the theoretical base for collecting and organizing the data should not be lost. Collecting these data has been key to the types of analyses that can be performed. Indeed, we have been able to model the entire Thai national economy and its internal labor migration and flow of funds based on the selected samples.

Risk Sharing

The optimal allocation of risk bearing suggests that consumption and other variables should be sensitive to aggregate shocks, but not to idiosyncratic shocks. Using the monthly panel data from the Townsend Thai project, in Chiappori, Samphantharak, Schulhofer-Wohl, and Townsend (2014), we seek to test this benchmark allocation of efficiency, taking advantage of the unusual length of the panel.

Again, theory suggests that household consumption should depend on time fixed effects that capture village aggregate risk/consumption. The long panel allows us to estimate heterogeneity in risk aversion: household consumption should move with aggregate village consumption, but more risk-tolerant households should absorb more of any aggregate risk. With 84 months of data, we were able to estimate these time fixed effects and heterogeneous coefficients. We found substantial variation across villages in the aggregate shocks that are inferred from the data, with a monthly standard deviation of 13 percent. We also found significant diversity in how aggregate shocks are borne across households, which suggests differences in risk preferences across households. Interestingly, these risk preferences turn out not to be related to wealth (which is also an implication of the as-if-complete-markets model). The bottom line was that we cannot reject the benchmark model's null hypothesis that household-specific idiosyncratic income risk does not influence household-specific consumption. Idiosyncratic risk is quite well pooled, essentially smoothed away to zero.[4]

The findings also offer a policy punchline. We conducted a hypothetical intervention, looking through the lens of theory. Consider the possibility of pooling across many villages so that what were aggregate shocks at the village level are now idiosyncratic and insurable shocks in the larger regional or national level. To put this point another way, we introduce in the thought experiment an indexed insurance product that, if actuarially fair, would replace observed variation in a village's aggregate shock with its mean. Such insurance would help the more risk averse. For example, risk-averse households in some of the villages are willing to accept up to a 3 percent consumption loss on average and still find such an intervention to be welfare-improving. But on the other side, such an intervention would actually harm the more risk-tolerant households in the same villages, who have implicitly been providing insurance to their more risk-averse village neighbors and receiving an implicit premium for doing so. These risk-tolerant households require up to 4 percent higher consumption on average, post-intervention, to be compensated. This lesson seems important, especially with continued efforts worldwide to introduce new types of insurance products, like ones indexed to rainfall, as if there were nothing indigenous already out there. Such well-intended interventions could actually harm some risk-tolerant households (again there is no correlation of risk aversion with wealth, and so the more risk-tolerant are not necessarily more wealthy).

The implications of informal risk-sharing arrangements as a community insurance mechanism keep showing up in work on other topics. For example, we can return to the observation that markets for the physical assets of households are relatively thin. Obviously, households in low-income countries are not trading equity

[4] In related work (Chiappori, Samphantharak, Schulhofer-Wohl, and Townsend (2013), we also estimate risk preferences with a less demanding portfolio choice model. In that paper, assets with uncertain returns are chosen by each household to satisfy intertemporal optimization in consumption and returns, and we do not have to take a stand on cross-household risk sharing. Nevertheless, the findings are similar—that is, the measures of heterogeneous risk preferences across the two studies are well-correlated.

claims on the production projects associated with these assets in a formal village-level securities market (not yet, anyway). There is no stock market and no market in state-contingent securities. Nevertheless, one can extend the risk-sharing theory to get implications for the portfolio of projects held by households and the associated rates of return across households, as in Samphantharak and Townsend (2016). In this paper, a rate of return can be computed each month for each household, as the flow of profits during the month and average physical assets held during the month. We then have returns over all sampled households and all months. The principal residual risk in the village economy is at the village-aggregate level, so what matters, and cannot be diversified away, is the covariance of a household's return on assets—profits divided by capital—with the village average return. The higher the covariance of the household's return with the aggregate return, the higher the risk, and so the higher must be the expected (average) return, to compensate for that risk. Likewise, according to the theory, idiosyncratic risk can be shared in principle within the village, and can be pooled away. With the relatively long panel of the Townsend Thai data, means, variances, and covariances of returns can be measured reasonably well and so these implications of the theory can be tested.

