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Introduction to development theory

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Abstract

This article introduces the symposium on economic development theory.

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Development Economics, a subject that studies institutions, growth, inequality and poverty in the developing world, is a large, lively and exciting area of research. The objective of this symposium is to put together some contributions in economic theory with a distinct focus on development questions.

Much of early development economics, as typified in the work of Paul Rosenstein Rodan, Arthur Lewis, Ragnar Nurkse, Tibor Scitovsky, Allyn Young, Gunnar Myrdal, Harvey Leibenstein, Albert Hirschman, Amartya Sen and others, was theoretical, though not necessarily in the sense of formal model-building. These authors all felt the need to bring new concepts into economic theory: ideas that would capture the essential features of underdevelopment, and provide explanations of the development process. Existing economic theory, with its focus on competitive markets, depersonalized exchange, steady technical progress, and balanced growth simply did not seem right when matched with the ground realities of underdevelopment: fragmented, imperfect, or just plain missing markets, highly personalized transactions in credit, insurance, or agrarian tenancy, ambiguous or nonexistent property rights, and widespread evidence of poverty traps and historical lock-ins.

A fair bit of modern economic theory was born in some of these early development writings. Young's article [40] on increasing returns was perhaps the first systematic study of nonconvexities in economic growth. Rosenstein-Rodan's celebrated paper [32] on post-war intervention in Europe provided a conceptual foundation for modern thinking on coordination failures

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and multiple equilibria. Hirschman [18], whose work in development continues to defy precise formalization, was—along with Rosenstein-Rodan—deeply interested in questions of "moving" an economy from a "bad equilibrium" to a "good" one, leading to his provocative and insightful ideas on "unbalanced growth". Scitovsky [34] stressed the fundamental role played by externalities, pecuniary (when markets are incomplete) and otherwise. Myrdal [26] was concerned with the self-fulfilling nature of poverty traps. Leibenstein [19] contributed to some of the above ideas and more: stressing the importance of efficiency failures created by traps and lock-ins as opposed to the distortion occasioned by departures from local first-order conditions. Sen's deep involvement with questions of economic inequality clearly lay at the heart of his concern with interpersonal comparisons and the development of these ideas in the theory of social choice [35,36].

These were radical ideas that did not sit well with the dominant Walrasian paradigm and its applications: simplistic models based on the marginal calculus that emphasized the Harberger triangles of deadweight loss. The novelty of the ideas had its cost: it was hard to embed these concepts within the neatly worked-out paradigms so assidiously summarized in the work of Walras, Arrow, Debreu, McKenzie and others. And so matters stood for several years: general equilibrium theory proceeded with calm inexorability, and there was little or no room for the seemingly untameable ideas that passed as "theories" of underdevelopment.

Two strands of thought were, however, to enable some integration of these ideas into mainstream economics. First, the rise of game theory in economics made it possible for people to study nonWalrasian scenarios with some degree of formal rigor. Economists who worked with Nash equilibria as their starting point were entirely comfortable with the generic inefficiency of such equilibria. Lack of efficiency in a decentralized world was no longer the variant. It was the norm.

Second, the work of economists such as James Mirrlees, Kenneth Arrow, Joseph Stiglitz and George Akerlof built on game theory to provide central, robust paradigms of imperfect information and its consequences.

These two strands of thought provided a home for central ideas in development economics. Agrarian contracts could be studied as special cases of principal-agent models. It was now possible to embed fuzzy ideas of coordination failure and multiple equilibria into a formal game-theoretic structure. One could provide a foundation for the persistent effects of increasing returns by invoking missing credit markets, which in turn had a firm information-theoretic foundation. It was possible to model social capital or informal institutions surrounding credit and insurance as a repeated game. Situations with pervasive externalities could now be taxonimized along the lines of the prisoners' dilemma or the coordination game. Adverse selection and moral hazard provided a conceptual home for a variety of development phenomena. By the late 1970s and early 1980s, applied theorists inspired by economists such as Stiglitz were working deeply and passionately on development questions. Development theory was born—ungainly, patchy and varied—but born nonetheless.

I do not have the space here to go into a survey of the directions in which development economics has progressed. For a quick introduction, I refer the reader to my entry in the New Palgrave Dictionary; see Ray [31]. The reader interested in a more detailed acquaintance with the subject is invited to read Bardhan and Udry [4], Basu [5], Mookherjee and Ray [24], or Ray [28]. The reader interested in a recent debate on theory versus empirics in development economics is invited to read Mookherjee [22].[1] For broad views of development, consult Dasgupta [8] or Sen [37].

