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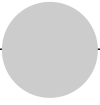
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Comment on Chapters 9 and 10

Maitreesh Ghatak

A common premise underlying many antipoverty policies is that current interventions can have long-run effects, outlasting the duration of the policy. This in turn reflects the fact that a notion of persistence of poverty underlies much of development economics. This means there is some underlying positive feedback mechanism—with a suitable push, the poor will be on a self-sustaining trajectory of development. If poverty was a transitional and a largely self-correcting phenomenon, such as life cycle poverty, then the effect of these policies would not be long lasting unless they are permanently in place. This is as we would expect from standard growth models (e.g., the Solow model) that feature convergence of an individual to a unique steady state within a reasonable time frame.

If poverty is persistent, then the scope for policies to have outcomes beyond their duration becomes possible. There are two broad class of mechanisms for it. The first relates to poverty traps where two individuals who, except for income or wealth, are identical can end up with very different steady-state income and wealth levels.¹ The second class of mechanisms relates to what in the growth literature is known as the notion of conditional convergence, namely, the poor face unfavorable productivity parameters and so while they converge to a unique steady state, that involves income and wealth levels that

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1. See Azariadis (1996), Barrett and Carter (2013), Ghatak (2015), and Kraay and McKenzie (2014) for reviews of the literature on poverty traps.

are below the poverty line.² In this world, two individuals who are identical in all respects except initial income and wealth will converge to the same income and wealth levels in the long run, and so they are not trapped in poverty in a narrow sense, but poverty can be persistent as the steady-state income or wealth levels are below the poverty line since their productivity is low (e.g., with geographic poverty traps).

I will discuss three recent empirical papers on the long-run impact of specific transfer policies aimed at the poor. The nature of the transfer programs, the settings, and the time horizons are quite different, which is very helpful in trying to absorb the lessons of these kinds of programs. Two of these papers are included in this volume—Araujo, Bosch, and Schady (chapter 10) and Macours and Vakis (chapter 9). The third one was presented in the conference that preceded the publication of this volume (Bandiera et al. 2017).

The plan of this comment is as follows. In the next section I will sketch a conceptual framework used to interpret the findings based on Ghatak (2015). In the third section I will discuss the three individual papers and draw out the general lessons from them. In my concluding remarks I offer some thoughts about how to combine theory and empirical work to provide a better understanding of “what works best where” in terms of the effectiveness of alternative strategies aimed at overcoming the persistence of poverty.

Conceptual Framework

Suppose that the current income of an individual, y_t , depends on the existing stock of capital (or wealth) k_t via a production function $y_t = Af(k_t)$ where A is a productivity parameter that depends on other factors, for example, the quality of institutions, infrastructure, and availability of complementary inputs. The capital stock evolves over time through investments the individual makes, i_t (say, investment), but that in turn depends on y_t or k_t via the budget constraint of the individual and his or her ability to borrow or save. For simplicity assume, as in the textbook Solow model, that individuals save a constant fraction s of their incomes and capital depreciates completely. Therefore, the transition equation is $k_{t+1} = sAf(k_t)$.

If capital markets are perfect, then an individual can borrow or lend k at a given (gross) interest rate r (which stays constant for this individual, for simplicity, because, say, interest rates are fixed by the international capital market), and will choose the efficient level of k defined by $Af'(k) = r$ irrespec-

2. Yet another mechanism for persistence of poverty could be that there is convergence to a unique steady state, but it is slow. Typically, this would happen under similar parameters that would also cause convergence to a steady state involving low levels of income and wealth, such as low productivity parameters and low saving rates.

