Wither the Economics of Agricultural Development?

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ABSTRACT

In spite of a healthy demand for a renaissance in economic policy for agricultural development, the academic supply response is found wanting. The infusion of public economics into the economics of agricultural development, which thrived during the 1970s and 1980s, has stagnated due to the lack of foundations in transaction costs, dynamics, and the co-evolution of specialization and governance. Many of the policy ideas found in the World Bank's WDR 2008, for example, reflect a post-modern tendency to seek and destroy market failures with new mandates and subsidies for farmer cooperatives, microfinance, crop insurance, and land reform. The new development microeconomics favors form over substance and overemphasizes multiple equilibria, trap theories, new market failures, and the new case for social insurance. Empirical research has likewise suffered from the quest for clever instruments and methods instead of informative results that estimate parameters of established theories, distinguish between competing theories, or challenge theory to explain empirical patterns. These latest fads and fancies have distracted economists from the quest for fundamental explanations of development patterns, especially the nature and causes of specialization as an engine of growth. The stage is set for young dynamic scholars to develop new tools of analysis to explain empirical patterns in behavior and organization in developing agriculture and to build the foundations of a public microeconomics of development.

INTRODUCTION

Demand for a renaissance

The economics of agricultural development was an exciting field and an important part of development economics in the 1960s, '70s, and '80s. Johnston and Mellor's (1961) classic established the critical role that agriculture plays in economic development and, along with Jorgenson (1961), served as a corrective to the widespread belief that agriculture should be squeezed to finance modern industrial growth. Agricultural development flourished as a discipline in the mid and late 1970s, albeit largely outside of academia. The Agricultural Development Council, under the inspirational leadership of Arthur Mosher, Clifton Wharton, and Vernon Ruttan, helped to create the economics of agricultural development as a distinct field, based in large part on the seeds planted by Theodore Schultz, and by attracting

such stalwarts as Hans Binswanger and Robert Evenson. Agricultural development also became an important part of the World Bank's Development Research Department and was central in the mission of the International Food Policy Research Institute, founded in the mid-1970s.

Theoretical and empirical research during the 1970s focused on behavior and organization at the farm level. Did feudal patterns of landownership, risk aversion, and credit constraints stagnate agriculture? Were these constraints the reason that the Green Revolution did not deliver on its initial expectations? Many of the innovations from this period were adaptations of mainstream economics to development issues: methods of modeling and measuring risk and risk aversion, measuring the contribution to productivity growth by new agricultural technology, applying the new household economics to market conditions in developing countries, and extending and applying the new institutional economics to agricultural contracts.

As the 1980s brought new enthusiasm for trade-led growth, agricultural development thinking became more outward-oriented and infused with public economics. The new paradigm summarized the nature of agricultural policies with their effective protection rates, calculated their consequences with partial and general equilibrium models, and analyzed their causes with political economy models. This new paradigm culminated with a mammoth study at the World Bank in the late 1980s, which was published in five volumes (Krueger et al. 1991-92). The idea that agricultural development might be efficiently supported through subsidies and protectionism slowly withered away. For reasons of both sociology of knowledge and political economy, however, the liberalization of agriculture lagged behind that of manufacturing. Rising per capita incomes, increased capital-labor ratios, and agriculture's increasing concentration and commercialization all contributed to the resiliency of agricultural protection.

Even before the publication of the Krueger-Schiff-Valdez volumes, however, the economics of agricultural development began to slowly wither away. This resulted partly from the long secular downtrend in relative agricultural prices, leading donors to believe that agricultural development was no longer a high priority. Within academia, agricultural development has long been somewhat of an orphan. University economics departments, with the notable exception of the University of Chicago, relegated agricultural development to departments of agricultural economics.[1] But agricultural economics was dominated by production economics, farm management, marketing, quantitative methods, and applied econometrics, with development relegated to the fringe. Moreover, departments of agricultural economics were focused primarily on problems of rural development and agriculture within the U.S., not the developing world.[2] Institutional support for analytical studies of agricultural development also declined, with the demise of the Agricultural Development Council and diminished interest from the World Bank.[3] As

[1] Agricultural development at the University of Chicago resulted from the historical accident of Ted Schulz resigning in protest from the University of Iowa and moving to Chicago, where he was joined by D. Gale Johnson.

[2] Exceptions included the Food Research Institute at Stanford and the agricultural economics departments at Cornell University and Michigan State University.

[3] In its search for political correctness, the World Bank added non-economists in greater numbers, leading Bank watchers such as Larry Summers to observe that the Bank had lost its analytical edge.

the Washington Consensus faded in the Clinton era, so did the enthusiasm for applying public economics to agricultural policy.

Recently several forces have converged to create a demand for a renaissance in the economics of agricultural development. First is the food-feed-fuel nexus that created the 2008 spike in food prices and renewed calls for programs in food security. These are exacerbated by new input scarcities, especially water and fertilizer, and new or threatened regulations amid protests that agriculture is a "user and abuser" of the environment. There is also renewed awareness of agriculture's role in sustained poverty reduction, due to the well-known linkages of lower food costs, increased employment, stimulation of non-agricultural investment, and the more recently studied participation effect of agricultural growth (Christiaenen et al. 2006). In addition, observers proclaim that there is a new agriculture, evidenced in particular by the supermarket revolution. Finally we have new economic theories and tools, including the new development microeconomics, the new institutional economics, and the new labor econometrics.

In the next section, we assess how development economics has responded to the new challenges and find that much remains to be done. In section 3, we summarize the lessons from said assessment. Section 4 contains a discussion of particularly promising research.

HOW HAS ECONOMICS RESPONDED TO THE CHALLENGE? WDR 2008 AND BEYOND

Donor agencies, universities, and other training and research institutions are now asking whether, how, and to what extent the field of agricultural development is ripe for renewed investment. What are the questions and what fields of knowledge are needed to address them? A natural starting point for this investigation is the World Development Report (WDR) 2008, the first such World Bank report on food and agriculture in 25 years. The World Bank is the dominant firm in development economics, and its annual WDRs provide considerable insight into the evolution of development economics (Yusuf 2009).

