Theory, General Equilibrium, Political Economy and Empirics in Development Economics*

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Abstract

I discuss the role of economic theory in empirical work in development economics with special emphasis on general equilibrium and political economy considerations. I argue that economic theory plays (should play) a central role in formulating models, estimates of which can be used for counterfactual and policy analysis. I emphasize why relying solely on "instruments" for this type of analysis may be insufficient, and discuss why, in certain instances, counterfactual analysis based on microdata that ignores general equilibrium and political economy issues may lead to misleading conclusions. I illustrate the main arguments using examples from recent work in development economics and political economy.

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1 Introduction

I believe that without economic theory we cannot make sense of the causes of poverty and low income in much of the world or make progress in designing policies that could contribute to individuals, regions and nations unleashing their economic potential. Unfortunately, this view does not easily translate into a "recipe" for doing academic research and policy work that combines economic theory with data and econometrics. There are two main reasons for this. First, economic theory often fails to provide as clear a guidance to empirical work as we may wish; this is both because of the complexity of the problems we are dealing with in the context of development economics, and also because we, as theorists, do not always do a good job of distinguishing which elements of theories and models should be taken empirically seriously, tested and used as part of policy analysis. Second, many economic insights that are important and relevant for development problems come from "general equilibrium" and "political economy". General equilibrium here refers to factors that become important when we conduct (or ask questions involving) counterfactuals in which large changes are contemplated, and the difficulty lies in the fact that such counterfactuals will induce changes in factor prices or technology that we hold fixed when we do partial equilibrium analysis and empirical work exploiting microdata. Political economy refers to the fact that the feasible set of interventions is often determined by political factors and large counterfactuals will induce political responses from various actors and interest groups. General equilibrium and political economy considerations are important because partial equilibrium estimates that ignore responses from both sources will not give the appropriate answer to counterfactual exercises. Unfortunately, neither is straightforward to incorporate into a systematic empirical strategy, though several recent papers make progress in this direction.

In the rest of this essay, I will try to substantiate the claims made in the above paragraph. In Section 2, I briefly argue for the role of theory in thinking about the key problems in development economics. In Section 3, I discuss why it is difficult, in general, to know how to make economic theories empirically operational, and I suggest a preliminary approach to thinking about these issues more systematically. While my conclusion is that there is no simple recipe (and I do not even understand what the "right way" of doing this would be), I will point out that some papers make progress in a direction that I find more promising, and I will illustrate this by using the example of a recent paper by Weese (2009). In Section 4, I will explain why I believe "general equilibrium" reasoning is important in several major problems in development economics, and I will try to illustrate this using my work with Simon Johnson on the effect of life expectancy on growth (Acemoglu and Johnson, 2007). In Section 5, I will confront one type of empirical strategy without economic theory, which uses "instruments" without some explicit or implicit theory about the first and second stages of an instrumental-variables strategy, and will illustrate the problem with this strategy continuing with the example of the effect of life expectancy on economic growth. In Section 6, I will argue why political economy considerations have to be central to any investigation of development problems, and how inferences that ignore political economy can go wrong. I will then discuss recent papers by Goldstein and Udry (2008), Ferraz and Finan (2008, 2009), Dell (2009) and Naidu and Yuchtman (2009) as fruitful attempts at using political economy in answering important questions in development economics. Section 7 concludes.

2 Why We Need Theory

It is generally accepted that we need a theoretical framework to make sense of the world. What is more controversial in economics is whether we wish to use economic theory to obtain "structural parameters" to be estimated. Structural parameters have a mixed reception in economics, and perhaps rightly so. Take a simple problem: understanding the relationship between the cost of schooling and schooling decisions. We can describe this relationship purely as a descriptive one, focusing on a sample and looking at the correlation between the two variables (or at conditional correlation, which is what multivariate ordinary least squares would amount to). For example, we could specify the following "reduced-form" relationship:

$$\log(s_i) = \mathbf{X}_i'\boldsymbol{\beta} - \alpha \log(c_i) + \varepsilon_i, \tag{1}$$

where as usual i denotes an individual in the sample, s_i is years of schooling, c_i denotes the cost of schooling to the individual resulting, for example, from forgone earnings and actual costs of attending schools, $\mathbf{X}_i$ is a vector of characteristics of this individual that we may wish to control for, and $\boldsymbol{\beta}$ is a vector of parameters. The parameter of interest is α. We can then use ordinary least squares (OLS) to estimate $\boldsymbol{\beta}$ and α.

Alternatively, we could start with a simple economic model. In fact, some simple theories will lead to an equation identical to (1). Suppose, for example, that human capital that an individual i will be able to sell in the market is given by $h_i = s_i^{1-\sigma}/(1-\sigma)$, for some parameter $\sigma \in (0,1)$, and can earn income equal to $y_i = wh_i$, where w is the market wage per unit of human capital. In addition, individual i has a cost of schooling given by $\zeta_i c_i s_i$, where ζ_i is a non-observed non-monetary cost component and c_i is the monetary cost of schooling for this individual. Suppose that each individual maximizes net income, so that individual i will choose schooling according to the following program:

$$\max_{s_i \geq 0} w \frac{s_i^{1-\sigma}}{1-\sigma} - \zeta_i c_i s_i. \tag{2}$$

This gives the optimal choice of individual i as

$$s_i = K \left(\zeta_i c_i\right)^{-1/\sigma}, \tag{3}$$

where in this case $K \equiv w^{1/\sigma}$. Taking logs and defining $\varepsilon_i \equiv -\log \zeta_i / \sigma$, this simple model implies a relationship identical to (1), with α now corresponding to $1/\sigma$. Once this equation is derived from (2), estimation is again straightforward. Since (1) is linear in parameters and has an additive error term, OLS can again be used for estimating $\boldsymbol{\beta}$ and α as well as relevant moments of the error term ε_i.

Next comes the harder part. We have seen that equation (1) can be posited as a reduced-form relationship, or it can be derived from a simple economic model. But at the end it is the same equation, and it can be estimated in the same manner. So what is the sense in which we can think of it as a "structural relationship"?

To answer this question, let us first define $D\left(c, s \mid \boldsymbol{\theta}, \mathbf{X}, \mathbf{Z}\right)$ as the data generating process (DGP), determining the joint probability distribution over the cost of schooling and years of schooling for the population of interest, given the parameters $\boldsymbol{\theta}$ (which might include $\boldsymbol{\beta}$ and α), and the values of the vector of variables $\mathbf{X}$ and some other additional variables $\mathbf{Z}$. One definition would be that (1) corresponds to a structural model and we can think of α as corresponding to the true structural parameter of interest if the DGP has a representation in the form of equation (1). Notice that this definition did not make any reference to whether (1) was derived from economic theory.