[1] Mookherjee's article is followed by comments from Abhijit Banerjee, Pranab Bardhan, Kaushik Basu, and Ravi Kanbur.

It would be absurd to imagine that the papers in this collection are fully representative of the panorama of the subject. However, in their choice of questions and issues, they certainly come under the umbrella of recent theoretical research in development economics.

The papers by Newman [27] and Ghatak et al. [14] study theories of entrepreneurship. The subject is of critical importance to development research. What sorts of economic conditions are conducive to entrepreneurship? How might the market select entrepreneurs from a population of heterogeneous economic agents? Both these papers are fundamentally concerned, among other factors, with the nature of occupational self-selection, in which "being an entrepreneur" is one of several choices that an economic agent might make.

This sort of analysis is important because entrepreneurship, along with the usual suspects—physical capital and nonentrepreneurial human skills—is a central input into production. Growth accounting for developing countries suggests that those economies are well within some world technology frontier. But that distance to the frontier is just the famous Solow residual, a catchall for "technological differences". One suspects, however, that the nature and quality of entrepreneurship occupies a large part of this residual. Perhaps there are systematic reasons to believe that individuals "under-enter" the entrepreneurial category in developing countries. That is why theories of entrepreneurship have the potential to tell us something about underdevelopment.

Newman [27] revisits the risk-based theory of entrepreneurship, which argues that entrepreneurs are relative risk-lovers who insulate workers from fluctuations by paying them fixed wages. Entrepreneurs are, therefore, essentially insurers. If assets are correlated with attitudes to risk via decreasing risk aversion, this theory also predicts that entrepreneurs are likely to be richer than workers, which is a sensible enough finding in many circumstances.

All of this presumes, however, that the choice of occupation is the only available instrument for risk-sharing (if you want insurance, be a worker, otherwise bear risk and be an entrepreneur). In actuality, the payoffs in both these occupations could be partially insured by some third-party, the incompleteness of such insurance presumably arising from incentive constraints. Newman studies one such incentive constraint: the exertion of (noncontractible) effort by the agent. This leads to an opposing effect: it is actually *harder* to provide incentives to a richer agent, because the provision of such incentives necessitates utility differentials. If utility functions flatten out with increased wealth, this translates into the need for larger spreads in money space. There is, therefore, an obvious tradeoff here: richer agents are less risk-averse, and so they can be more easily given high-powered incentives on this account, but they also have flatter utility functions, and this effect goes the other way. For a broad class of utility functions identified by Newman, the second effect dominates the first and it turns out that *poorer* individuals bear *greater* risk. If we define entrepreneurship as risk-bearing, then, it is the poor who are more likely to be entrepreneurs.

This striking result may be viewed in two ways. One might conclude, as Newman does, that "a plausible modification of the basic Knightian model leads to an implausible prediction", and that "the fragility of this theory's empirical predictions suggests that we probably should look elsewhere for explanations of the roles and causes of entrepreneurship". Or one could—as I am provisionally about to do now—take the Newman prediction seriously. Perhaps it simply *is* true that the poor bear greater risk than the rich. This may be more convincing if we drop the labels "entrepreneur" and "worker", but it is not obviously false even if we retain those labels. In developing countries, the relatively poor engages in an enormous variety of entrepreneurial activities, ranging from agricultural tenancy to streetside retailing. The existence of the entrepreneurial poor is perhaps the single greatest reason for the large size of the services sector in developing countries.

Newman's contribution throws light on this issue. Developing economies are often very poor *and* very unequal. Therefore, as far as the poor are concerned, one expects very strong incentive impacts from relatively tiny variations in monetary payments. The high inequality, in turn, make the relatively rich not too different from the middle class in developed countries. The differential impact of the incentive effect is likely to be significantly heightened in developing countries.

Ghatak, Morelli and Sjöstrom [14]—GMS—study the effect of adverse selection in the entrepreneurial market. Suppose that economic agents have varying entrepreneurial ability; then the rate of return to entrepreneurship (e.g., as measured by the interest rate they pay on loans or the prices they command for their products) will be determined by the *average* entrepreneurial quality in the market. Therefore talented entrepreneurs are at a disadvantage; they invest less and borrow less.