The organizing framework of WDR 2008 is a taxonomy of economies – agricultural-based, urbanized, and transition. The report seeks to derive policy recommendations for each type by an empirical investigation of the nature of growth, without regard to the nature and consequences of specific policy instruments in the sample countries.

As exemplified by the WDR 2008, modern policy prescription in development economics is still heavily infused with planning. Ad hoc policy objectives (improving market access, establishing efficient value chains, enhancing smallholder competitiveness, and increasing incomes of the rural poor) are substituted for the fundamental objectives of equity and efficiency, and the "visible hand of the state" is advocated to foment "a productivity revolution in smallholder agriculture," seek and destroy market failures, deliver multiplier effects, promote land reform, and support cooperatives for small farmers. This new paradigm is reminiscent of Paul Krugman's (2006) dictum: you can flush bad ideas, but – like New York cockroaches – they keep coming back. We might add that the cockroaches return in mutated form and are not recognized as the resurrected pests that they are. By reviewing a few of the current fallacies below, I hope to illuminate how existing knowledge can be used for a more balanced economics of agricultural development. This will in turn help to reveal promising new research directions.

The persistent small farm bias

In the 1960s, presumed dualism between peasant and modern-sector labor markets was used to explain the association between

development and the declining relative size of the agricultural labor force (Fei and Ranis 1961). Dualism was used in turn to support protectionism and import-substitution as a development strategy. When development economists belatedly recognized Jorgenson's (1961) theory of two-sector development without dualism and Corden's (1971) study of the stagnating effects of protectionism, dualistic economic development and import substitution were abandoned. The growth with equity decade (1970s) was born, and with it a renewed interest in agricultural development.

Having not learned from experience that dualism embraces misplaced exogeneity, economists were doomed to repeat the fallacy. This time, dualism was said to characterize small, subsistence farms versus large, commercial ones. Small farms allegedly paid only subsistence wages, whereas large, commercial farms paid the institutionally-set wages of the modern sector. The lower opportunity cost of labor allegedly led to more workers per hectare employed on small farms, thus explaining the notorious inverse-relation between yield per hectare and farm size. According to this theory, land-to-the-tiller reform would increase productivity as well as improve equity.

With the advent of the New Institutional Economics (started in the 1970s but only recognized in the '80s and '90s), economists realized that agricultural dualism was ad hoc. But instead of abandoning dualism and the small farm bias, economists found new ways to justify their old conclusions. Large farms are now said to be at a transaction cost disadvantage because of the need to supervise hired labor. Again the "evidence" that small farms often yield more per hectare was offered as proof that this transaction cost disadvantage was pervasive and important and that land reform could potentially improve both efficiency and equity.

Land reform was similarly aided and abetted by Joseph Stiglitz, World Bank Chief Economist (1997-2000), who argued that 50-50 share tenancy is tantamount to a 50 percent labor tax (e.g., Stiglitz 1993), ignoring thereby the landlord's ability to at least partially control tenant effort through monitoring and penalization (Cheung 1969; Roumasset 1995). Stiglitz's view implies that the marginal product of labor on small, tenant farms is too high, in direct contradiction of the dualism and transaction-cost-disadvantage explanations of the inverse relationship. This contradiction has apparently gone unnoticed.

Another component of the small farm bias comes from the alleged bimodal pattern of landownership used to buttress the argument for agricultural dualism and the inefficiency of agriculture. Considerable ambiguity remains, however, regarding exactly what distribution is supposed to be bimodal. In resurrecting the bimodal distribution argument, the WDR 2008 illustrates the frequency distributions of farm size for Bangladesh, India, and Brazil, each for two years. The distributions for Brazil were bimodal in 1966 and unimodal in 1977. India's were trimodal in 1970 and unimodal in 1995. Brazil's distributions were trimodal in 1970 and bimodal in 1996, albeit approximately unimodal. Even in these non-random selections, bimodality is hardly pervasive. As it turns out, the intended meaning is simply that "most of the land is in large farms, while most farms are small" (ch. 3, footnote 48). This has necessary connection to bimodality, however, and is satisfied, for example, by the unimodal Pareto distribution. More importantly, the presumption that equality in size implies inefficiency is misleading.

The assertion that inequality of land ownership is inefficient has a long history in development economics (Johnston and Kilby 1975; Lipton 1979; Stiglitz 1993). But these arguments overlook the fundamental reasons for the observed diversity in farm size. For example, production in the U.S. has become even more

concentrated on large farms even as the number of small farms has increased for lifestyle and tax reasons. Just as the defunct structure-conduct-performance paradigm ignored the reasons for diverse market structures, so the farm size debate ignores the reasons for the tremendous diversity in observed farm sizes. Indeed, unequal land distributions reflect differences in land characteristics, diverse farmer preferences, tax laws, prospects for internal and external economies of scale, as well as differences in wealth and investment opportunities. Top-down land reform programs overlook these fundamental reasons for diversity.

Perhaps the most revealing inconsistency of the small-farm bias is the treatment of the supermarket revolution (Reardon and Timmer 2007), whereby food markets are vertically coordinated from farmers to retailers, largely by means of specialized wholesalers, dedicated to specific supermarket chains. This places small farms at a transaction cost advantage, further augmenting the disadvantage they face in credit markets. But while the transaction cost advantage of small farms is used to advocate policies that would further shrink average farm size, the transaction cost advantage of large farms is not used as an argument for considering market-smart consolidation. Apparently proponents of these views believe that any advantage of small farms is natural, but any advantage of large farms results from a "policy bias." In effect, the alleged political economy of Latin America (e.g., de Janvry 1981; IFAD 2001) has been presumed to be accurate for the world as a whole. The apparent policy implication is to subsidize small farm agriculture through marketing and credit cooperatives.