Nevertheless, I think economic theory is crucial, and the reason for this can be seen from a slightly different definition of a structural parameter based on a notion that I will refer to as *counterfactual validity*.[1] A large part of the reason why we estimate empirical relationships such as (1) is that we would like to use the parameter estimates for counterfactual or policy analysis, or we wish to test some underlying theories.[2] I define a parameter as "structural" if it is useful in conducting counterfactual analysis—or equivalently, if it, or the model on

[1] This definition is closely related to, though also somewhat different from, Marschak's (1953) definition, which distinguishes between structural parameters that provide "useful knowledge" for understanding the effects of policy within a given sample and/or in new environments. It is also related to the estimation of "causal" effects (e.g., Angrist, Imbens and Rubin, 1996), though the implicit requirement here is somewhat stronger, since causal effects may be specific to a particular experiment or sample, and thus may lack counterfactual validity.

[2] Our interest in testing and discriminating between theories is related to our desire to use these theories in

which it is based, has "counterfactual validity". For example, we could ask the question: what would be the effects of subsidies to reduce the cost of schooling, c_i, for a set of individuals? This counterfactual (policy) experiment could be motivated by a potential policy that is being contemplated or it may be used for understanding the implications of our theory. It could be for the same sample on which the initial estimation was performed, or it could be for an entirely different sample or population. In either case, one answer to the above question readily follows from using the estimates of α to compute the increase in the years of schooling for individuals whose cost of schooling has declined. But can we really trust this answer?

If α is indeed a structural parameter or has counterfactual validity, then the answer is yes (obviously, subject to standard errors). But how do we convince others (and perhaps ourselves) that this answer is indeed plausible? This is where economic theory becomes useful. As a first step, we have to defend, using economic theory, common sense and evidence, that the functional form in (1) captures relevant aspects of the reality ("is a good approximation to reality"), that this functional form is stable over time and across relevant samples, that variation across individuals not captured by the covariates and the cost of schooling can be incorporated into the error term ε_i, and that this error term indeed can be modeled as additive and orthogonal to (uncorrelated with) the other variables included in equation (1).[3]

A precondition for counterfactual validity is that key factors that affect responses to the relevant counterfactual should be included in the model being estimated. Using economic theory, for example specifying a model such as (2), is often the best way of clarifying whether key factors have been omitted, and whether the underlying assumptions can be defended and provide a good approximation to reality. However, the previous discussion also highlights that specifying a model that justifies a specific estimation equation is typically not difficult. The proper use of economics does not mean writing down of a specific model; instead, we should be

other spheres and thus it is closely related to "counterfactual analysis". This appears consistent with Marschak's (1953, p. 1) emphasis, when he starts his classic article on structural estimation with the sentence: "Knowledge is useful if it helps to make the best decisions." In the remainder of the paper, I will focus on counterfactual analysis, broadly construed to implicitly include the "testing of theories". Nevertheless, in some instances "testing theories" could be less demanding than estimating parameters valid for counterfactual analysis. For example, an approach similar to Townsend (1994) would be useful in discriminating between certain theories of risk sharing, but would not typically deliver estimates that can be used for counterfactual exercises.

[3] This discussion also highlights that in certain cases one could achieve counterfactual validity without much theory. For example, we need only the most basic theory in interpreting a controlled experiment designed to evaluate the effectiveness of a drug. In this case, we can say that common sense and a very limited amount of medical theory are sufficient to interpret the results of the controlled experiment and decide whether they are informative about the effectiveness of the drug in question beyond the experimental setting. It should also be noted that the evaluation of the effectiveness of a drug in this example has a clear parallel to "modeling individual behavior" in economics. As further emphasized in Section 4, the role of economic theory becomes even more central when our focus shifts to "modeling equilibrium behavior".

developing the case that economic theory *robustly* leads to the estimating equation in question and clarify which important economic mechanisms and effects are being excluded.

Another advantage of the structural reasoning based on theory is that once we go through the process of explicitly justifying the equation we are estimating, either using economic theory or other theoretical or empirical arguments, we may realize that such an equation cannot really be defended and it has to be interpreted with greater caution.[4] Finally, economic theory provides us the best way of interpreting what the estimates from an equation such as (1) mean (for example, when it is derived from (2), $\alpha = 1/\sigma$ is a function of the elasticity of the human capital production function).

The key pitfall is to use (1) for counterfactual or policy analysis when it cannot afford a structural interpretation. To elaborate on this point, let us return to the DGP definition. Imagine that one of the variables in $\mathbf{Z}$ in the DGP is the subsidy to the cost of schooling (this variable is not directly in our vector of covariates, $\mathbf{X}$). Suppose that the DGP indeed corresponds to (1). Then, we can measure the effect of the subsidy on the cost of schooling, and compute the increase in schooling using the estimates of the parameter α. In contrast, if the true DGP does not have a representation of the form (1), then the counterfactual analysis based on the estimation of this equation will lead to misleading results.

Imagine, for example, the case where years of schooling are constrained by school enrollments, which are in turn constrained by the sizes of schools. More specifically, suppose that the constraint on school size implies that total years of schooling for all individuals $i \in \mathcal{I}$ should be equal to $\bar{S}$, or $\sum_{i \in \mathcal{I}} s_i = \bar{S}$. Furthermore, to simplify the discussion suppose that (3) still holds at the individual level, though with a different value of K, because individuals with low cost of schooling get proportionately more of the available school resources (for example, due to some type of efficient rationing). The constraint on total schooling then implies that

$$K = \bar{S} \left(\sum_{i \in \mathcal{I}} (\zeta_i c_i)^{-1/\sigma} \right)^{-1}$$

in equation (3). In this simple example, the DGP is not given by (1), though when we estimate the relationship between s and c imposing the reduced-form functional form in (1), we will nonetheless obtain a meaningful(-looking) estimate of α. The problem is that when the cost of

[4] Many empirical equations that do not correspond to structural relationships or to the true DGP may nonetheless contain useful information. Even if what we are uncovering are just correlations, this can help us distinguish between theories, since many relevant theories will have implications about what these correlations should look like. This suggests that it may sometimes be useful to estimate "reduced-form" relationships that do not have structural interpretations, but when doing so, we should be explicit about how they should be (and not be) interpreted.

schooling is subsidized, the DGP remains unchanged, but after this policy, the reduced-form relationship captured by (1) will change (exactly as captured by the above formula for K), and the estimate of α from the pre-subsidy regime will not necessarily inform us about the post-subsidy relationship between cost of schooling and years of schooling.