Thus far the story is absolutely standard: simply replace "entrepreneur" by your favorite lemon and invoke Akerlof. GMS consider an important variant on this story: they allow for occupational choice. Suppose that agents who do not become entrepreneurs become workers (assume no adverse selection in that market). Now the equilibrium wage will be endogenous, and it will depend on the relative proportions of entrepreneurs and workers. To understand this equilibrium better, consider for a moment a partial-equilibrium version of the occupational choice model, one in which the wage rate is given. Now if that wage rate were to rise, the least talented agents in the entrepreneur pool will relinquish entrepreneurship and become workers: this softens the adverse-selection problem. That softening, in turn, leads to better conditions for entrepreneurs: they will invest more and hire more labor. If this response is strong enough, it will lead—at least over some range—to an upward-sloping aggregate demand curve for labor. Now return to the endogenous wage story: we see clearly the possibility of multiple equilibria, with high-wage-talented-entrepreneurship outcomes coexisting in the same model with low-wage-adverse-selection outcomes.

What is more, if no entrepreneur can resort to other means of separating himself and revealing her talent, then GMS show that such equilibria must be Pareto-ranked, with both employers and workers supporting policies such as minimum wage legislation to break the low equilibrium trap. Alas, only the Scandinavians (and perhaps not even they) seem to live in such an enlightened world, and it is only reasonable that GMS look for sensible modifications of their framework that do not yield such unrealistic camaraderie. Such modifications are easy enough to find: as GMS point out, if some employers can signal their talent by other means they will be insulated from the adverse selection problem and will certainly not support any policy that taxes entrepreneurs and subsidizes workers. One way of achieving this insulation in the credit market is to put up adequate collateral; no interest-rate premium is needed from deep-pocketed entrepreneurs to account for adverse selection. To the extent that other entrepreneurs cannot adequately signal their ability, multiple equilibria are still very much a possibility. In addition, a gain in wages will also ensure that less drastic steps need to be taken for adequate screening, adding again to the self-fulfilling nature of the wage increase. However, it is now clear that the deep-pocketed entrepreneurs will resist policies that move the economy to a high-wage equilibrium.

Since Rosenstein-Rodan [32] and Hirschman [18], models of multiple equilibria have been used in development economics as possible explanations of why the *very same* fundamentals may be compatible with widely varying equilibrium outcomes. The GMS paper fits right into this category. The role of inequality is also worth noting. If developing countries are highly unequal, then there is a real chance that screening devices (such as the ability to post collateral or rely entirely on internal financing) will exist for a subset of entrepreneurs, who will then vigorously oppose any form of minimum-wage legislation. In this sense, high inequality may be more conducive to the persistence of the bad equilibrium outcome.

In both these papers, a high level of ambient economic inequality sharpen the salience of the results. In contrast, the contributions by Mookherjee and Napel, Saint-Paul and Zeira all study the effect of different economic phenomena on the development and persistence of inequality.

The Mookherjee–Napel contribution [23] belongs to a growing literature in which imperfect (or missing) credit markets permit the past to cast long shadows into the future. The reason is simple: when capital markets are imperfect, the existing economic conditions that individuals find themselves in fundamentally affect their costs and benefits of investment (Dasgupta and Ray [9], Banerjee and Newman [3], Galor and Zeira [13]). In itself, this is not necessarily inimical to long-run "convergence" (Loury [21], but—as Ray [29], Ljungqvist [20], Freeman [12] and Mookherjee and Ray [25] have pointed out—when human capital in different occupations are imperfect substitutes, *every* steady state of the system may necessitate persistent inequality. Indeed, generally there is a continuum of such steady states, indexed—in a two-occupation model—by the proportion of individuals in one of the occupations. This indeterminacy means all sorts of policy shifts will have permanent effects on the steady state, as long as the shift is small enough and the starting point lies in the interior of distributional support.

The objective of the Mookherjee–Napel analysis is to allow for occupational mobility across generations by introducing ability shocks into the model, in the spirit of Loury [21]. It turns out that such heterogeneity also has the effect of markedly shrinking steady-state multiplicity: they are now generically isolated and finite in number. Thus heterogeneity generates mobility to be sure, but even a small amount of heterogeneity also reduces the extent of history-dependence. Small one-time policy interventions no longer affect long-run outcomes. Indeed, in many cases that the authors examine, the long-run outcome is unique and independent of initial conditions.