Again such policy implications are premature absent a fundamental explanation of behavior and organization in developing agriculture. Despite an abundant literature,[4] the nature of specialization inherent in the new agriculture has not, to my knowledge, been adequately explained. In the old agriculture, inevitable differences in products across space, time, and form were dealt with by wholesalers and others in the marketing chain, who sorted and processed products after production. A farmer would not know the destination of his product in advance and could not tailor it to the particular tastes of consumers in that market. In the new agriculture, the varieties, quality, and timing of farm products can be pre-arranged through a contracting process with particular farmers. In this way, vertical coordination begets horizontal specialization and produces external economies that improve total factor productivity (Lucas 2009).

Even leaving aside the transaction cost advantages of large farms regarding credit, marketing, and self-insurance, the alleged transaction cost disadvantages of hiring labor are countered by their advantages in facilitating specialization. As shown by the labor market pyramid (Roumasset and Lee 2007), more farm labor facilitates horizontal specialization by task within the farm and vertical specialization whereby worker teams or firms (especially for land preparation, planting, weeding, and harvesting) provide intermediate inputs. Because of these internal and external economies of specialization through which hired labor can increase labor productivity, it may well be that promoting small farm agriculture through regulations, confiscations, and subsidies will exacerbate pre-existing distortions that have rendered many farms smaller than efficiency would dictate.

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[4] Reardon (2008) notes that WDR 2008 cites his work with various coauthors no fewer than 25 times.

A related omission is the extent to which misguided land reform policies have impeded the natural evolution of economic organization, for example economies of vertical integration and horizontal specialization of food markets. Surely some policies result from rent-seeking large farmers, but the inefficient breakup of large farms and the unseen prevention of consolidation are not typically considered.

The transaction cost wedge allegedly facing large farms is the centerpiece of both the case for land reform ("small farm entry") and small-farm "competitiveness" subsidies for fertilizer, credit, insurance, and producer cooperatives. The empirical support for the need for this government intervention is that the existing equilibrium is dualistic. Dualism, a resilient cockroach from the 1960s and '70s, rests in turn on the notorious claim that the inverse relationship between farm size and yield per hectare is evidence of labor market duality and therefore inefficiency. The inverse-relationship-implies-inefficiency view has had a long history in agricultural development circles (e.g., Sen 1962, 1964; Berry and Cline 1979; de Janvry 1981; Stiglitz 1993; IFAD 2001). What this literature has in common is the adverse selection inefficiency arguments of the phenomenon to the exclusion of more fundamental efficiency explanations.

There are at least three efficiency explanations of the inverse relationship between farm size and land productivity. The first is that landlords allocate smaller parcels of better land to farm families to equalize the marginal product of labor across their holdings and thereby maximize total rents (Roumasset 1976; Roumasset and James 1979). The second is the transaction cost wedge whereby larger farms have a greater incidence of hired labor and therefore hire less labor per hectare, (e.g., Roumasset and Smith 1981; Sah 1986; Otsuka 2007). The third is endogenous occupation choice given heterogenous farm skills (Assuncao and Ghatak 2003). Thus on logical grounds alone, one cannot jump to the conclusion that the inverse relationship is prima facie evidence of inefficiency or that corrective intervention is required.

The most reliable empirical test of the three fundamental explanations of the inverse relationship found support for the land quality hypothesis, but not for the other two (Assuncao and Braido 2007). This does not imply that the second and third explanations are invalid. Rather the measurement and specification problems, especially regarding transaction costs, may have rendered those relationships insignificant.

Despite the implicit assumption to the contrary in much of the large-is-inefficient literature, small farms in low-wage Asian economies often do hire labor at the margin (e.g., Roumasset and Smith 1981; Hayami and Kikuchi 1982).[5] And labor turnover costs (Stiglitz 1974a), including recruiting, training, and negotiating costs, are subject to economies of scale. This turns the "transaction cost disadvantage of large farms" argument on its head. Small farms that are indeed self-sufficient regarding labor may avoid using the labor market because they cannot capture the economies of scale, including those of specialization, that larger farms afford. Also, as the relative cost of farm equipment declines relative to labor, the scale economies of farm mechanization drive up the optimal farm size. None of this means that large farms are necessarily more efficient than small farms but that the quest to determine

[5] For example, the rapid emergence of the labor market for rice production in Laguna, Philippines during the 1970s was a small farm phenomenon

whether large or small farms are more efficient is futile. There are diverse reasons for the diversity of farm size, and a land reform program that mandates farm consolidation could be equally misguided as one that mandates breaking up large farms into smaller units.

This does not mean that government should promote reverse land reform to encourage consolidation, but that biasing land policy to promote small farms may increase inefficiency and stagnation.

Other examples of non-fundamentalism

The small farm bias discussed above illustrates a pervasive tendency to diagnose market failure and prescribe government policy correctives without a fundamental explanation of the observations at hand. In order to assess the potential consequences of proposed policy reforms, we first need fundamental explanations of behavior and organization in developing agriculture.[6] Other examples of non-fundamentalism abound, beyond the small farm bias.

There is a pervasive tendency in the new development microeconomics to presume that poverty is the result of some kind of trap. Poor farmers are either too risk-averse or credit constrained to undertake investments with a high expected value or they are driven to desperation and rationally undertake investments with a low profitability. Moreover, potential lenders are unable to accurately distinguish careful from desperate borrowers and tend to withhold credit from both groups thereby perpetuating the "bad equilibrium" (see especially Banerjee 2004). In the interventionist perspective of WDR 2008, the perversely excessive risk-aversion and credit constraints in turn are assumed to be the result of "market failures" in credit and insurance markets. Comparative institutional analysis, in contrast, is based on the premise that first-best efficiency is in general unattainable, whether by perfect markets or perfect governance.