In particular, in the simple example discussed in the previous paragraph, if all costs are subsidized by the same proportional amount, so that the cost of schooling for individual i declines to c_i/μ where $\mu > 1$, this will have no impact on the schooling choice of *any* individual. Nevertheless, the reduced-form estimate (1) would imply an increase of $\alpha \log \mu$. The problem described here is, of course, nothing but a version of the Lucas critique (Lucas, 1976), that "reduced-form" form relationships will not be stable in the face of policy interventions. Thus, our confidence in the implied answers to policy experiments crucially depends on our confidence in having captured the appropriate structural relationship (the DGP) with the model we are estimating.

The structural approach also faces major challenges, however:

- How can we really justify (1)? Certainly, writing down a model such as the one in (2) is not sufficient. That model itself made several assumptions which are restrictive and may not provide a good approximation to the economic phenomena we are interested in. Put differently, the key justification for a structural relationship does not come from finding *a* model that delivers it, but from using economic theory to show that a range of models *robustly* lead to the predictions being investigated.

- Why do we even think that there is a constant elasticity α between years of schooling and costs of schooling?[5] After all, we know that all theories are abstractions and approximations, so there is little reason to believe that a parameter such as α—or the intertemporal elasticity of substitution, or the Frisch elasticity of labor supply, or the elasticity of substitution between two factors, or any elasticity—should be really constant. But without such constancy, there are severe limits to counterfactual validity.

- Do "structural parameters" really exist? What we take as a structural parameter for one theory will naturally become an endogenous object in another, so that even if we approximate the true DGP by $D\left(c, s \mid \boldsymbol{\theta}, \mathbf{X}, \mathbf{Z}\right)$, and have some parameter α^* that gives us useful information for several counterfactual variations in policy and environment

[5] When I was a graduate student, Ken Binmore once asserted that elasticity was a terrible concept because economists define it and then assume that because it has been defined it must be constant. I only now understand how right he may have been, even though the conversation was in a noisy pub.

captured by the variables in $\mathbf{Z}$, the true DGP will generally be much more complex, for example, $D(c, s \mid \boldsymbol{\theta}, \boldsymbol{\theta}_1, \mathbf{X}, \mathbf{Z}, \mathbf{Z}_1)$, where $\mathbf{Z}_1$ is another vector of variables or interventions, and once these change, the relationship of interest will also change in line with the parameters represented by the vector $\boldsymbol{\theta}_1$. So a simplified model can serve us well as an abstraction for a series of counterfactual experiments, but there will exist other experiments for which it will be much less informative or even misleading.[6] This is almost by necessity: a precondition for counterfactual validity is that key factors relevant for the outcome of the counterfactual should be included in the model and models as abstractions have to exclude several relevant factors, so no single model could include all of the relevant factors for all possible counterfactual exercises.

The challenges discussed in the preceding paragraphs notwithstanding, it is clear that we often have to take a position about the parameters being estimated corresponding to structural parameters (at least for a well-defined though perhaps limited set of variations in environment and policy)—otherwise we will have no claim to counterfactual validity and no way of conducting counterfactual policy analysis.[7] We often need a claim to counterfactual validity (even if it is only implicit), and economic theory is our best guide for formulating the appropriate models and justifying such claims to counterfactual validity.[8]

3 Conversation Between Theory and Econometrics

The previous section emphasized the importance of economic theory in helping us specify empirical models with some degree of counterfactual validity. But how do we do this in practice? Economic theory is based on mathematical models that are abstractions of reality. Our models involve several assumptions, many of which are adopted for convenience or as "simplifying" assumptions. With all of its assumptions, a model implies a set of relationships

[6] The concerns neither in this nor in the first bullet point depend on the fact that the parameter $\alpha = 1/\sigma$ in (2) is not one of the commonly-used preference or technology parameters, such as the coefficient of risk aversion or some elasticity of substitution, which we normally identify with the Marschakian structural parameters (Marschak, 1953). Exactly the same questions apply to Marschakian preference or technology parameters. For example, an elasticity of substitution may be constant with respect to certain variations, but would change in response to others.

[7] See, however, Imbens (2009), for an insightful discussion, with a different emphasis, of when one might prefer different types of estimates for policy analysis.

[8] Taking such a position does not require one to spell out and solve a complete or complicated economic theory. Sometimes, for example, as in Angrist and Krueger (1991) discussed in footnote 20, the relationship is clear and sufficiently well-grounded in economics or common sense that a fully spelled out theory may not be necessary. The important point is that even in such cases external validity relies on a (sometimes implicit) theoretical justification.

between different variables. But we may not necessarily wish to take all of these relationships as empirical predictions to be tested or used for counterfactual analysis. I now suggest that we ought to distinguish between the key implications and auxiliary implications of models, though in practice we often fail to do so.

Let us think of a model as a mapping from assumptions into empirical relationships. For concreteness, let us think of these empirical relationships as moments in the data (e.g., the conditional covariance of one variable with another), and focus on a specific problem: the relationship between income distribution, credit markets and occupational choice, for example, studied by Banerjee and Newman (1993)—henceforth, BN. Let $\mathcal{A}$ denote the set of assumptions that one could make in modeling this problem. A element $A \in \mathcal{A}$ corresponds to a set of assumptions, that is, such things as the exact specification of production functions, the parameterization of the joint distribution of talent and initial wealth, the intertemporal preferences of agents, assumptions concerning how the credit market works, and assumptions on conjectures, expectations and the equilibrium concept. Since A is an instance of a complete set of assumptions for the problem at hand, we can think of this as a "model". This model A then generates some empirical implications. I will summarize these by a set of moments, denoted by $M \in \mathcal{M}$, which could include the correlations between the interest rate, the occupational distribution, productivity and initial wealth. Economic theory then amounts to using these assumptions in order to derive empirical relationships, or sets of moment conditions. Thus we can think of economic theory as a mapping (correspondence) $f : \mathcal{A} \rightrightarrows \mathcal{M}$, specifying which set of moments we should expect in the data for a set of assumptions.

The difficulty here is twofold. First, in writing down a model $A \in \mathcal{A}$, most theorists, rightly, will go for a minimalist structure. In many instances, A will not even contain any stochastic elements. For example, BN's model leads to a unique nonstochastic equilibrium for most parameter values, where the initial distribution of wealth together with an individual's wealth determines his and his dynasty's occupational choices. To generate moment conditions that would correspond to correlations in the data one would then have to add an unmodeled error term. Although one could interpret this error term as coming from "measurement error," this is clearly not a satisfactory interpretation, since there are many other factors relevant for occupational choices not captured by the model at hand, and they will all be subsumed into this error term, though they are not in reality related to measurement error. Yet, this is not a shortcoming, but in some sense a strength of the model. BN's model is successful largely because it abstracts from several features of the world. Second, for the same reasons of parsimony and simplicity, a model typically involves assumptions that are "auxiliary," meaning that they are made for convenience, and in the hope that they are not the source of its "main" conclusions. Naturally, what these main conclusions are is not always a simple matter to determine.