We found that idiosyncratic risk dominates total risk, with the percentage of the total ranging from 55 to 88 percent. However, in terms of risk premia—that is, deviation from the risk-free rate as compensation for risk—covariate risk is what matters most. The aggregate risk premia range from 67 to 80 percent of the total in three of the four provinces where the model fits well. An "autarky model" where households have no method to insure against risk would imply that covariate and idiosyncratic risk enter symmetrically into risk premia, but this model is soundly rejected in the data. Idiosyncratic shocks do show up in risk premia, because they do influence consumption, but by much less than the autarky model would imply.

The benchmark model and our risk decomposition guide us to a salient policy conclusion: namely, adjusting for idiosyncratic and aggregate risk separately—as dictated by our theoretical framework and the data—makes a large difference to inferences about underlying rates of return. Poor households are more exposed to village aggregate risk than their more wealthy neighbors, and thus the poor have high unadjusted rates of return. But poor households with high returns are not necessarily credit-constrained within their current set of activities on the intensive margin—that is, in wanting to expand but unable to get the credit to do so. Richer households have risk-adjusted returns which are higher than the risk-adjusted returns of the poor in the Central region, and no lower in the Northeast. The relatively poor households are constrained in a different sense, on the extensive margin, unable apparently to enter occupations and sectors of the relatively rich.

Within the village, or within the larger tambon, family and financial networks seem to be the informal institutional mechanism underlying many of these insurance results. In Kinnan and Townsend (2012), an initial census for the monthly data enumerated all structures, where each individual eats and sleeps, and kinship ties through three generations. Though the sample of households within a village is drawn at random, transactions of the sampled households with transaction partners

identify those partners for all village residents (that is, mapping even to those households that were in the original census, but not resampled in the monthly surveys). We are able to exploit this in-depth knowledge by extending the basic risk-sharing regression to include the interaction of idiosyncratic income with whether a household is in an active financial network of gifts and loans, now actually measured, or whether or not a household is in a kinship network in the village. We found that these networks are helpful in at least partially insuring idiosyncratic risk. Measured gifts also showed up in Samphantharak and Townsend (2016) as smoothing much of the idiosyncratic shocks.

One particularly striking institutional mechanism works like a refinancing credit chain, as noted at the outset of this article. A household borrows from a formal source (outside government bank or village fund), has difficulty in paying this off in a bad year, borrows short-term from informal sources (family, friend, moneylender) to pay off the formal loan, is then in good standing and so borrows again formally, and finally pays off the informal bridge loan (Sripakdeevong and Townsend 2016). Such patterns should serve as a reminder to policymakers that informal (and typically unmeasured) arrangements can be important. They also make the point that markets in village economies can be quite sophisticated in their functioning.

Identifying Obstacles to Household Interactions

Interconnections among households are likely to matter, whether in an informal network of loans and gifts or in the sense of registering to underwrite the risk of someone else's formal sector loan. Because obstacles to trade will matter for observables, we can consider theoretical models that include different obstacles, and then infer which obstacles are most likely given the patterns that emerge from the data. A variety of studies taking this general approach suggest consistent conclusions about the obstacles to trade and how such obstacles vary by rural/urban status.

In Ahlin and Townsend (2007), we take this approach in work on joint liability loans given by the Bank for Agriculture and Agricultural Cooperatives (BAAC). Cooperation is modeled as the ability to commit costlessly to a set of actions that is Pareto optimal within the borrowing group. We considered a range of models in the existing literature, not previously tested, each of which embodies a different obstacle to trade: moral hazard, limited enforcement, information limits on screening borrowers, and others. The implications of these models for repayment of loans will vary. In the case of moral hazard, the ability to act cooperatively leads to less risk-taking by eliminating a borrower's ability to free ride on a partner's safe behavior. Thus, cooperation raises the repayment rate. However, in a model with limited enforcement, cooperation can lower repayment by making possible binding agreements not to use excessively harsh penalties. Overall, the limited enforcement model does best in the poorer, low-infrastructure Northeast. The screening model does best in the wealthier Central region. The patterns suggest that strategic default

may be a more prevalent problem in low-infrastructure areas, while information problems may be more prevalent in more developed areas.