Some of the proposals in WDR 2008 are recalcitrant remnants of the interventionist '70s. The report claims that land reform is needed to address inequalities in asset distribution and lauds postwar Japan, Taiwan, and the Republic of Korea as examples where land reform can improve productivity as well. This tends to denigrate evidence that unless done quickly and cleanly, land reform will tend to lower productivity (e.g., Fabella 1999), and to ignore the unique political economy features of the three examples that allowed land reform to be quick and clean, even if confiscatory.[7] The Republic of Ireland provides another example of pre-independence land reform being imposed by the United Kingdom for geopolitical reasons. It appears that successful land reform has been effected by political economy forces, not from the imaginings of Robin Hood economists.

In the 1970s, it was commonly argued that small farmers are risk averse and therefore underinvest in promising but risky ventures. This argument has been largely discredited on both logical and empirical grounds but has been resurrected in a more sophisticated form. Risk aversion is now viewed as being sourced in the inability to smooth consumption over time,

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[6] Fundamental explanations are sometimes known as invisible hand explanations, after the Adam Smith prototype of assumptions about atomistic players leading to consequences quite distant from those assumptions (see especially Nozick 1974).

[7] The report cites Appu (1996), Deininger (1999), and Lutz et al. (1996) for cases where land reform lowered productivity but apparently disregarded these as examples of how the process, not the concept, of land reform needs to be improved.

and farmers are said to self-insure by coping mechanisms (e.g., taking children out of school in bad years) and by investing in more stable, albeit low return production techniques and capital equipment. For example, poor farmers in semi-arid India (ICRISAT data) are said to be unable to invest in irrigation pumps that would return 22 percent on their investments (Fafchamps and Pender 1997; WDR footnote 62).

The need for self-insurance is attributed to imperfections in the credit and insurance markets. This leads immediately to the consideration of institutional innovations in insurance, credit, and land markets. The conclusion in WDR 2008, chapter 6, is:

> "Land markets are still incomplete and inefficient. Financial markets are still laden with asymmetries of access and information. Insurance against risk is available to only a few individuals and communities" (p. 157); and "access to credit and insurance are thus closely tied and must be jointly improved to enhance access to credit" (emphasis added).

Consideration of such market-development innovations is called a "new role of the state," even though market facilitation can be traced back to Adam Smith.[8] Unfortunately, the report does not suggest a framework for the evaluation of such innovations. Presumably each would be evaluated according to benefit-cost criteria. But the benefit-cost paradigm is based on how a particular project would impact a competitive equilibrium distorted by exogenously specified taxes, tariffs, and missing markets (Squire 1989). It has not been extended to Dixit's (1996) second-best, i.e., optimization and equilibrium theory with information and other transaction costs.

The failure to consider that an equilibrium may already be second-best[9] leads much of the development profession to conclude that agents like the non-pump-buying Indian farmers are trapped in poverty (e.g., Banerjee 2004). Lacking perfect insurance and credit markets, the farmers must self insure against their vulnerability, accept credit constraints, and forego the profitable pumps. On the other hand, their desperate circumstances may cause them to "plunge" into actions that decrease their expected incomes even as plunging lowers the risk of falling below some threshold.

This view presumes, without compelling theory or evidence, that poor people are necessarily trapped and that the trap derives from market failure, especially in credit and insurance markets. However, many poor people are not inescapably trapped but temporarily poor, either because of income shocks from which they will recover or because they are in the process of acquiring skills and assets to move themselves out of poverty. Mary Hollensteiner's (1972) description of Tondo residents in Manila is especially revealing in this regard. And those who can be meaningfully characterized as trapped are likely to be in a Schultzian (1964) trap of inappropriate technology and inadequate

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[8] See also the explanations of "pro-market intervention" (Roumasset and Barr 1992) and "market-friendly intervention" (World Bank 1993).

[9] This possibility has been dismissed out-of-hand as the Super Coase Theorem (Dixit and Olson 2000). Most theorists reason that inasmuch as a meaningful Coase Theorem has never been proved for a world with zero transaction costs, one will not be proved for a world with positive transaction costs. This view overlooks the Cheung-Coase Theorem, which has been proved (e.g., Roumasset 1979b; Johansson and Roumasset 2002), showing that competitive contracts are a perfect substitute for competitive markets. Ironically, Yang's (2003) tour de force takes for granted that the Super Coase Theorem is valid and proceeds to investigate its implications for the evolution of specialization.

human capital. Small farmers in such traps are in need of technology appropriate to their agro-climatic zone and household shadow prices, not unsustainable credit and insurance subsidies. The tendency to presume that traps must be modeled as "bad equilibria" associated with non-convexities derives from the predispositions of economic modernism, not from empirical realities.

Looking to new innovations in insurance markets also presumes that actuarially fair insurance, even if it could somehow be provided without administrative costs, would provide a substantial boost to economic welfare and poverty alleviation. This is not necessarily true. While the report makes much of idiosyncratic risk, idiosyncratic risk aversion is entirely ignored.

Risk aversion regarding contemporary choices is logically sourced in transaction-cost impediments to consumption smoothing and aversion to fluctuations in lifetime income (Roumasset, 1979a). But Rabin (2000; see also Rabin and Thaler 2001) has provided a strong case that uniform convexity toward lifetime income implies incredible hypothetical behavior for choices involving large gambles. In addition, one can demonstrate through numerical exercises that modest lifetime risk aversion tends to be swamped by transaction costs in the creation of contemporaneous risk aversion. But transaction costs are inherently idiosyncratic and not amenable to risk-spreading institutions. Indeed, a second-best efficient competitive equilibrium requires that farm-households maximize welfare with respect to their own individual shadow prices, and subsidized insurance would distort those incentives (Foley 1970; Roumasset 1979a).