This issue notwithstanding, the central difficulty here is that the set of moment implications M depends on the entire set of assumptions, A. Suppose, for example, that $A = A' \cup A''$, where A' and A'' are two disjoint sets of assumptions, and those in the set A' correspond to the "key" assumptions, while A'' contains the auxiliary and simplifying assumptions. For example, in BN, the assumption that all individuals have the same ability in all occupations, that there is no intensive margin of production, and that dynastic saving decisions are "myopic" are auxiliary assumptions. These assumptions, taken together, lead to a set of predictions. For example, taken naïvely, the model implies that there will exist a threshold level of wealth, such that all dynasties with initial wealth below this level will remain in subsistence or become workers, whereas those just above this threshold will become entrepreneurs. If we were to take such an assumption seriously, it would lead to the rejection of the model, but this would not be an insightful rejection. Instead, the BN model is insightful because it highlights how the credit market problems create a link between wealth and occupational choice, and how this link depends on factor prices, which are themselves endogenously determined by the entire distribution of income.

This discussion highlights two problems. First, the insights from certain models may be "conceptual" and thus difficult to translate into moment conditions. For example, the insight that income distribution matters for occupational choices of an individual (with a given income level) is a conceptual point, even though one could devise tests by comparing different economies or the same economy over time in order to investigate the degree to which such a link is present. Second, not all of the implications of the model should be taken seriously. The second problem suggests that we may wish to separate the set of moment conditions, M, into two disjoint sets, $M = M' \cup M''$, so that M' corresponds to the set of "robust" moment predictions, which we should test or use as guidance for empirical work, whereas M'' corresponds to the moment conditions generated by the "auxiliary" assumptions. However, such a separation is not typically possible, since each moment implication of the model is potentially generated by all of the assumptions taken together. In the BN model, for example, we cannot simply remove the assumptions regarding the form of the production function and still obtain moment conditions about the relationship between occupation and wealth.

This discussion suggests that in formulating economic theories which we wish to apply to data (either by ourselves or by others), we should pay special attention to which dimensions of the model are introduced just for achieving tractability and parsimony (the so-called "auxiliary") assumptions, and which assumptions and implications of the model are "robust" and should be relied upon and used empirically (or conceptually). Unfortunately, we do not have the theoretical and econometric tools to achieve this,[9] and developing such tools, or at the very least, trying to emphasize in specific instances which predictions are more robust, would be a useful direction for future research. In addition, even though such tools are not currently available, in specific instances, considerable progress is possible. I will now illustrate this using a recent paper by Weese (2009).

Weese (2009) studies the mergers across Japanese municipalities. Changes in Japanese government policy on municipality finance in 1995 led to major changes in municipality structure. Many small Japanese municipalities that did not previously have incentives to merge, because this would have reduced the transfers they received from the central government, were induced to merge after this change in policy and the number of municipalities declined from 3232 to 1800. Mergers across municipalities are important for public finance (because they determine the type and amount of local public goods), for development economics (since there are marked inequalities across municipalities in terms of income and provision of public goods, e.g., Acemoglu and Dell, 2010), and for political economy (as they are a major example of endogenous coalition formation). Weese is interested in estimating the "preferences" of different municipalities concerning mergers, and whether given these preferences, a better policy could have been devised. This type of counterfactual exercise clearly requires structural parameters in which we can have some confidence. Thus this exercise must start with a theoretical model, which will then be estimated to obtain structural parameters to use for the counterfactual and policy analysis.

One line of attack would be to specify a dynamic or static game of coalition formation, with specific assumptions on the game form. But these specific assumptions will translate into different predictions on which coalitions will form (which mergers will take place). Thus using a specific model, one could typically obtain significantly different predictions then using another related model, and structural estimation of each of these models is likely to lead to very different conclusions because auxiliary assumptions, such as those related to the order in which offers are made and how different equilibria are selected, will impact implications and inference.[10]

[9] On the theory side, the literature on robust comparative statics, which provides qualitative predictions for a range of models, might be one useful direction. Such robust comparative statics can be obtained for environments that can be represented as supermodular games (Milgrom and Roberts, 1994, Vives, 1990) or for those that can be represented as aggregative games (Acemoglu and Jensen, 2009).

[10] Acemoglu, Egorov and Sonin (2009) consider a class of dynamic coalition formation games, where "auxiliary"

Instead, Weese adopts a different approach, more in line with the type of conversation between theory and empirics suggested here: he specifies a general hedonic coalitional game, where municipality preferences depend on a few characteristics (average income, distance, etc.), and given these preferences he focuses on the Von Neumann-Morgenstern stable set.[11] This set is not a singleton, thus the model, equipped with this equilibrium/solution concept, does not make a unique prediction. Nevertheless, it rules out a large set of mergers given underlying preferences, and thus specifies a set of moment conditions that can be used for estimation. Crucially, these are not all of the moment conditions that will follow from a model that would make additional auxiliary assumptions to specify, say, a unique equilibrium merger structure for every value of the underlying parameter vector. Interestingly, in this case, Weese is able to estimate the underlying preferences and conduct counterfactual policy analysis.[12] His estimates show that a different government policy would have led to better (merger) outcomes, and that, somewhat surprisingly, allowing side transfers would have disadvantaged poor municipalities (because their willingness to merge with richer municipalities would have given the latter significant bargaining power).

Overall, one important direction for applied theory work (in economics in general and in development economics) would be to carefully delineate which sets of predictions are more robust, and thus (policy) invariant to auxiliary assumptions, and develop empirical strategies and methods of conducting counterfactual experiments that exploit these more robust implications. In the meantime, this discussion also highlights that if structural estimation relies on all of the moment conditions implied by a (simple) model, this may lead to misleading results. Making good use of theory does not mean taking all of the predictions of a model seriously, but to make use of the key and robust implications from theoretical models to specify and estimate structural parameters. It thus also requires us to be cognizant of which dimensions of a model are adopted just for simplicity, tractability and convenience.

4 The Centrality of General Equilibrium

General equilibrium analysis creates several challenges for theory and empirical work. The theoretical challenges stem from the fact that, without imposing additional assumptions, gen-

assumptions, for example, those concerning the order in which offers are made and acceptance and voting procedures, do not affect the set of predictions. This provides another example of a strategy to obtain "robust implications," even though such results can only be obtained under certain, somewhat restrictive, assumptions.

[11] See, among others, Pakes (2008) and Tamer (2003), for different approaches to the estimation of models with multiple equilibria.