In an approach that combines data on occupation choice and the financing of investment, in Paulson, Townsend, and Karaivanov (2006), we look at the entry decisions of households into business. We also modify the full information full risk-sharing benchmark to accommodate a variety of potential obstacles to trade with moral hazard in effort or the ability to default/walk away. We found that the quantitative mapping implied by a moral hazard model fits best in the Central region, whereas either a limited commitment model or a mixed financial regime with both limited commitment and moral hazard in combination fits best in the Northeast. Likewise, as assets increase, borrowing decreases in the Central region, probably because increased wealth means more self-financing and less moral hazard; however, as assets increase, borrowing increases in the Northeast, as those at a binding collateral constraint can increase borrowing when collateralized assets increase.

In Karaivanov and Townsend (2014), we extend this work, focusing on households running businesses over time, rather than the entry decision, and also expanding the variables to include consumption, income, capital, and investment. Using the rural monthly panel, we apply dynamic programming, linear programming, and maximum likelihood methods to find that a relatively simple borrowing-lending regime fits the overall data best. However, when we compare the rural to the urban data, a moral hazard model fits the capital stock transitions best.

This body of research papers, using distinct data and models, leads to consistent findings. If the policy goal were to try to alleviate the obstacles to cooperation noted here, then making contracts more complete and alleviating collateral constraints would be the suggested policy in rural areas, but mitigating information problems would be the suggested policy in urban areas. To put this more dramatically, a one-size-fits-all reform is likely to be unproductive, or even counterproductive, in either rural or urban areas.

Microfinance: A Natural Experiment Example

One advantage of fielding a long-term survey over many years is that the sample experiences natural experiments. We cannot do experiments in which capital is injected at random in the cross-section of villages. Yet in 2001, the government of Prime Minister Thaksin introduced a village-level saving and loan association, funding each with one million Thai baht (roughly $24,000), which was intended to make micro-finance loans within these villages. With five years of data in hand, this presented an exquisite opportunity for a quasi-natural experiment. Each village got the same amount of funds, yet the number of households in a village varied considerably, leading to random variation in the size of the intervention when measured on a per household basis. Moreover, that number of households is largely unrelated to any measured economic variable we can find in our own or in secondary data. At the time of the policy intervention, there were roughly 77,000 villages in Thailand,

and so the total used for funding was roughly 1.5 percent of GDP, making it one of the largest microfinance programs ever.

In several papers (Kaboski and Townsend 2011, 2012), we investigated the effects of this experiment with reduced form nonstructural statistical models, as a preliminary fact finder, and with a structural model. We also used the parameters to carry out alternative, counterfactual experiments, as if the program had been designed differently. The nonstructural reduced form paper (Kaboski and Townsend 2012) used the annual panel data and inverse village size as the instrument. An increase in total short-term credit increased consumption (by more than credit), increased agricultural investment and income growth, but decreased overall asset growth. We also found in the monthly data a positive effect on wages. However, some of these effects are attenuated over the years. Short-term credit remains high, but the effect on consumption and income becomes lower. Default may have increased but with a lag, the year after borrowing.

Consistent with the inferred obstacles to trade and in the work just mentioned above, we take these facts and construct a structural model, specifically a buffer stock model with a credit limit and a lumpy investment possibility (Kaboski and Townsend 2011). Parameters of preferences and technology are estimated on the five years of pre-intervention data, and then the structural model is used to predict what would happen over the next few years if the credit constraint were loosened. We use the same methods on the model-generated data as in the nonstructural paper, and the predictions compare well to what happened subsequently in the data. The point here is that the model provides an interpretation: A surprise increase in future credit availability causes households to run down buffer stock savings and increase consumption. But this jump cannot be sustained in the long term. In general, long-term impacts are something that shor-term randomized controlled trial studies, with baseline and endline, have trouble picking up. However, when Bannerjee, Breza, Duflo, and Kinnan (2015) carried out a longer-term resurvey of additional credit in in Hyderabad, their results complemented what we report here.