Consider the similar case for "social insurance," Chetty and Looney (2006) show that the surprisingly low consumption fluctuations in developing countries do not imply small welfare gains from social insurance as had been previously suggested. If farmers are highly risk averse, they may accept high costs of smoothing (e.g., selling durable goods at a deep discount and taking children out of school), and therefore welfare gains of social insurance may be high after all. By providing low-income households with subsidized social insurance, they will not have to make such extreme and costly sacrifices. The problem with this theory is its neglect of transaction costs and misplaced exogeneity regarding the causes of risk aversion. In a more fundamental theory, however, smoothing is sourced in transaction costs and the substitutability of intertemporal consumption. Indeed the high-cost-of-smoothing result can be derived in a world of perfect certainty. The contrivance of exogenous risk aversion is not required. In this world, failure of the private insurance market does not provide a case for social insurance inasmuch as there is no need for insurance to begin with. Instead attention is properly focused on the possibility of lowering transaction costs through better roads, communication facilities, and legal infrastructure.

Understanding the source of risk aversion is just one illustration of the central role that transaction costs must play in policy analysis for agricultural development. But while the transaction-cost-wedge model (commonly attributed to de Janvry et al. 1991) is recognized as an explanation for price insensitivity of peasant farmers in chapter 3 (WDR 2008), its wider potential for understanding behavior and organization in developing agriculture is not exploited. Most fundamentally, the wedge model reveals that searching and correcting "market failure" is as impotent a paradigm for economic development as the single-minded pursuit of private property and free markets. Since both perfect government and perfect private cooperation are capable of achieving first-best efficiency in the abstract, declaring the superiority of one or the other in a world of

positive information and enforcement costs is futile (Coase 1988).

Similar limitations can be found in popular development thinking regarding marketing parastatals, "market-smart subsidies," and microfinance. The paradigm involves showing why a market solution is not first best, or not even second-best in the sense of Greenwald and Stiglitz (1990), and proceeding to suggest market correctives without a model to evaluate the consequences of the proposed reforms. The required model for such assessments should allow for shadow price differences over space, time, and agents. Lacking such a model, any policy recommendations should be carefully qualified.

None of the above criticisms of non-fundamentalism should be taken to mean that land and labor markets in developing country agriculture succeed in allocating resources according to first-best efficiency. In the face of transportation, storage and transaction costs, the economic equilibrium is characterized by different shadow prices over time, space, households, and firms. Finding systematic patterns relating one shadow price to one characteristic of the farm-household is elusive, however, partly because second-best explanations are likely to involve several interdependent shadow prices, and offsetting effects abound. Even if a second-best explanation were to be found, this would not imply the existence of inefficiency. Any constrained Pareto optimal allocation in the face of transaction costs will also be characterized by divergent and diverse shadow prices. The fallacy of Stiglitz (1993) suggests that in an imperfect information world, there is an expanded role of government from that of Pigouvian interventionism. By assuming that the state has a monopoly on governance and multilateral contracting, Stiglitz implicitly holds up government coercion as a panacea.[10] This is the second-best Nirvana fallacy, inasmuch as it imbues the government with a monopoly over governance and multilateral contracting.

WDR 2008 goes a step beyond Stiglitz in concluding that not only has the state a new role, but that the role is to expand the small farm sector. The correct conclusion is that there may be a new role of the state associated with transaction costs, but one cannot make any definite conclusions without explicitly doing the requisite comparative institutional analysis.[11]

While public economics has become entrenched in agricultural policy analysis for developed countries (due especially to the pioneering efforts of the late Bruce Gardner), the WDR 2008 bypasses those principles. Instead the WDR team declared its policy objectives to be improving market access, establishing efficient value chains, enhancing smallholder competitiveness, and increasing incomes of the rural poor. In doing so, it skirted the tasks of articulating policy instruments and evaluating country policies in light of the primary policy objectives, efficiency, and equity. Inasmuch as most policy instruments are dominated by other instruments with respect to income distribution (Musgrave 1969; Kaplow and Shavell 1994, 2002) and because "relative" Pareto improvements often render the efficiency-equity tradeoff unnecessary (Wicksell 1896), most

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[10] See also Chambers and Quiggin (2007) for pertinent considerations, although they view risk aversion as exogenous and inherently spreadable.

[11] Indeed when the wedge model was introduced in the Philippine context to explain the rapid emergence of the farm labor market and the inverse relationship between farm size and productivity (Roumasset and Smith 1981; Sah 1986), the authors did not suggest that either phenomenon was evidence of inefficiency.

instruments can be evaluated with respect to efficiency.[12] For example, measures to enhance smallholder competitiveness (e.g., by subsidizing producer cooperatives or discriminating against established commercial enterprises) should be accountable to the principles of benefit-cost analysis and fundamental rights to equality under the law.

WDR 2008 also describes several pathways out of poverty in contrast, e.g., to "those who stuck to the more traditional farming systems" (p. 73). This is apparently not meant to imply a behavioral failure because, a few pages later, farmers are portrayed as rational. Rather the reader is left to conclude that those farmers were stuck by "the lack of access to insurance and credit policies" and that "protecting rural households against uninsured risks is an area in need of greater policy attention."

Using the wedge model[13] to portray transaction-cost equilibria offers a way to effect genuine comparative institutional analysis as recommended by Coase (1988). Since the wedge model takes the unit transaction costs as exogenous, the model is structurally equivalent to that of competitive equilibrium with transaction costs, which has been successfully modeled (e.g., Foley 1970). One can thereby compare the welfare associated with an equilibrium before and after a government project or policy reform, especially one designed to decrease the unit transaction cost wedge, e.g., improving transportation, communication, and legal infrastructures.

The wedge model can be similarly used to evaluate the costs and benefits of crop insurance. There is a strong prima facie case against subsidizing crop insurance for developing countries because of the high administrative load (Roumasset 1978; Wright and Hewitt 1994)[14] and because of idiosyncratic risk aversion, but the wedge model can make this explicit.