[12] For example, one estimation strategy is to assign uniform probabilities to all coalition structures in the Von Neumann-Morgenstern set and then to estimate the underlying parameters by quasi-maximum likelihood.

eral equilibrium interactions can often reverse sensible partial equilibrium conclusions. The empirical challenges are related but distinct. First, economic theory becomes more central in (general) equilibrium situations because we need to understand how individual behavior determines (aggregate) equilibrium behavior. Second, most empirical strategies do not directly estimate general equilibrium effects, which may be central for counterfactual and policy analysis.

More specifically, the bulk of empirical work using microdata, particularly in development economics, engages in "partial equilibrium" comparisons. For example, one could estimate the returns to schooling by looking at a small group of individuals who are induced to remain in school for longer, and comparing them to other individuals in the same market (thus facing the same prices) who have dropped out of school. This is a partial equilibrium exercise in the sense that the source of variation in the schooling decisions does not generate changes in market prices. But for many of the questions relevant for development economics, we wish to think of counterfactuals in which a large fraction of the population acquires more schooling. In this case, it is no longer plausible to assume that prices will necessarily remain constant. Imperfect substitution between different skill levels will typically imply that a large increase in the schooling may reduce the return to schooling. A clear example comes from Angrist (1995), who shows that the large school building programs in the occupied territories in Palestine led to a sharp drop in the skill premium.

Similar considerations apply, though perhaps more subtly, when thinking of the importance of credit market imperfections. For example, Banerjee and Duflo (2005) survey a large body of evidence that small and medium-sized businesses in less-developed economies are credit constrained and an extension of credit to these businesses will make them increase production. While this evidence is convincing, part of the reason why we would like to know the effect of credit expansions is to undertake counterfactual experiments in which credit markets are improved so that there is more credit available to all or a large fraction of firms. Such large-scale counterfactual experiments are important both to develop a better understanding of the causes of low output and low income in less-developed economies and to evaluate potential policy interventions. But once again, a large fraction of firms having access to better credit is a general equilibrium change. Can we use the parameter estimates from the partial equilibrium analysis for this general equilibrium counterfactual? The answer is unclear and depends on economic theory. For example, if, in the partial equilibrium empirical exercises, firms that have access to better credit expand production *at the expense* of other firms that do not have access to credit (i.e., by "business stealing"), then total output may not increase as much or even at all when additional credit becomes available to a large fraction of firms. One could thus imagine a situation in which partial equilibrium estimates of relaxing credit constraints are large, while the general equilibrium effects would be small.[13]

Other important examples of general equilibrium effects include the potential response of technology to large-scale changes. Partial equilibrium empirical exercises hold technology as well as factor prices constant. The recent literature on technological change emphasizes that both the speed of technology adoption and the bias of technologies will respond to factor proportions, prices and regulation. For example, in evaluating the effect of trade opening, one could not simply rely on partial equilibrium estimates derived from firm level variation in access to foreign markets, since trade opening is a general equilibrium change that will impact technology choices.[14]

I now elaborate on this point in the context of the effect of life expectancy (and health) on economic growth. A large microeconometric literature shows that healthier individuals are more productive.[15] On the basis of this, we would expect an increase in the life expectancy of the workforce to lead to greater aggregate productivity. But once again, one should take general equilibrium effects into account, since an increase in life expectancy also increases population and thus changes factor proportions and factor prices in the economy. Following Acemoglu and Johnson (2007), henceforth AJ, consider a simple form of the neoclassical growth model, where economy i has a constant returns to scale aggregate production function

$$Y_{it} = (A_{it}H_{it})^{\alpha} K_{it}^{\beta} L_{it}^{1-\alpha-\beta}, \tag{4}$$

where $\alpha + \beta \leq 1$, K_{it} denotes capital, L_{it} denotes the supply of land, and H_{it} is the effective units of labor given by $H_{it} = h_{it}N_{it}$, where N_{it} is total population (and employment), while h_{it} is human capital per person. Normalize $L_{it} = L_i = 1$ for all i and t. Suppose, somewhat heroically that the multiple dimensions of health can all be captured by life expectancy at birth, which may increase output (per capita) by affecting total factor productivity (TFP) and human capital, and impose the following isoelastic relationships:

$$A_{it} = \chi_{it}\bar{A}_i X_{it}^{\gamma},\ h_{it} = \bar{h}_i X_{it}^{\eta}, \text{ and } N_{it} = \bar{N}_i X_{it}^{\lambda}. \tag{5}$$

[13] See also Townsend (2009) for a complementary discussion of the role of general equilibrium analysis in development economics, with special emphasis on credit market issues; Heckman, Lochner and Taber (1998) for a discussion of general equilibrium issues in the analysis of the effects of technology on wage inequality; and Duflo (2004a) for a discussion of other difficulties in "scaling up" policy interventions evaluated using microdata.

[14] See, for example, Acemoglu (2002, 2007) on the endogenous response of technologies to factor supplies and regulations, and how this might have implications for economic development.

[15] See, among others, Behrman and Rosenzweig (2004), Schultz (2002), and Straus and Thomas (1998).

where X_{it} is life expectancy (at birth) in country i at time t, and $\bar{A}_i$ and $\bar{h}_i$ are the baseline differences across countries, and χ_{it} is a random productivity shock. The last relationship in (5) captures the fact that greater life expectancy will also lead to greater population (both directly, and also potentially indirectly by increasing total births as more women live to childbearing age).

Now imagine the effect of a change in life expectancy from some baseline value X_{it_0} at t_0 to a new value X_{it_1}at time t_1. Suppose, for simplicity, that the total capital stock remains fixed at some $\bar{K}_{it_0}$ (see AJ, for the case in which the capital stock adjusts). Then, substituting (5) into (4) and taking logs, we obtain the following log-linear relationship between log life expectancy, $x_{it} \equiv \log X_{it}$, and log income per capita, $y_{it} \equiv \log (Y_{it}/N_{it})$:

$$y_{it} = \beta \log \bar{K}_{it_0} + \alpha \log \bar{A}_i + \alpha \log \bar{h}_i - (1-\alpha) \log \bar{N}_i + (\alpha (\gamma + \eta) - (1-\alpha) \lambda) x_{it} + \varepsilon_{it}, \quad (6)$$

for $t = t_0, t_1$, where $\varepsilon_{it} \equiv \log \chi_{it}$ is a stochastic term resulting from random variations in productivity. This equation shows that even though the effects of greater life expectancy on productivity and human capital may be positive, an increase in life expectancy may reduce income per capita if the positive effects of health on TFP and human capital, measured by $\alpha (\gamma + \eta)$, are exceeded by the potential negative effects arising from the increase in population because of fixed land and capital supply, $(1-\alpha) \lambda$.