These findings should not be confused with results on ergodicity in the stochastic optimal growth model (see, e.g., Brock and Mirman [6] and Loury [21]). There, each economic agent follows an independent Markov process, so that with enough communication across states built into the ability shock, the long-run outcome must be independent of initial conditions. In Mookherjee and Napel's model (as in the literature that they extend), the economy-wide stochastic process is interactive across agents, the interaction occurring via the realization of equilibrium factor prices at every date. (And indeed, they obtain finitely many steady states in general, instead of full uniqueness.)

A full accounting of how initial conditions map into final steady states requires a study of non-steady-state dynamics. In the model with no mobility and a continuum of steady states, Ray [30] shows that competitive equilibrium with perfect foresight always converges to a steady state. With the introduction of heterogeneous abilities, however, competitive equilibria may fail to converge. Mookherjee and Napel provide conditions under which convergence can be restored. Under these restrictions, it is indeed possible in their model to map the dependence of long-run outcomes on initial conditions.

The two papers that follow, by Zeira [41] and Saint-Paul [33], both study inequality in dynamic models, though their concerns are of a more applied nature relative to Mookherjee–Napel. Zeira addresses the well-known phenomenon of widening wage differentials between skilled and unskilled labor, an ongoing process worldwide since the late 1970s. Two principal explanations have been offered for the widening gap. One is trade liberalization, a process that would presumably keep unskilled wages depressed and increase the rate of return to skilled labor, at least in developed countries. The second is simply the nature of technical change that has occurred over the last few decades, with economists arguing that it has been biased towards skilled labor. Zeira describes a two-country framework in which some of these effects can be studied simply and tractably.

In Zeira's model, trade occurs in a continuum of intermediate goods, each of which can be produced by one of (at most) two linear technologies. The first uses unskilled labor alone. The second uses skilled labor alone. The emergence of the second technology is what Zeira refers to as technical progress. Thus in this framework, technical progress is a steady expansion of the *set* of intermediate goods which can be produced by two technologies instead of one.

Now, the very fact that a technology is available does not mean that it will be automatically adopted. After all, skilled labor may be expensive relative to unskilled labor. If we view a country as relatively developed if it has a higher percentage of skilled labor, then the developed country will have a lower skilled wage premium and will adopt more of the new technologies. (The consequently greater reliance on skilled labor will mean that the skill premium will not be as low as that indicated simply by skill endowments alone.)

Now permit these two countries to trade with each other, possibly with exogenous restrictions on the set of tradeables. We then obtain a simple general equilibrium model of trade parameterized by two objects: the set of goods for which the skilled technology is available (the "technology frontier") and the set of goods which are tradeable (the "trade frontier"). An expansion in the former is *technical progress*; an expansion in the latter is *trade liberalization*. The model permits Zeira to examine the effects of these changes, not only on fairly standard outcomes (wage inequality for instance), but on other auxiliary indicators. These include the pattern and importance of trade, or TFP differences across developed and developing countries.

The model certainly delivers some of the usual findings. Skill-biased technical progress increases wage inequality in both developed and developing countries, though with the particular kind of progress that Zeira considers, the effect is likely to be substantially higher for developed countries. This is a consequence of endogenous adoption.[2] In contrast, trade liberalization increases developed-country inequality while it lowers inequality in developing countries. This is a standard argument for pinning the blame on technical progress, and the model supports it.

But the model also yields other implications. For instance, technical change and trade liberalization have different predictions for the importance of trade (relative to GDP): technical progress raises this share for developed countries and generally lowers it for developing countries, while trade liberalization will increase trade shares all around. These implications, apart from being of intrinsic interest, serve as another potential avenue for discriminating between the two classes of explanations for wage inequality. (Zeira takes some very preliminary steps in this direction.)

Endogenous technology adoption also creates a "multiplier effect" for TFP differences across countries, as the initial surge in the technical frontier is more fully utilized in developing countries. The same amplification can also be brought about by trade liberalization, as developed countries specialize more heavily in skill-intensive production. Such amplification effects are potentially of great interest in the study of international productivity differences. While the present model is limited in several respects, including the omission of physical capital, it seems to me to take a significant step in the understanding of such issues.