WDR 2008 takes a somewhat schizophrenic posture toward a possible government role in price stabilization. On the one hand, chapter 5 acknowledges the reality that parastatals "often destabilize prices through unpredictable market interventions, border closures, and poorly timed imports." Nonetheless the same chapter concludes that "lessons over several decades suggest that … food price interventions should be part of a long-run strategy… to raise productivity of food staples, improve the efficiency of markets… and minimize the impact of price shocks..." This conclusion ignores theoretical and numerical results showing that stabilization schemes such as buffer stocks and price controls are extremely costly and eventually destabilizing. Parastatals also displace private transportation and storage infrastructure and substitute costly domestic storage for efficient transportation (Williams and Wright 1991; Roumasset 2000).

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[12] The consumer price of staple foods is a possible exception (Roumasset and Setboonsarng 1988).

[13] For a simple exposition of the wedge model and its application to output, labor, and credit markets, see Roumasset 1981. For further development with respect to price responsiveness, see Fafchamps et al. (1998). To date the comparative statics analysis in such models has been limited to the case wherein changes in exogenous prices do not change the farm households category (importing, exporting, or self-sufficient). Market elasticities based on this assumption are inevitably incorrect because they presume that the frequency of households that start and end in each category adds up to 100%. Moreover, category changes can be indirect via input markets. For example, an output price increase can lower the shadow price of credit, and shift the output supply curve to the right, thereby putting the farm household into the "exporting" category.

[14] Wright and Hewitt 1994 note that when the administrative load in the U.S. is added to indemnities, the loss ratio increases from 2 to 2.5. The number is substantially higher in developing countries, given the large number of small farms.

WDR 2008 also follows the common tendency to declare failure in rural credit markets and to assert that innovations in microfinance and farmer cooperatives are solutions, without providing any theoretical construct by which such programs can be evaluated. Again the wedge model provides a start. Each farm household can be characterized by a shadow prices schedule of borrowing and lending/investing, including the returns from selling durable assets. To complete the analysis, one would need to invoke agency theory to model the supply side of the credit market. Unlike ordinary commodities, it is not suitable to assume that lenders provide a specific quantity loaned to undifferentiated borrowers at each price of loanable funds. Rather each lender associates each potential borrower with an offer curve of interest rates and quantities. Equilibrium is then characterized by a matching of the amount borrowed by each agent from each lender such that adjusting the match cannot produce a mutually beneficial improvement.[15]

Applying the wedge model to rural credit also epitomizes the role of government as facilitator not coercer of last resort. In general shadow prices of credit differ across lenders and across borrowers. In the second-best equilibrium, there are many shadow prices of borrowers greater than shadow prices of lenders but they are precluded from trading by transaction costs. The role of government as facilitator involves asking whether government has a comparative advantage in reducing transaction costs through information provision, administrative assistance, communication facilities, or legal infrastructure such that the benefits of mutually-beneficial loans facilitated exceed the costs of facilitation.

While much of WDR 2008 pays homage to micro-foundations, if only to declare market failure, the organizing framework of the report strangely avoids them and substitutes a kind of cluster analysis to sort countries into types. The apparent objective is to infer the "natural" pattern of development from the panel data and then to somehow formulate development strategies for each type -- all without articulating the appropriate policy instruments of interest or the model to be used for evaluation.

A more appropriate objective is to prescribe policies that can first get countries onto the efficient path and then to facilitate movement along that path. Instead of plotting the panel data and drawing circles around three "clusters," what is needed is to solve for the efficiency frontier, not just regarding the two variables shown on the graph, but with regard to several variables, including preconditions such as the initial physiological population density, resource abundance, and endowments of produced and human capital. Now one can observe which countries are particularly far off their efficiency paths and proceed to look for the causes thereof and the indicated policy reforms. One can also articulate the kinds of government action that can facilitate development along the efficiency path.

WDR 2008 laudably promotes the importance of high-value agriculture and the linkages between agriculture and the environment as well as the futility of promoting agriculture through protectionism. But while WDR 1982 was revolutionary in its call for liberalization, the 2008 report returns to the 1970s' interventionism. The method is to diagnose a market failure and then design a government intervention on the blackboard, heedless of the warnings of Coase (1988) and Demsetz (1969) about the nirvana fallacy and the need for comparative institutional analysis.

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[15] See Roumasset (1986) for preliminaries.

The Handbook of Economic Development, V. 4: Did Economists Save the Day?

One wonders whether the coolness of the WDR 2008 team to mainstream economic policy analysis has anything to do with the team's being predominantly composed of agricultural economists.[16] If this were the case, one might expect the bias to be counterbalanced by the dedication of the Handbook of Economic Development, V. 4 (Schultz and Strauss 2007) to agricultural development, inasmuch as the Handbook authors are mostly economists. Instead of inquiring into new methods and challenges regarding agricultural development policy analysis, however, the volume was predominantly devoted to empirics and modern methods in applied econometrics.

The editors, T. Paul Schultz and John Strauss, are known for combining economic development and labor economics and to understandably pay homage to agricultural development giant Ted Schultz, human capital guru Gary Becker, and their joint fatherhood of what was once called the new household economics, wherein the relationship of farm production and household consumption and production is recognized. The editors thus seek to define the frontier of knowledge of that part of development economics that is particularly edifying for agricultural and rural development, especially as it pertains to rural poverty.

Along with the notable improvement in access to and collection of data in the form of household surveys and censuses, a number of advances in econometric methods have evolved, especially for the analysis of panel data. Collectively, these are popularly known as labor econometrics. Thirteen out of 16 chapters of the Handbook highlight modern or improved methods of econometrics – cross effects, program effects, randomized evaluation, selection bias, matching methods, propensity score, double difference estimators, instrumental variables, control functions method, regression discontinuity, grouped errors, stratification, among others. Only three chapters (those of Munshi; Banerjee, Iyer, and Somanathan; and Fafchamps and Quisumbing) displayed theoretical underpinnings in the analysis of development issues and even these stinted policy analysis. In the end, however, the volume reflects the trend of too little theory in development economics (Mookherjee 2005) and that the pendulum between theory and empirics has swung too far toward "natural experiments," clever use of instrumental variables, and other techniques of modern econometrics (Kanbur 2005). If there is too little theory in development economics, it seems likely that the lacuna is an even greater problem in the economics of agricultural development, given its orphan status.