How could one investigate whether these general equilibrium effects are important? One approach is to use information from other sources in order to "calibrate" the values of the parameters and then combine this with micro estimates of the effect of health and life expectancy on individual outcomes.[16] This approach would be successful when we can have confidence in the calibration exercise. A second approach is to use cross-country variation, even though such variation will be affected by several potentially omitted factors. AJ adopt the second approach. Then, representing the factors that are invariant by country and time by fixed effects, equation (6) can be written as

$$y_{it} = \pi x_{it} + \zeta_i + \mu_t + \varepsilon_{it}. \quad (7)$$

The parameter of interest, π, measures the relationship between log income per capita and log life expectancy. Given (6), we can interpret π as corresponding to $(\alpha (\gamma + \eta) - (1-\alpha) \lambda)$,

[16] This is the approach advocated by Banerjee and Duflo (2005) and used by Weil (2007) in the context of health and economic development and by Heckman, Lochner and Taber (1998) in the context of the relationship between technology and wage inequality. A third, perhaps most promising, approach is to combine microdata with regional variation to estimate partial and general equilibrium effects simultaneously. This approach is adopted and developed in Acemoglu and Angrist (2000) to estimate human capital externalities exploiting individual-level differences in schooling together with state-wide differences in average schooling. A similar approach is used in Duflo (2004b) on Indonesian data. I will not discuss this approach further because of space constraints.

which provides us with the potential way of interpreting estimates from (7) in light of theory. While (7) can be estimated by OLS, this is likely to lead to biased estimates of π, since societies that are successful in solving economic and institutional problems to achieve higher growth are also likely to be able to provide better public health and other measures in order to improve life expectancy, and also the increase in income per capita is likely to lead to a mechanical improvement in life expectancy. For example, we could have that

$$x_{it} = \bar{\zeta}_i + \bar{\mu}_t + \mathbf{Z}'_{it}\boldsymbol{\beta} + \phi A_{it} + \upsilon_{it}, \tag{8}$$

where υ_{it} is a random shock, $\mathbf{Z}_{it}$ denotes a vector of other determinants, and the (presumably positive) parameter ϕ measures the impact of overall productivity on health and life expectancy. Equations (6)-(8) imply that x_{it} and ε_{it} in (7) will be (positively) correlated, making OLS estimates upwardly biased.

To overcome this problem, AJ adopt an instrumental-variable strategy, exploiting the international epidemiological transition, whereby important drugs and chemicals were invented and spread around the world, reducing mortality from major diseases. They construct the variable *predicted mortality* as:

$$M_{it} = \sum_{d \in \mathcal{D}} \left((1 - I_{dt}) M_{dit_0} + I_{dt} M_{dFt} \right), \tag{9}$$

where M_{dit} denotes mortality in country i from disease d at time t, I_{dt} is a dummy for intervention for disease d at time t (it is equal to 1 for all dates after the intervention), and $\mathcal{D}$ denotes the set of the most important 15 infectious diseases at the time. M_{dit_0} refers to the pre-intervention (date t_0, or in practice 1940) mortality from disease d in the same units, while M_{dFt} is the mortality rate from disease d at the *health frontier* of the world at time t. Predicted mortality, M_{it}, thus uses a country's initial mortality rate from each of the 15 diseases until there is a global intervention, and after the global intervention, the mortality rate from the disease in question declines to the frontier mortality rate. Predicted mortality, M_{it}, is then used is an instrument for life expectancy in the estimation of (7), with the first-stage relationship given by

$$x_{it} = \psi M_{it} + \tilde{\zeta}_i + \tilde{\mu}_t + u_{it}. \tag{10}$$

The key exclusion restriction for the estimation strategy is $\text{Cov}(M_{it}, \varepsilon_{it}) = 0$, where recall that ε_{it} is the error term in (6) or (7). Note that both the second stage and the first stage (the exclusion restriction) are motivated by theory. The second stage is derived from the neoclassical growth model, while the first stage (and thus the exclusion restriction $\text{Cov}(M_{it}, \varepsilon_{it}) = 0$) is predicated on the theory that global intervention for a particular disease will affect mortality in a country in proportion with the number of initial deaths from the disease in question in that country, and more importantly, that baseline levels of mortality from different diseases do not have a direct effect on future income beyond their impact working through future life expectancy and health conditions.[17] For example, if we assume (8), then $\text{Cov}(M_{it}, \varepsilon_{it}) = 0$ will apply provided that M_{it} is orthogonal to A_{it}. Given the construction of predicted mortality, M_{it}, a sufficient (though not necessary) condition for this is that A_{it} should be serially uncorrelated (conditional on $\bar{A}_i$). In addition, the model outlined so far is also useful as it clarifies the conditions under which the exclusion restriction $\text{Cov}(M_{it}, \varepsilon_{it}) = 0$ may not hold. This may be, for example, because $\sum_{d \in \mathcal{D}} M_{dit_0}$ may be correlated with A_{it_1}, even after we control for country effects and condition on A_{it_0} or on country fixed effects.

AJ do not estimate the full model implied by equations (4)-(10). Many of the relationships implied by this model are subsumed in fixed effects. Instead, the estimation takes the form of a simple two-stage least squares (2SLS) exercise. This is again in the spirit of focusing only on the set of implications that are central to the hypothesis at hand. For example, for questions that are being investigated in this context it is not important whether the aggregate production function can indeed be represented in a Cobb-Douglas form, and that the impact of life expectancy on all variables takes an iso-elastic form. Instead, the key implications here relate to the potential effect of life expectancy on population and GDP (working through productivity effects as well as by increasing the labor force), and the assumption that variations in life expectancy induced by predicted mortality, defined in equation (9), are unrelated to other factors potentially affecting population and income.

It is also useful to note the surprising finding in AJ, that despite the well-established positive micro estimates of the effect of health on productivity, in general equilibrium the effect on income per capita appears to be negative, most likely because the improvements in life expectancy were associated with very large increase in population. While this conclusion comes with several caveats, not least because the negative estimates are often quite large and come from a specific episode (during which mortality rates may have declined unusually rapidly relative to morbidity rates), it illustrates the possibility that general equilibrium empirical conclusions can be quite different from partial equilibrium ones.[18] It reiterates the importance

[17] AJ provide evidence consistent with this exclusion restriction. For example, prior to 1940 predicted mortality does not predict future income or population growth, which is consistent with the notion that past levels of life expectancy do not have a direct effect on future growth.

[18] The conclusions may also depend on the fact that AJ focus on changes in health largely (though not solely) associated with mortality. Bleakley (2007), focusing on changes related to morbidity, obtains different results.

of incorporating general equilibrium considerations for conducting counterfactual exercises concerning the effects of large changes in variables such as schooling, health conditions or access to credit on income per capita or other developmental outcomes.