I have already alluded to the fact that Zeira uses a particular notion of technical progress, measured by the number of goods for which a new technique is available. This is different from a

[2] The interesting case to consider is one in which the technical frontier serves as the binding constraint for developed countries, so that all goods with two available techniques are indeed produced using the skilled technique in those countries. In contrast, assume that in developing countries it is *not* worth adopting all techniques that are available, either because (Hicks-neutral) productivity is too low or because the skill premium is too high. Then it is easy to see that the "first-order impact" of technical progress is to raise inequality in developed countries, with no effect on developing countries. There will be general equilibrium effects as trade patterns respond, but in general these will not swamp the first-order effects.

traditional, black-boxed view of technical progress as "increased productivity", and the unpacking of this vaguer concept yields useful insights. Very much the same is true of the contribution by Saint-Paul, which emphasizes yet another aspect of the knowledge process. Saint-Paul's objective is to study how technical knowledge is received, used in production, and possibly re-transmitted to other producers, and the effect of such "knowledge chains" on economic inequality.

Saint-Paul [33] begins with the observation that a modern economy exhibits "knowledge ladders", through which knowledge is produced, applied and passed on to ever greater audiences, until it is finally used in the productive sector. Technical change in Saint-Paul's paper is equated with the ease of transmission of knowledge, measured by the number of agents who can obtain knowledge from a single supplier. The supplier may, in turn, have received the same information from someone "higher up" in the knowledge chain.

To understand the forces that transform agents into producers of knowledge, assume that agents have heterogeneous ability, and that such ability is complementary to acquired knowledge in the production of human capital. Assume, moreover, that human capital can be used either in production, with linear returns, or in the further transmission of information (a market-based "knowledge sector"). Finally, suppose that the quality of transmitted knowledge is increasing in the human capital of the provider. Then a simple single-crossing argument guarantees that agents above a certain ability cutoff specialize in knowledge transmission, while those below the cutoff specialize in production. The bottom of the ability distribution specializes in productive work, and consecutive ability intervals or "bins" in the ability distribution produce knowledge for the bin one step down. Wages rise through the ability distribution with increasing marginal returns.

Now, if the transmission technology for knowledge becomes more effective in the sense described earlier, some knowledge workers will necessarily be displaced downwards: every member of the knowledge chain now taps on the resources of higher-ability knowledge producers, and those high-skilled teachers reach greater audiences. Thus a shift in the transmission technology tends to hurt intermediate knowledge workers. Knowledge becomes cheaper, so buyers seek out higher quality, increasing the demand for high skills and displacing lower skills in each bin to a lower level in the knowledge ladder. Low skill production workers, on the contrary, unambiguously benefit from the cheaper access to high quality knowledge.

Saint-Paul's contribution (as well as Zeira's) is valuable because it explores a new dimension of technical progress. In doing so we obtain a more nuanced account of what is happening to inequality. It may well be that once a summary statistic of inequality—such as the Gini coefficient—is applied to the wage distribution across the knowledge ladder, overall inequality will appear to have increased. But an analysis of the overall Lorenz curve will reveal more nuanced changed, possibly with local clustering near the bottom of the wage distribution, and even larger returns to the highest skills. Indeed, Saint-Paul's analysis is more consistent with the idea that technical progress increases *polarization* (rather than inequality) across the wage distribution, in the sense described by Esteban and Ray [11]).

I now turn to the remaining two papers in this issue, those by González [16] and Anderson [1].

One of the most crucial concerns in developing countries is the security of property rights, and what that implies for investment and growth (see Ray [31] for a quick discussion of the main issues, and references). Many authors have studied this theme, largely from an empirical perspective. In line with Demsetz [10], the bottom line for many of these authors is already very clear: the security and unambiguity of property rights is a necessary condition for development. Their task is to hunt down correlations—often across countries—that support this view. For a smaller set of writers, beginning with Weitzman [39], ambiguous property rights often have equity effects that do not go

the same way as efficiency-minded economists would like them to go (Cohen and Weitzman [7], Baland and Platteau [2], Goldstein and Udry [15]). The ambiguity of property rights can serve as insurance, redistributive device, or buffer against extreme poverty. For such economists (and I include myself in this group) the question of property rights is a far more nuanced issue.

The contribution by González [16] displays a similar reticence to give property rights an unambiguous green signal. But it is particularly distinctive in that it places the discussion on exactly the same turf that efficiency-minded economists like to stand on: investment, growth and *Pareto* efficiency rather than equity. A second distinctive feature of the paper is that it considers intermediate property rights rather than the pure and unrealistic extremes of no rights, or fully protected rights.