These studies and others are also picking up the importance of heterogeneity. In Kaboski and Townsend (2011), we can distinguish various types of households: for example, those near default, hand-to-mouth credit-constrained households, and households on the margin of investing. Some households actually lose with more liberal credit limits, because they can borrow at interest to cover loans coming due and are no longer allowed to default to a reasonably high level of consumption. Other households reduce consumption to co-finance investment. All of this is in contrast to work which imagines one or two types of households only. Again, we are using the lens of the model to understand more clearly this heterogeneity and what happened.

As a counterfactual policy, we evaluate the impact of lump sum grants costing the same amount to the Thai government, and find that this alternative is preferable for many households, though not all. Likewise, some households would gain more, relative to the original program, from credit that is stipulated to finance investment, as ironically this takes away the welfare loss that the would-have-been defaulting

borrowers experienced in the original program. New work and other studies find that households that experience the greatest impact from micro credit interventions on the production side are those already in business and that have higher total factor productivity (Banerjee, Breza, and Townsend 2016). Yet the allocations of loans by the village fund committee arguably appears random if not inefficient (Vera-Cossio 2016). In hindsight, and in thinking about future interventions, one could have designed a better mousetrap.

Constructing Regional and National Economies

Given the strong theoretical roots in the Townsend Thai project and extended sampling ultimately designed into the surveys, it becomes possible to construct and estimate larger model economies based on the measured micro underpinnings.

In Samphantharak and Townsend (2010), we provide a key starting point, by using the monthly data to create a complete set of financial accounts for the sampled households. In effect, we envision these households as corporate firms, each with an income statement, balance sheet, and statement of cash flow. Then, following the steps outlined from the US Bureau of Economic Analysis, in Paweenawat and Townsend (2012, 2014), we use these accounts, aggregate up, and create village-level national income and product accounts—including savings/investment accounts, balance of payments accounts, and flow of funds accounts.

In turn, these accounts can be used to disentangle real and financial factors. In these papers, we establish that there is within-village and across-village sharing of consumption risk, though the latter seems worse than the former. The smoothing mechanisms are also different. Within-village, there is greater use of gifts, but in a typical village's relationship with the rest of the economy, there is greater use of cash and formal borrowing. Regarding investment, we explore the Feldstein and Horioka (1980) relationship of domestic saving to domestic investment. Here we find that investment is not sensitive to savings at the village level, a sign of good cross-village intermediation, but is sensitive once incoming (net) remittances are included in the saving variable. In short, village economies can take investment opportunities even when short of internal funds, by relying on external funds. Likewise, work by Srivisal (2014) uses the flow of funds accounts data at the village level and analyzes the impact of monetary policy, generated at the national, aggregate level but impacting the villages differentially.

Given that obstacles to trade seem to vary systematically by region or by rural/urban stratifications (as discussed earlier), in Moll, Townsend, and Zhorin (2016), we construct and compute steady-state solutions to a model of the national economy with two sectors, limited commitment in the rural sector and moral hazard in the urban sector, as in the micro data and those earlier studies. We then calibrate the model economy parameters around measured differences in the constructed financial accounts across these two sectors, that is, differences in income, consumption/income, capital/income, and wealth, and in the incidence of enterprise. Parameter

estimates for preferences, technology, and the degree of constraint from limited commitment are consistent with parameter values in the literature. At these calibrated values, the model predicts substantial flows of capital from rural to urban areas: 23 percent of capital utilized in urban areas is imported and rural areas lose 39 percent relative to capital utilized. At the same time, there are huge flows of labor in the same direction: 75 percent of labor in the urban sector comes from this migration and rural areas lose 86 percent. Equivalently, the urban sector is 79 percent of the economy's capital and 65 percent of the economy's labor, even though the urban sector is only 30 percent of the population. In other words, we can explain a national economy model with urban concentrations similar to those we see in the data that are based only on varying obstacles to trade across rural and urban areas. Obviously there are other forces behind urbanization, but this surprising effect comes from the integrated model and its calibration, again based on differential obstacles alone.