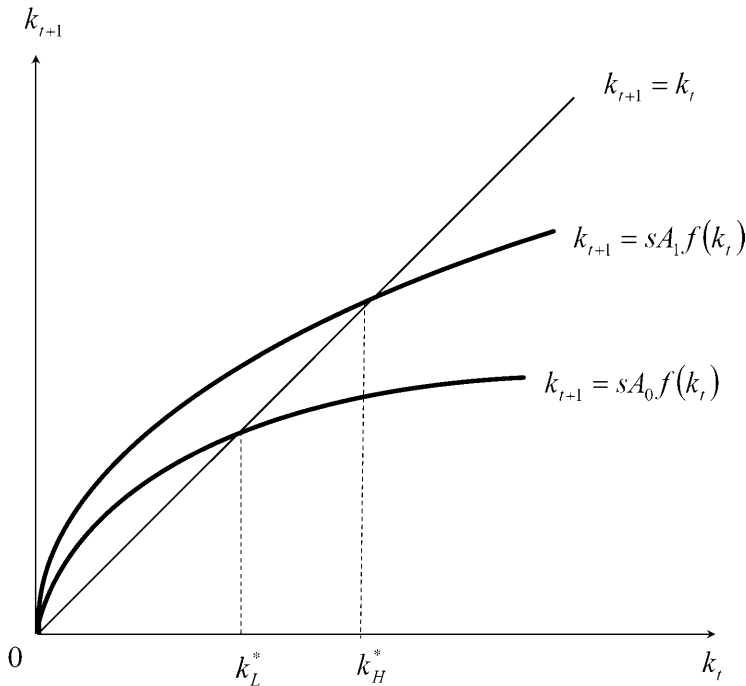


Fig. 9/10C.1 Conditional convergence in the Solow model

tive of how much k he owns. Let $\pi = Af(k^*) - rk^*$ be the maximized value of profit. Then the transition equation is $k_{t+1} = s(\pi + rk_t)$.

If either the production technology is convex, in which case the transition equation will be a concave function, or if capital markets are perfect, so that the transition equation is effectively linear (and hence, a concave function), the individual would converge to a unique stable steady state. However, even if there is a unique and stable steady state, but the actual A is much lower than the potential A (say, A'), poverty can be persistent, which is the notion of conditional convergence. In figure 9/10C.1 the production technology is convex, but depending on the value of A (with $A_0 < A_1$), the individual reaches steady-state levels of wealth k_L^* and k_H^* that are quite different. Any policy that can change A from A_0 to A_1 can have a permanent effect on the steady-state income and wealth levels of the individual.

If, however, the production technology is subject to nonconvexities, then poverty traps can arise. Suppose there is a threshold level of capital, \underline{k} , such that $y_t = A_1 f(k_t)$ for $k \geq \underline{k}$ and $y_t = A_0 f(k_t)$ for $k \leq \underline{k}$, where $A_0 < A_1$. If individuals could borrow from a competitive credit market, they could overcome the indivisibility by directly borrowing the amount k_H^* . But if capital markets are imperfect in addition to the nonconvexity, then multiple stable

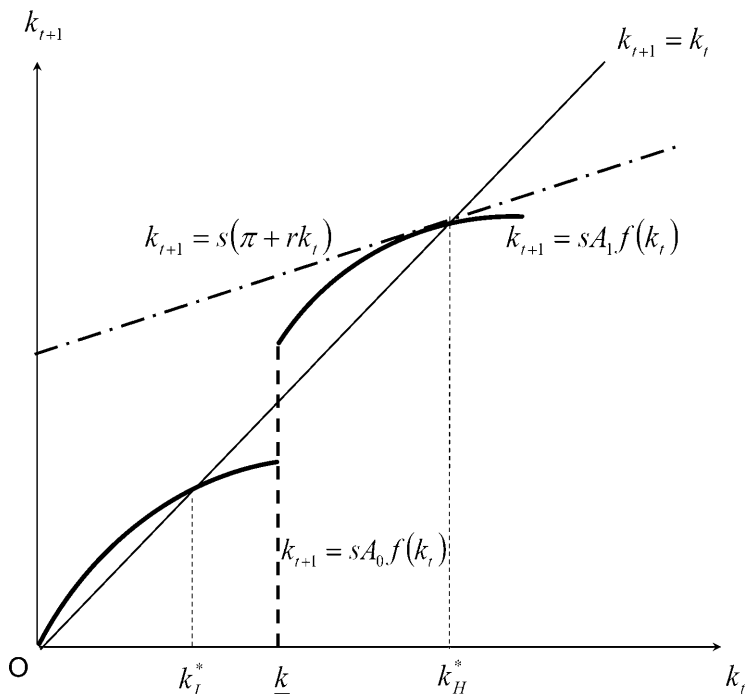


Fig. 9/10C.2 External frictions and poverty traps

steady states can arise as captured in figure 9/10C.2. In Ghatak (2015) I refer to this as an example of an “external frictions”-driven poverty trap. The poor here are just like the nonpoor in terms of their potential (that includes ability and preferences), but they operate with a tighter choice set, which is exacerbated by various frictions such as market failures as well as technological nonconvexities that make it disadvantageous to be operating at very low scales.