The problem is not only that there is too little theory, but that the theory that exists is somewhat one-sided. The new development microeconomics (e.g., Bardhan and Udry 1999, 2000) emphasizes market imperfections and fragmentation, poverty and underdevelopment traps (which rest on multiple equilibria and market imperfections), and market and institutional failures. They are grist for the mill of market intervention and economic planning without full acknowledgment of transaction costs and private governance.

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[16] Beginning with the "Rausser revolution" at UC Berkeley in the mid- to late 1970s (under chair, Gordon Rausser), agricultural economics has been increasingly integrated with mainstream economics over the last three decades. However, the field of agricultural development has remarkably lagged behind this movement and continues to be dominated by those who predate the revolution and their students.

LESSONS AND NEW DIRECTIONS

Much has changed from the heyday of the economics of agricultural development. The previous focus on production has expanded to the agricultural system and rural development. Linkages with the environment and poverty alleviation are increasingly appreciated. And the new development microeconomics promises to infuse the field with both theoretical and empirical innovations.[17] Yet much has stayed the same; policy prescription is seldom based on fundamental explanatory models.

As each development strategy failed (or lost its appeal) over the decades, development economists have returned to the blackboard to design a new approach that captures their collective fancy. But often, new policies were added without dismantling the old, leading to the "patch-on, patch-up" policy reform. Economic development policy in practice has thus been band-aid economics.[18] Moreover, by failing to learn the lessons of past development policy mistakes, we may be doomed to repeat them. Just as the Pigouvian interventionism of the 1970s was guilty of a nirvana fallacy (Demsetz 1969), post-modern interventionism (e.g., as championed by Greenwald and Stiglitz 1990; Stiglitz 1993; Stiglitz 2003; and Banerjee 2004) similarly portrayed the market equilibrium as a straw man without voluntary mechanisms of governance and multilateral cooperation and government as a benevolent and omnipotent (though not entirely omniscient) alternative. The tendency to socially engineer reforms instead of facilitating cooperation persists. Just as the old structure-conduct-performance paradigm was replaced by contestable market theory and other innovations, the prospects for improved empirical work on developing agriculture await the development of an appropriate structural model wherein farm organization, specialization between family and hired labor, and choice of contracts across tasks and economic environment are understood as parts of an endogenous whole.

In contrast, the publication of the Krueger-Schiff-Valdez 1991-92 volumes was a watermark achievement. It marked the fruition of a powerful paradigm in agricultural development policy analysis based on a description of the nature, consequences, and causes of economic policies. The consequences, by which alternative policies could be compared, rested in turn on the foundations of models of the distorted competitive equilibrium. While exceedingly useful, these models abstracted from spatial and temporal aspects such as transportation and storage, transaction costs, non-convexities and other barriers to entry.

Both the WDR 2008 and the Handbook of Development Economics, 4 bypass the public economics of agricultural policy. Both acknowledge the importance of transaction costs and dynamics but neither advances a model that can be used to evaluate the consequences of a particular program or policy reform in the face of transaction costs or, more ambitiously, transaction costs in a dynamic framework. Neither acknowledges the advances in understanding the evolution of institutions to facilitate specialization (e.g., Greif 2005; Yang 2003). And neither accesses the advances in sustainable development that have included natural capital and environmental degradation in the model.

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[17] For more on the fads and fancies of economic development thinking, see Roumasset (2007). For a related perspective on the history of development thinking at the World Bank, see Yusuf (2008).

[18] The terms "Band-Aid economics" and "patch-on, patch-up" policy reform were coined by development-policy pioneer, John Power.

The search-and-destroy method of the new development microeconomics as exemplified in WDR 2008 identifies transaction cost and multiple equilibrium problems that prevent the economy from attaining a full (first-best) Pareto optimum. Since the competitive equilibrium in a transaction-cost economy cannot achieve second-best optimality except by coincidence (Greenwald and Stiglitz 1990), economists feel empowered to suggest a panoply of interventions, with neither institutional details or a framework for evaluation. For example, market-smart fertilizer subsidies are recommended, without designing the details that would lower excess burden without impeding market development.

What these economists seem not to understand is that the competitive equilibrium as modeled by the interventionists is a straw man. For example, why assume perfect, one-price markets for some goods and non-existent or incomplete markets for others? Competitive equilibrium is a hypothetical construct meant to represent the polar-extreme in a world of bilateral contracts (Phelps 1985). In a world with information and enforcement costs, prices for the same commodity generally vary across agents for all commodities according to idiosyncratic transaction costs. Competitive equilibrium can be correspondingly modified ala Foley (1970) to incorporate unit transaction cost wedges analogous to shipping costs.

For contracting problems such as share tenancy and other agricultural institutions, the set of contractual equilibria (what the core shrinks to) is the relevant hypothetical construct.[19] For questions involving the relevant institutional form, contracting should include private governance, especially in dealing with shirking and selection problems. In contrast, Stiglitz (1974b, 1993, 2003) portrays private contracting as a straw man -- no private governance is allowed, despite a voluminous literature featuring monitoring, bonding, signaling, screening, and other governance mechanisms (e.g., Jensen and Meckling 1976; Williamson 1979; Hart and Moore 1990; Roumasset 1978, 1995).

Most of the non-fundamental policy prescriptions discussed above can be corrected by the application of existing tools from mainstream economics, including the correct application of the transaction-cost wedge model of the agricultural household to explain idiosyncratic risk aversion and to assess subsidies for microfinance and "market-smart" fertilizer programs. Likewise the economics of storage and transportation can expose the folly of parastatals. These correctives also reveal productive directions for the economics of transaction costs. First, the wedge model can be extended to general equilibrium framework so as to investigate cases in which the competitive equilibrium is or is not constrained Pareto optimal. Second, unit transaction costs themselves need to be endogenized using agency theory. And third, specialization needs to be endogenized in a dynamic theory, so that the intensification of production, specialization, and complexity of organization can be seen as coevolutionary.