At the micro level, we see that predictions across sectors are largely consistent with micro facts in the data that gave us the variation in financial obstacles across sectors in the first place. Over the relevant range, credit is strictly increasing with wealth in the rural/Northeast sector and nondecreasing with wealth in the urban/Central sector, as in Paulson and Townsend (2004), and there is much more persistence of capital stock levels in the rural sector than in the urban, as in Karaivanov and Townsend (2014). The model in Moll, Townsend, and Zhorin (2016) also offers some predictions that are validated when checked in the data. The growth of net worth is more concentrated in the urban/Central region than in the rural/Northeast, and distribution of firm size by capital in the moral hazard sector has a skewed right tail relative to the limited commitment sector, as does the Central region relative to the Northeast in the micro data. This line of research shows the value and relevance of constructing a macroeconomic model built on measured micro underpinnings. In particular, the model performs better and parameter estimates are more sensible when respecting this diversity across regions, rather than imagining underpinnings are held in common across all villages and towns.

These models allow the assessment of repressive policies that promote regional isolationism. In Paweenawat and Townsend (2014), we fit a small open economy model with collateral constraints to the rural data. We then conduct counterfactual experiments to determine gains and losses from potential restrictions on trade and on capital flows, separately and in combination. The effects can be substantial. For example, limiting capital outflows can lower interest rates, leading to more capital-intensive production and thus the hiring of more labor, thereby raising wages. But the low interest rate hurts those with savings. However, the same policy in another region can generate different numbers even using the same model, because baseline local conditions and calibration to locally observed paths produce different counterfactual predictions.

In a yet more explicit model, in Ji and Townsend (2016), we not only consider local markets within the Thai national economy—1,220 of them—but also the integration of these markets domestically into the economy as a whole, as well as

opening that economy up to observed international capital flows. The Townsend Thai data has its variables on cash holdings and loan-to-collateral ratios, and both credit and savings are an explicit part of the model. These can be used in conjunction with secondary data sources and then loaded onto a "geographic information system" (GIS) with other key variables: roads; the locations of bank branch openings from the Bank of Thailand; wealth, population, and other village characteristics from Community Development Department village census; the Thai National Population and Housing Census which helps identify municipal populations; and the Socio-Economic Survey which measures income and wages. The model allows us to distinguish the effect that branch expansion vs. international capital flows have on growth, inequality, and credit access.

Longitudinal Studies

The long-term trends revealed in our 18 years of monthly data on Thai villages are dramatic. Inequality falls, for example, with the bottom 50 percent having an increasing share of the wealth. Rates of return on household enterprise converge, rising for the rich, from 5 to 10 percent, and falling substantially for the poor, from 28 to 12 percent per year.

In Pawasutipaisit and Townsend (2011), we establish some of the underlying mechanics within the Thai context. Low-wealth households have higher overall rates of return, including risk premia, and those coupled with higher own-savings rates (investing profits back into their own businesses) boosts their income over time on average and lowers inequality. This autarky-like mechanism takes many years to play out.

In ongoing work (Ru and Townsend 2016), we suggest an effect from financial intermediation as well. Gifts seem to play a role not only in smoothing consumption deficits, but also in financing investment. With that in mind, we try out a costly state verification financial/information regime, combining the multiperiod contracts in Townsend (1982) with the costly state verification model of debt in Townsend (1979). This model has the property that debt can be used to finance investment despite repayment problems: When low profits make repayment difficult, a cost is incurred, so that the state of an investment project is made known to creditors. With multiple time periods, there are also gains to enduring relationships, paying more from returns when profits are high with the advantage of getting more favored treatment later when returns are low. This multiperiod costly state verification regime fits the overall data best for relatively poor households in villages. Indeed, consistent with earlier results, the costs of verification are lower for households with kin in the village. Finally, though this finding is only suggestive, this mechanism may have improved with the advent of the village funds, as the simpler borrowing and lending financial regime fits best for everyone, including the poor, before the intervention, but the multiperiod costly state verification regime fits best for the poor afterwards and does not attenuate over time. One speculates that the quasi-formal village funds

were somehow a catalyst, improving the performance of the pre-existing informal sector.

As we continue to gather panel data, we become able to address life-cycle research topics. We can see children born, school attendance, and soon a sufficient number of children who have grown up to have jobs and wages, as noted at the outset. We can see middle-aged households initially in their most productive years now quasi-retired, and others with disabling health shocks reduced to zero income at earlier ages. The longer we stay in the field and gather the data, the more the research possibilities grow.