González considers a model of endogenous growth in which property rights, such as they are, may be both challenged or secured by the expenditure of private resources. Appropriative and defensive activities both come out of an individual's budget, and therefore compete for attention along with the more traditional accounts of consumption and productive investment. The *security* of property rights is then an equilibrium outcome, though there are deeper parameters in the model which determine the ease with which property can be attacked or defended, and which for González constitute the primitive parameter determining rights.

Consider, now, the effect of an improvement in property rights: an increase in the efficacy of a given defensive budget (or a decrease in the efficiency of appropriation; the same parameter does double duty for both). In equilibrium, this change must reflect itself in safer property. Safer property certainly means that productive investment is more attractive than appropriation, a perfectly standard effect that encourages growth. But there is a second effect: as growth increases, the returns to appropriation rise over time. This tends to raise the amount of resources devoted both to attack and to defense. Notice that the second effect would never kick in if the initial parametric shift had been to a regime of *perfect* property rights. The second effect depends on property rights (while improved) not being fully secure.

In a static model, the second effect would arise as an indirect feedback and considerations of "stability" would guarantee that it cannot swamp the salubrious "direct effect", so that in the net, the greater security must be valuable. But in the fully dynamic model studied by González this is not the case; the negative effect arises later in time as the economy proceeds along the higher growth path. Surprisingly enough, González is able to show that there is always an interval of parametric values over which an improvement in property rights must lead to a Pareto-reduction in lifetime utilities; the second effect dominates the first. González concludes that faster economic growth may indeed lead to greater "conflict" in societies where property rights are not secure to begin with. Moreover, a tentative reform that improves property rights, but not comprehensively so, can itself generate the faster growth that is later so costly to society.

Other authors, such as Skaperdas and Syropoulos [38] or Grossman and Mendoza [17], have made similar points in their writings. For instance, Skaperdas and Syropoulos link intertemporal issues with conflict by arguing that more patient individuals may have a greater incentive to engage in appropriative activities. Grossman and Mendoza discuss a paradox of "anticipated abundance", in which the prospect of greater wealth in the future may intensify conflict today. While González's contribution contains some of these elements, it goes a step further in showing how more secure (but not fully secure) rights may create that very "anticipated abundance" which then lowers welfare by encouraging higher levels of appropriation in society.

The last paper in this collection [1], is a bit of an outlier relative to the others, but no less important for that. Anderson has actively written on the economics of marriage matching and

dowry, and I was fortunate to be able to include here one of her several contributions on the subject.

Anderson addresses the argument that population growth causes both the *marriage squeeze*—a rise in the average marrying age of women—and dowry inflation. Because men marry at a later age than women, the age pyramid guarantees an extra supply of women. Population growth flattens the age pyramid, creating an even greater excess supply, so that in a society in which dowry has been historically acceptable, the equilibrium dowry must climb. Simultaneously, women accommodate the excess supply by marrying later, which is the marriage squeeze.

However, in her formalization of this argument, Anderson argues that the two, in fact, cannot go together. If there is a population shock in one period, this causes a surplus of brides when this generation reaches bridal marrying age (since women marry elder grooms). Provided everyone marries, some brides must postpone marriage in succeeding periods, until the men from the shock generation reach marrying age. This leads to the "marriage squeeze". Now, the equilibrium time path of dowry payments depends on the sharing of marriage surplus between the matched couples and the cost of delaying marriage by one period. In all periods in which there is a marriage squeeze, women do not obtain a share in the marriage surplus, so that dowry payments must fall for elder brides by exactly their cost of delay.

There are several points in this paper that merit further discussion, and by no means will it be the last word on the subject. A full description of dynamics with ongoing population growth, as well as the implications of alternative, pairwise no-blocking conditions,[3] must await further research. However, it is difficult to overestimate the crucial importance of this subject. The leading case for dowry, India, exhibits a high and growing sex ratio in favor of males. One would be tempted to predict that such a skewed ratio must eventually bring the laws of supply and demand to bear on the marriage market, so that men no longer command a positive equilibrium price. However, this does not appear to have been the case.

What shores up dowries? One can think of several explanations: growing income inequalities between men and women, a rising male–female age differential at marriage (though one would not bestow true exogeneity on that variable relative to the problem at hand) and yes, growing populations. Anderson's contribution has been to enter this discussion with the serious intent of conducting the argument formally and carefully. Her assertion that this last explanation—growing populations—may be problematic, therefore deserves careful attention among demographers, sociologists and economists seeking to understand the problem of dowry.

This concludes my introduction to the symposium on development theory. Let the papers now speak for themselves!

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