For public economics to be useful for policy analysis in developing countries, it will need to be expanded to include the transaction cost paradigm just described. This will facilitate the analysis of the consequences of program and policy reforms in developing agriculture where pervasive transaction costs are a key determinant of behavior and organization. Likewise, the

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[19] Ironically, Stiglitz (e.g., 2003) often uses his 1974b principal-agent model of sharecropping as an illustration of the Greenwald-Stiglitz theorem.

accumulation of produced and natural capital, endogenous and idiosyncratic risk preferences, and the evolution of institutions, governance, and specialization are important candidates for extending development microeconomics. One step in endogenizing specialization can be made by applying the models of international trade to problems of domestic fragmentation. As these modeling efforts advance, it will also be important to distinguish between Dixit's (1996) second-best optimum and the third-best equilibrium that incorporates the effects of coalitions of public policy and the constitution of economic policy.

If the economics of agricultural development is to succeed in helping real-world agricultural development live up to its pro-poor growth potential, it must co-evolve with innovations in mainstream economics. The nature, causes, and consequences of trade policy should be extended to an understanding of economic fragmentation inside a country and policies that can facilitate the evolution of change of specialization. The economics of space, time, and quality should be employed to understand the consequences of parastatal food marking concerns. Yang's (2003) Economic Development, the New Institutional Economics of agricultural organization, and industrial organization in a globalizing economy need to be combined in a theory of agriculture's role in the evolution of specialization. Risk and uncertainty theory should become part of a dynamic theory of farm-household behavior. And the new political economy (e.g., Acemoglu 2005; Dixit 1996; Coate and Morris 1995; Besley 2007; Rodrik 2007) should be used to explain the causes of public policy and to provide politically-feasible reforms so that agriculture plays its needed role in sustained poverty alleviation.

These innovations will help foment a policy-oriented paradigm for agricultural development. Traditional development economics has been somewhat prescriptive (e.g., "thou shalt industrialize") without diagnosis of the patient's anatomy and with often scant attention to the possible consequences of alternative economic policies. In a more useful paradigm, policy analysis will be properly founded on positive explanations of the evolution of specialization and other patterns of rural and economic development, across both time and space.

A few promising applications of this ambitious research agenda are exemplified by the following, which, while listed separately, are interlinked:

i. Calculating the consequences of trade and regulatory price policies for the different agricultural prices that prevail domestically over space, time, and form
ii. Calculating the effects of lifting farm-size ceiling regulations
iii. Numerical demonstrations of how credit market reforms such as assistance to informal lenders would impact the shadow prices of credit across farmers
iv. Numerical illustrations of consumption smoothing under perfect certainty or risk neutrality and current-period risk aversion in the face of risk-neutrality toward fluctuations in lifetime income
v. Quantifying the welfare effects of decreased unit transportation or transaction costs on small-farm households, including the reduction of costly smoothing activities
vi. Quantifying the impact of limited-term fertilizer subsidies on the fertilizer distribution network
vii. Quantifying the effect of water markets on the allocation of water over time and space, where water exchange rates reflect water transport costs and differential water quality
viii. Quantifying first-best agricultural subsidies

for carbon sequestration using above and below-ground carbon-cycle dynamics and second-best enforcement scheme

CONCLUDING REMARKS

The importance of the economics of agricultural development for both sustainable growth and poverty alleviation has been recognized. But the predominant paradigm – seek-and-destroy market failure -- is not up to the challenge. Many of the policy ideas in WDR 2008 (e.g., subsidize farmer cooperatives, microfinance, crop insurance, and land reform) can only be properly evaluated with optimization and equilibrium models that incorporate transaction costs. Yet such models have not been formalized.

The infusion of public economics into the economics of agricultural development, which thrived in the 1980s, has stagnated due to the lack of foundations in transaction costs, dynamics, and the coevolution of specialization and governance. These subjects have foundered in economics as well, due to the paucity of examples. Thus, the stage is set for young dynamic scholars to develop new tools of analysis to explain empirical patterns in behavior and organization in developing agriculture and to build the foundations of a public microeconomics of development.

A first step is to extend the wedge model to general equilibrium and to investigate cases in which the competitive equilibrium is or is not constrained Pareto optimal. Second, unit transaction costs themselves need to be endogenized using agency theory. And third, specialization should be endogenized in a dynamic theory along the lines of Yang (2003) so that the intensification of production, specialization, and complexity of organization can be seen as coevolutionary. Finally, equilibrium models should allow for the accumulation/depreciation of produced and natural capital and the variation of shadow prices over time, as well as space and agent characteristics.

For cases involving common property resources and local public goods, competitive equilibrium must be extended to include voluntary local cooperation. The theoretical policy question of interest in such cases is, "What economic constitution can facilitate the efficient degree of local cooperation?" Formalizing the transition of the efficient locus of governance from local government to more centralized levels and the corresponding coevolution of governance and property will provide an important beginning. Finally, designing public policy reforms that are politically feasible will require both understanding the nature of the rent-seeking equilibrium being reformed and the creative packaging of win-win reform combinations (Buchanan 1989).

Modern development economics seems to be off-track. Empirical research has suffered from the quest for clever instruments and methods instead of informative results that estimate parameters of established theories, distinguish between competing theories, or challenge theory to explain empirical patterns. Less appreciated is the possibility that development theory may also be off-track, as enamored as it is with multiple equilibria, trap theories, new market failures, and the new case for social insurance. This latest fashion may partially reflect the difficulty of expanding public economics to transaction-cost economies. If the fundamentalist agenda sketched above is to progress, it will require not only theoretical results but illuminating applications.

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