Advantages of Long-Term Studies and Measurements

The longevity of the surveys and the repeat interviews build trust and thus reliability. Interviews are conducted in a conversational style and do not seem to be tedious for households. Enumerators have largely memorized the questionnaires, and of course take extensive notes during the interview, so that specific modules can be filled in afterward. This approach allows for eye contact and one-on-one back-and-forth during the interview, allowing sensitive topics to be revisited as conversations proceed. Our re-interview rates are quite high. For the monthly surveys, our resurvey rate has been over 99 percent per year: 602 out of the 710 original households starting in 1998 were still being resurveyed in 2014. For our annual survey, the average resurvey rate has been 98 percent during the last five years.

There is also a human story to the logistics of fielding the survey, the experience among enumerators, supervisors and staff from headquarters. These experiences are recounted in *Chronicles from the Field* (Townsend, Sakunthasathien, and Jordan 2013), a book aimed at both a general interest audience and providing supplementary material for anyone teaching a course on survey design and implementation. Rules matter in doing a survey, but additional human and logistical aspects must also be taken into account.

Another advantage to longevity is that enumerators remain in the field and in contact with the households, thus allowing researchers to follow-up both with individual households and to field systematic supplemental questions. For example, follow-up has been used when the numbers in the financial accounts appear as outliers. Supplemental questions have been asked concerning individual health insurance histories, behavior and attitudes toward discounting and commitment, and reconstruction of payment histories that distinguish deposits of cash from electronic deposits.

These benefits of long-term high-touch interviewing come with costs. Our interview techniques do not accommodate computer-assisted personal interview techniques, so we deal with mounds of paper and printing costs. We worry about the representativeness of the remaining sample, because even low attrition rates accumulate over time. There is chronic worry about funding, along with grant proposals to write and rewrite and also developing contingency plans. Any serious gap in

funding would likely undo the long-term panel. As a cautionary example, the original ICRISAT sample had very low attrition, but many households were lost in the gaps between restarts. At present, new funding from the Bank of Thailand and the Thailand Research Fund along with continuing grants from the US National Institute of Child Health and Human Development (NICHD) will cover the costs for the next two years of field research, and there are discussions about going further.

Creating a local organization called the Thailand Family Research Program, with my close collaborator Sombat Sakunthasathien, enabled us to field the surveys and helped to keep down some of the costs associated with a research endeavor of this kind. TFRP kept a lean budget and fully 75 percent of its budget went to direct personnel costs, mostly the small army of enumerators, field editors, and coders who run the operation on the ground. Even so, the costs are significant, with about 31 full-time office staff and a larger number of field staff, around 70 enumerators, working part-time but year round.

Influence on Projects Elsewhere

The rarity of projects of this length and scale give the impression that such projects are difficult to conduct, but here are a few somewhat comparable projects. The above-mentioned ICRISAT studies on villages in India were conducted in 1975–1984, 1989, 2001–2009, and 2009–present. But for perspective, the ICRISAT second-generation survey has 500 questions from 9 modules, but the Townsend Thai monthly has 3,500 questions from 24 modules. The annual resurveys with more villages and urban neighborhoods are less intensive, but add to these totals.

Other roughly comparable projects are the large-scale multi-year panel surveys in Ghana and Tamil Nadu, India, being carried out by Yale's Economic Growth Center (EGC). These nationwide panel surveys will ultimately span 15 years each, with resurveys occurring every three years, providing depth across years and spotlighting socioeconomic mobility. They are not location-based per se, but rather track migrants who would otherwise be eliminated from the samples. Another ambitious endeavor is the Kavli Human Project, which plans to survey approximately 10,000 residents in about 4,000 New York City households over the next several decades.

But with these larger projects duly noted, smaller projects are easier to carry out. We generated many useful research papers from the 1997 baseline and first few years of annual resurveys. Even if we had stopped at that point, we would have learned much. The Thai modules have served as prototypes for others. For example, in Chile, a survey of small household businesses (*Encuesta de Microemprendimiento*) began in 2009, which complements the Central Bank of Chile household surveys. In Mexico, a study of BANSEFI credit unions used both the Townsend Thai project institutional and household questionnaires. I was recently in China as an advisor to help implement key add-ons to an ongoing survey modeled after University of Michigan's Panel Study of Income Dynamics. The household financial accounts created

as part of the Thai project have been brought back to United States and are now being integrated with payments diaries and surveys from the Federal Reserve Bank of Boston (Samphantharak, Schuh, and Townsend 2016). The GIS database archive developed as part of the Townsend Thai project is being adapted and implemented in Brazil and China.