However, even if there are no frictions in the external environment (in particular, the technology is convex and credit markets are perfect in the current setting), poverty traps can arise if preferences display strong income effects. For example, in the current setting, the poor can have a saving rate s that is significantly lower than the nonpoor in a way that can generate multiple steady states. This is illustrated in figure 9/10C.3, where there is a threshold level of capital, \underline{k} (kept the same as in the previous case for comparability), such that the rate of saving is s_0 or s_1 with $s_0 < s_1$ and this affects the transition equation under autarchy as well as the one with perfect credit markets. The dichotomous savings rate is merely meant to convey the idea simply. In Ghatak (2015), there is a simple model that illustrates this more formally. Clearly, a more realistic framework with savings as a continuous

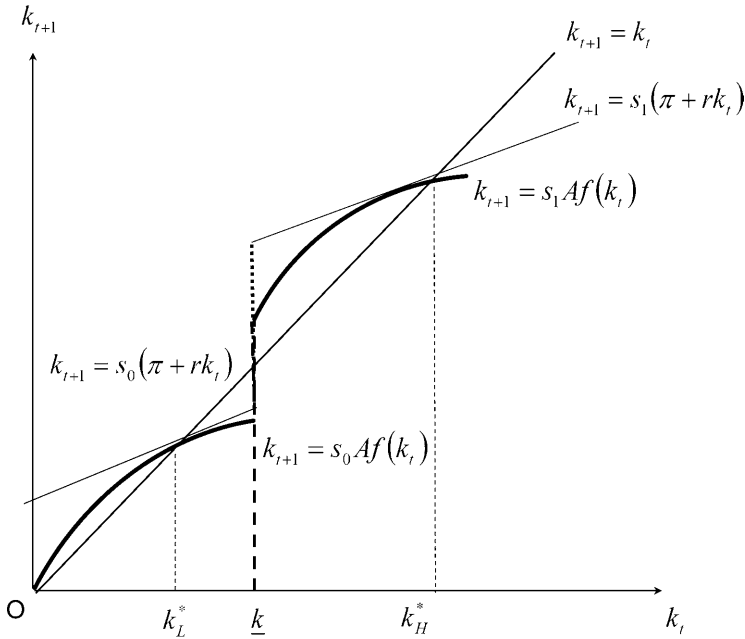


Fig. 9/10C.3 Choice under scarcity and poverty traps

function of income (or capital) will have a similar positive feedback feature that gives rise to a poverty trap.

In this view of poverty traps, even if there are no frictions in the external environment that policy can potentially try to “fix,” the poor make choices that are very different from the nonpoor in a way that can reinforce poverty. This can happen for a number alternative reasons: subsistence needs may rule out the feasibility of saving and investing money in health and education at high rates for low levels of income, the poor can discount the future more, or put less weight on the welfare of their children.

The advantage of this framework is that it helps us see that different mechanisms could be at work to find similar effects of a policy in reducing poverty in the long run, but at the same time, it provides some structure to offer ways to try to interpret the evidence and try to disentangle these alternative mechanisms. We use this framework because it connects directly with the classic growth models and helps us see clearly what kind of departures from these models could lead to poverty traps. It should be noted that there are many other potential mechanisms that could lead to poverty traps that are not directly captured in theoretical framework developed here. For example, poverty may make individuals more risk averse, thereby choosing projects that are low variance but also low mean. Also, it is possible that some behavioral biases are accentuated with poverty (see, e.g., Banerjee and Mullainathan 2010).

Evidence

I now turn to the discussion of the three recent empirical papers that study medium- to long-term effects of transfer programs to the poor mentioned in the introduction. All three papers study some form of cash/wealth transfer to the poor: a pure cash transfer (Araujo, Bosch, and Schady), a combination of transfer of livestock assets and skills (Bandiera et al.), and a conditional cash transfer (CCT) combined with exposure to social interaction with local leaders (Macours and Vakis). The first study has one of the longest horizons—they study the effect of cash transfers in Ecuador after a decade. The other two studies have shorter time horizons, but both look at effects of these programs even after the direct resource transfer was withdrawn. Therefore, all three studies are particularly well placed to study the effect on such policies on persistence of poverty.