5 Fallacy of "Instruments" Without Theory

Let us return to the effect of life expectancy on income discussed in the previous section. The estimating equation used there, (7), could alternatively be written as $\Delta y_i = \pi \Delta x_i + \Delta\mu + \Delta\varepsilon_i$, where Δ denotes the difference between dates t_0 and t_1 (for example, in AJ 's estimation 1940 and 1980 or 2000). Estimating this differenced equation would lead to identical results to the estimation of (7).

The literature on the cross-country relationship between health and income sometimes estimates equations of the form

$$\Delta y_i = \alpha y_{it_0} + \beta x_{it_0} + \pi \Delta x_i + \Delta\mu + \Delta\varepsilon_i, \tag{11}$$

where x_{it_0} is the initial (1940) level of log life expectancy and y_{it_0} is the initial (1940) level of income per capita. It may then be tempting to estimate (11) using a similar 2SLS strategy, in particular, using predicted mortality introduced in the previous section as an instrument (either by treating the initial levels x_{it_0} and y_{it_0} as exogenous or by instrumenting for them using their lagged values or geographic controls). The reasoning would be that if predicted mortality is a good instrument for estimating (7), then it must be a good instrument for estimating (11). This is the "fallacy" of instruments without theory.

As noted in the previous section, we need theoretical reasoning to justify both the second and the first stages. The second stage in AJ was justified using the neoclassical growth model. A second stage equation such as the one in (11) could be justified using a different and related theory, for example, one where life expectancy in 1940 would have had a direct effect on productivity in 1980 or 2000 (40 or 60 years thereafter). While I do not find this to be a compelling model, as long as the reasoning is spelled out there is nothing logically wrong in using such a model. However, the estimation strategy also requires a theoretical justification for the first stage, or the exclusion restriction (embedded in the assumption above that $\text{Cov}(M_{it}, \varepsilon_{it}) = 0$). As highlighted in the previous section, this exclusion restriction could only make sense if the baseline level of mortality does not have a direct effect on future growth. If it did, the assumption that $\text{Cov}(M_{it}, \varepsilon_{it}) = 0$ would be directly violated.[19] But this implies

[19] This may or may not be a valid assumption in general. As noted in footnote 17, AJ provide evidence to substantiate this assumption and increase the plausibility of this exclusion restriction.

that the theoretical argument underlying the exclusion restriction cannot be *logically* combined with a model that takes the form (11), even though it is entirely consistent with (7). We thus have a simple example where one needs to consider the theoretical foundations of the entire set of economic relations (or more explicitly, the first and second stages) together in order not to make logical errors.

The broader point is that one cannot think of "instruments" without theory. What makes a particular variable a valid instrument is a (theoretical) justification for the entire set of economic relationships being estimated, that is, both the specification of the structural parameters and the corresponding first stages and exclusion restrictions. When either of these changes, the validity of the instrument may be jeopardized. This, of course, should not be surprising in view of the discussion in Section 2: the plausibility of structural parameters, and thus their estimation, crucially depends on using economic theory to justify the underlying model. In this light, it should be clear that there cannot be "instruments" without theory.[20]

6 No Development without Political Economy

There is increasing recognition that institutional and political economy factors are central to economic development. Many problems of development result from barriers to the adoption of new technologies, lack of property rights over land, labor and businesses, and policies distorting prices and incentives. These institutions and policies are not in place because of a lack of understanding of economic principles on the part of policymakers, but because policymakers introduce or maintain such policies in order to remain in power or to be able to enrich themselves, or because politically powerful elites oppose the entry of rivals, the introduction of new technologies, or improvements in the property rights of their workers or competitors (see, e.g., Acemoglu, Johnson and Robinson, 2005). But this perspective implies that theory again becomes particularly important in evaluating (or framing) possible effects of large-scale policy interventions, and as with those that do not take account of general equilibrium effects,

[20] Some well-known and successful implementations of 2SLS methodology are sometimes interpreted as being relatively "a-theoretical". For example, Angrist and Krueger (1991) estimate the return to schooling using a Mincerian wage regression and the quarter of birth of an individual as instrument for schooling. I believe that to say that this paper does not make use of economic theory and does not specify a structural relationship would be incorrect. On the contrary, the model that Angrist and Krueger estimate is the log-linear Mincer equation, which has good theoretical foundations (e.g., Acemoglu, 2009, chapter 9), and thus the Mincerian returns to schooling that they estimate, under some assumptions, can be used for policy analysis or for inferring the return to schooling in other environments (see Card, 1999). Moreover, the first-stage relationship that they exploit, that quarter of birth should influence whether a high school student can drop out, follows from the laws in place, and is both plausible and does not generate any contradiction with the assumptions made for the second-stage (structural) equation.

counterfactual analyses that ignore political economy factors may give misleading answers. In this case, convincing micro or even macro (general equilibrium) evidence about the effects of a particular policy change on economic outcomes is not in itself sufficient to gauge what the implications will be when such a policy is encouraged or implemented.

The experience of Ghana with exchange rate policy under Prime Minister Kofi Busia in 1971 provides a sharp illustration. Busia pursued expansionary economic policies after coming to power in 1969, and maintained various price controls and an overvalued exchange rate. But Ghana was soon suffering from a series of balance of payments crises and foreign exchange shortages. Faced with these crises, Busia signed an agreement with the IMF on December 27, 1971, which included a massive devaluation of the currency. A few days following the announcement of the devaluation, Busia was overthrown by the military led by Lt. Col. Acheampong, who immediately reversed the devaluation (see, e.g., Herbst, 1993, or Boafo-Arthur, 1999). There was little doubt that devaluation was good economics in Ghana. But it was not good politics. State controls over prices, wages, marketing boards and exchange rates were an important part of the patronage network, and any politician who lost the support of this network was susceptible both at the polls and against the military. Busia suffered this fate.

This episode illustrates a general point that when political economy factors are important, evidence on the economic effects of large-scale policy changes under a given set of political conditions is not sufficient to forecast their impact on the economy and society. This principle does not just apply to exchange rate policy. For example, the fact that increasing availability of credit to firms would increase aggregate output given all other policies does not imply that an actual reform of the credit market will necessarily work. For example, Haber and Perotti (2008) argue and provide evidence that limiting access to finance is a powerful tool in the hands of political and economic elites for restricting entry into lucrative businesses. So reforms of credit markets will often face political opposition from powerful parties, and even when they are implemented, this implementation may be imperfect or accompanied by other policies aimed at nullifying the effects of the reform. This type of endogenous policy response undermining the objectives of a reform is termed the *seesaw effect* in Acemoglu, Johnson, Robinson and Querubín (2008), who provide evidence that the reforms aimed at reducing inflation by granting independence to the central bank typically do not work in societies with weak institutions and sometimes trigger other policy responses, for example, larger government deficits, to undo the reduction in the ability of the government to provide favors to politically powerful groups.