The Townsend Thai data have been continuously cleaned and uploaded to Dataverse (at https://dataverse.harvard.edu/dataverse/rtownsend). This data also contains the relevant secondary data sources. While application must be made for access to the subset of secondary data that is proprietary, the primary, cleaned, data are available to all. At last count there have been 90,000 downloads of the public data. In addition to my collaborators and co-authors, numerous other researchers have found the Townsend Thai data to be a useful tool, with almost 50 papers citing the use of the data, including at least 11 graduate students utilizing the data in their dissertations.

Conclusion

The Townsend Thai project is a theory-based data collection endeavor, measuring and mapping village and larger economies into general equilibrium frameworks. Theory determines key questions to ask of households, about contracts and mechanisms, for example. Theory can guide sample selection, so as to ensure data are adequate for tests of null hypotheses. In turn, observations from the field and findings from theory-based analysis of the gathered data can lead to new questions.

This way of doing research has given rise to a literature on village economies, and then extends to thinking about models of regional and national economies based on detailed micro underpinnings. The discussion in this paper has reviewed a number of findings, implications, applications, and lessons learned. What are the next steps? Among the many, we can single out here three leading examples of where I believe progress can be made.

A first key area has to do with an even better understanding of informal financial networks. We know in the Thai data that these networks are playing crucial roles in the reallocation of risk. But many questions remain unanswered. How have households managed to come up with these institutions, turning bilateral trade into functioning multilateral links that resemble sophisticated money markets? How aware are households of how these networks are functioning? Are there costs in coordination or perhaps missed opportunities? How do these networks change with increases in external financial development? Are informal and formal financial sectors substitutes or complements for each other? What are the implications for regulation?

A second area for further research concerns the industrial organization of financial service providers. Does the private sector compete with the public sector, deliberately partitioning itself off from the mainstream by targeting the poor, or rather does it act in concert with the public sector, driven by political or altruistic

motives? Is the overall outcome efficient? How do we integrate the theory of contracts into the supply side, making both contracts and the location of branches an endogenous outcome, with tools that allow this conceptualization to be taken to data? What is the actual degree of competition among commercial banks? Methodologically, the exciting prospect here is a synergistic merger between development economics and industrial organization to address economies in transition.

A third area for research concerns bringing the kinds of models, data, and analysis from the Townsend Thai villages project to US communities. This line of research received great impetus from the US financial crisis. Considerable localized data is available at the zip code, census block, Metropolitan Statistical Area, commuting zone, county, and state level. The contributors to the US literature use creativity and determination to take advantage of this data. Yet these data are not yet organized systematically the way they would be when coming from the standpoint of integrated financial accounts. Moreover, the US data have not yet been mounted on a comprehensive geographic information system archive such as what is underway in Thailand, China, and Brazil. I am convinced that developing these models and data would bear much fruit in allowing us to think about local communities, how the larger US economy is put together, and implications for policy.

What remains is to find the right balance as we move forward in this world filled with these and other new possibilities. One dimension emphasized here is to be eclectic about the various possible measurement techniques, adopting what is needed to measure what is key and to elicit accurate and enduring responses. Another dimension is the use of the research in policy, either taking advantage of quasi-natural experiments or doing counterfactual experiments within the estimated models themselves. This experimental view can bring researchers together and arbitrages across subfields, unifying micro and macro with theory and data in the context of applied general equilibrium analysis.

■ *Research support from the Eunice Kennedy Shriver National Institute of Child Health and Human Development (NICHD) (grant number R01 HD027638), the National Science Foundation (grant number 0649302), the John Templeton Foundation (grant number 12470), the Consortium on Financial Systems and Poverty at the University of Chicago (funded by Bill & Melinda Gates Foundation under grant number 51935), the Centre for Economic Policy Research (CEPR) and the Department for International Development (DFID) under grant MRG002_1255, and the Ford Foundation are gratefully acknowledged.*

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