Araujo, Bosch, and Schady study whether cash transfers can help households escape poverty traps, that is, the long-term effects of cash transfers. The key mechanism they have in mind is whether transfers had positive effects on various measures of human capital accumulation by relaxing the budget constraints of liquidity constrained households, enabling them to invest in their children's human capital. They use two data sets and empirical strategies to look at the medium- and long-term effects of this cash transfer program in Ecuador on human capital that started in 2003. They provide experimental evidence that used the fact that children under the age of six years were assigned to early and late treatment groups. Although the early treatment group received twice as much in total transfers, the long-term enrollment rates, grade attainment, and test scores are not significantly different between these two groups. They also note the fact that a poverty index was used to determine the eligibility for transfers. Using a regression discontinuity approach comparing children who were in the just-eligible and just-ineligible households in late childhood in terms of school attainment ten years later, the authors find that the transfers did increase secondary school completion but the effects are relatively small. Overall, the authors conclude that the effect of this cash transfer program on intergenerational persistence of poverty is likely to be modest.

Macours and Vakis analyze the medium-term impacts of a short-term CCT program in Nicaragua and, in particular, the role of social interactions of beneficiaries with local female leaders for sustaining the impact of this program. They build on their earlier work (Macours and Vakis 2014) that showed that social interactions with successful leaders substantially increased program impacts on nutritional and educational investments while the program was operating. In this chapter, they use data collected two years after the program ended to show that these social multiplier effects persisted to a remarkable degree. Two years after the transfers stopped, households who live in the proximity of successful leaders still show significantly higher

investments in both education and nutrition of their children. The earlier work showed social interactions with nearby leaders positively affected human capital and productive investments as well as the future-oriented attitudes of other beneficiaries during the program, but the worry was whether these shifts are sustainable—households might well quickly revert back to preprogram behavior when the transfers stop. The new results suggest that interactions with leaders may have affected other households' aspirations by setting good examples and sharing their experiences. They further show that interactions with leaders changed parents' beliefs or expectations about their children's educational and occupational potential, which can help explain the sustained higher levels of human capital investments.

The paper by Bandiera et al. (2017) provides experimental evidence on a nationwide one-off combination of transfer of livestock assets and skills transfer in Bangladesh to poor women in rural areas. They study the medium- to long-term effects of this program and, in particular, focus on the impact even after the direct resource transfer is withdrawn. Therefore, this study is particularly well suited to study the effect of antipoverty policies that involve income or asset transfers with or without some other "missing input" (in this case, training) on the persistence of poverty. Their study covers around 1,300 villages in rural Bangladesh and 21,000 households surveyed four times over a period of seven years. Poor women in their sample mostly engage in supplying casual labor, while wealthier women specialize in livestock rearing, which has higher hourly returns and more regular labor demand. Their study focused on the question of whether a one-off transfer set the poor women on a sustainable trajectory out of poverty by allowing them to switch from supplying labor to the more remunerative activity of livestock rearing. They found that the treatment group saved more (a ninefold increase after two and four years) and accumulated more assets over time, leading to larger gains, which is consistent with the mechanism of poverty traps. These gains are not at the expense of reduction in overall labor supply. Rather, the program leverages idle capacity with the average beneficiary working 22 percent more hours and earning 37 percent more.

The key question that arises in the Araujo, Bosch, and Schady study is why this transfer program, one of the largest cash transfer programs in proportional terms in Latin America, was not very effective. There are several possible explanations. It could be because the sums were small relative to need (e.g., private schools), or the lack of conditionality (even though the households were encouraged to spend transfer income on children), or the lack of combination of this program with other complementary interventions (e.g., improvement in schools). Another possible explanation of the results of Araujo, Bosch, and Schady is that they lack a true control group. If any intervention yielded an effect, whether early or late, then the difference *between* the two treatment groups would be insignificant even if the

difference between either treatment group with a *no-treatment* group might be large.³