So how should empirical research in economic development take political economy into account? As with the questions of drawing robust predictions from theory for empirical work or dealing with general equilibrium effects, there is, once again, no simple recipe or clear answer. A first step would be to use empirical work to better understand the role of political economy factors in development. This type of empirical political economy of development is relatively new. The first generation of work focused on cross-country variation. There is now a number of papers using microdata in order to shed light on how political economy in less-developed economies works, though given the importance of political economy for the problems of development, it is surprising how few papers investigate important political economy channels using microdata and careful empirical strategies. I now discuss a few of these papers to give a sense of what types of avenues are available.

An important recent paper this area is Goldstein and Udry (2008). Existing micro and macro evidence show that property rights over land are important in encouraging investments and the efficient use of land and labor (e.g., Besley, 1995, Field, 2007). But why are property rights imperfect? Goldstein and Udry provide an interesting answer from southern Ghana. The major investment to increase agricultural productivity in this area is fallowing. However, the amount of fallowing is generally insufficient. Goldstein and Udry (2008) show that this is related to the insecure property rights, increasing the risk that fallowed plots will be confiscated and reallocated by powerful chiefs and other local politicians. Those with sufficient political power, who presumably do not face such risks, choose significantly higher levels of fallowing. This illustrates the role of local power structures in villages in shaping the security of property rights and incentives for investment.

Two recent papers by Ferraz and Finan investigate some of the key implications of political agency theory (e.g., Besley, 2006). Ferraz and Finan (2008) use audit reports from an anti-corruption program in Brazil to estimate the effect of electoral accountability on corruption and misappropriation of funds by politicians. They find that mayors who cannot get reelected because of term limits are significantly more corrupt and misappropriate 27% more resources than mayors with reelection incentives. They also show that, consistent with theory, these effects are stronger when voters have access to less information and when judicial punishment against corruption is weaker. In a related paper, Ferraz and Finan (2009) study the effects of politician salaries on politician behavior and quality of public services. They exploit a discontinuity in the salaries of local politicians across Brazilian municipalities, resulting from a constitutional amendment imposing salary caps depending on the size of municipal population. Using regression discontinuity techniques, they find that greater salaries are associated with greater competition among potential candidates, and the quality of the elected legislatures measured by education or experience improved. More importantly, higher salaries are also associated with improvements in various dimensions of politician performance.

A recent paper by Dell (2009) also uses a regression discontinuity design, but to answer a very different political economy question. A major question in assessing the effects of institutions and political economy factors is the extent to which certain important institutions have long-run effects. Acemoglu, Johnson and Robinson (2005) summarize several cross-country studies suggesting that there are such persistent effects of colonial institutions and other critical events such as the separation of the Koreas. Controlling for confounding factors is often difficult in cross-country studies and the exact mechanism leading to persistent effects is often impossible to pinpoint. Dell focuses on the potential effects of the forced labor system used by the Spanish colonial government in Peru and Bolivia, the *mita*. This system, which forced a large fraction of the adult male population of villages in the catchment areas of the Potosi silver and Huancavelica mercury mines to work in these mines, was used extensively in the 16th century, and was abolished in 1812. The presence of a catchment area implies another form of discontinuity, whereby those inside and outside the boundary of the catchment area were subject to different labor regulations. Using a regression discontinuity design, Dell finds that areas subjected to forced labor more than 200 years ago now have about one third lower household equivalent consumption. The available data also allow her to investigate some potential mechanisms for this very large and persistent effect, which appears to be related to lack of public goods in areas subject to forced labor, which in turn may be related to the policies of the Spanish governments to limit competition for labor in the catchment areas from private landholders and businesses. Ferraz and Finan (2009) and Dell (2009) thus show how regression discontinuity techniques can be used for investigating different political economy questions.

Finally, again related to the issue of coercion, Naidu and Yuchtman (2009) investigate how the ability of employers to imprison or fine an employee for breach of contract under the Master and Servant Acts, which remained in effect in Britain until 1875, affected labor market relations. They provide empirical evidence that employers made extensive use of their coercive ability under the law, and as a consequence, labor demand shocks were largely met by using increased persecutions for contract breach rather than higher wages.

Overall, the above-mentioned papers, though distinct in methodology and scope, show how microdata and regional variation in institutions and laws can be used to shed light on the role of political economy factors in development. To the extent that, as argued above, political economy factors are major drivers of key aspects of the process of development, empirical work in development economics must pay more attention to, and build a more systematic understanding of, political economy; it must also study how different counterfactual and policy experiments will interact with or be resisted by political factors, and may sometimes unleash political changes of potential import for future economic outcomes.

7 Concluding Remarks

This paper discussed the role of economic theory in empirical work in development economics. A key objective of empirical work in development economics is to discriminate between theories about the causes of economic growth and to conduct counterfactual analysis to build a systematic understanding of how an economy will respond to large changes in factor supplies, technology or policy. Theory plays a major role in specifying the relevant structural parameters that will affect the outcomes of counterfactual exercises, in guiding us towards more appropriate empirical strategies, and in interpreting estimates obtained from different empirical strategies. Without theoretical guidance, judging the reliability of counterfactual experiments, the counterfactual validity of inferences, or the validity of identification strategies and more specifically of instruments would be difficult or impossible. Nevertheless, this certainly does not amount to taking all predictions of simple models seriously, either for testing the theories or for counterfactual exercises, since most models include several assumptions for tractability, convenience or simply as auxiliary assumptions not central to their main focus. An important direction for future applied theory must be to more clearly delineate which predictions should be considered as "robust" and central, and which are auxiliary and should not be taken empirically seriously.

I emphasized two aspects of theory, which ought to be central to most counterfactual analysis in development economics: general equilibrium and political economy considerations. I illustrated how even when armed with reliable micro estimates from partial equilibrium analysis, we may be unable to arrive at meaningful conclusions on the impacts of large policy changes without factoring in general equilibrium interactions and political economy responses. I discussed several recent papers illustrating the role of general equilibrium effects or estimating the importance of political economy in various different areas related to development.

Needless to say, an essay of this form is highly subjective and reflects the views of the author, some of them idiosyncratic. My hope is not that everyone will agree with the views expressed here (though I would be happy if at least some did), but rather that the ideas and questions raised in this essay will help others formulate clearer approaches to using economic theory in empirical work in development economics and to incorporating insights from general equilibrium and political economy in the study of development.

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