What the paper by Macours and Vakis shows is that the combination of shifting beliefs and resources can be effective, and the effects long lasting. Ideally, we would like to see the marginal contribution of individual inputs, that is, beliefs and resources. In particular, in their study shifting aspirations is like shifting expected returns, and is equivalent to a one-shot change in the productivity parameter A . This alone can be effective to reduce the persistence of poverty as in the conditional-convergence-like argument discussed earlier. It is similar in spirit to Jensen (2010), who found school completion rates improved in the Dominican Republic when 8th grade boys were provided a correct measure of returns to secondary school, which was higher than the perceived rate of returns in the baseline population. Therefore, while the study cannot separate out a poverty trap mechanism from one that involves a one-time shift in the productivity parameter, it provides strong evidence on the ability of temporary policies to have long-term effects in reducing the persistence of poverty, and also suggests (though cannot demonstrate) that a combination of different interventions is likely to be particularly effective compared to individual policies.

While Bandiera et al. provides strongly suggestive evidence on poverty traps, several questions remain. Since they study the effect of the combination of asset transfer and training, we cannot answer the question whether an equivalent transfer of cash or access to credit in suitable terms might have worked as well. Like the Macours and Vakis study, it is possible that training shifted the productivity parameter, and that itself would be enough to push these women out of poverty. This seems unlikely in this context, as McKenzie and Woodruff (2014) review training business owners from a dozen randomized experiments and find little lasting impact on profits or sales. Still, having a sense of the marginal contributions of the asset transfer and the training would help us calibrate the design of the policy in contexts where the relative scarcity of these two inputs vary.

All three studies provide evidence on the ability of specific policies or their combinations to reduce the persistence of poverty in the long run. There are several common elements that these studies offer in terms of what we learn about the effectiveness of antipoverty policies.

First, a combination of policies seems to work well. Is it the case of classic complementarities that we are well familiar with from standard economic models, where $y = Af(x_1, x_2)$ and $f_{12} > 0$ where y is the outcome variable of interest and x_1 and x_2 are two different policies? Or do they reflect a multiple-friction or distortion view of the world and so just a big enough cash transfer or making credit available is not enough? For example, certain critical markets other than credit could be imperfect (e.g., training), individuals may not possess the best information about themselves or the external

3. I thank Christopher Barrett for this observation.

world (aspirations, self-belief), and given all of this, simply providing cash or capital may not be sufficient.

Second, together they make some progress toward disentangling different mechanisms that could lead to the persistence of poverty—namely, whether it is poverty traps or conditional convergence, and if it is poverty traps, whether it is due to external frictions or strong income effects that cause the poor to insufficiently save and invest in their own or their children’s future. However, a lot remains to be understood as the findings are consistent with several mechanisms. For example, the evidence provided by the Bandiera et al. (2017) paper supports all three mechanisms: these women were credit constrained, they (presumably) needed training, and their saving rates increased significantly as result of the program. They support other mechanisms too that are not directly captured by theoretical framework here. For example, the training component of this program not only involved initial training, but also regular visits by livestock specialists and program officers of the nongovernmental organization that undertook the program over a two-year period after the transfer to cover the life cycle of livestock. One could argue that to the extent the poor are subject to behavioral biases, these visits may have helped them overcome these in addition to the stated goal of helping them overcome their limited experience of dealing with livestock.

Third, they highlight the need to have a clear theoretical framework that helps us understand better the relationship between specific antipoverty policies and particular mechanisms for persistence of poverty. For example, if strong income effects are the main culprit, then cash transfers are the best (unless there are supply-side delivery constraints, such as markets being inaccessible in remote rural areas or strong grounds for paternalistic concerns). Otherwise, cash transfers may not have much of an effect beyond current consumption. That may be the reason behind the findings of Araujo, Bosch, and Schady in Nicaragua. In contrast, if capital market frictions are the main problem, then direct provision of credit or facilitating borrowing and savings may be better and more cost-effective than income or wealth transfer. However, if training or some other input is depressing overall productivity in the area, then without addressing that constraint, none of these interventions will be particularly effective.

Conclusion

As we know, what a given policy evaluation provides is the marginal effect of changing an instrument (or, a combination of instruments) given a certain vector of individual and local characteristics. What it does not tell us (interactions do a partial job) is what would be the effect of the same policy in other settings. This is partly a point about external validity that is well known. But a related and more subtle implication of this point is, given those characteristics, what would be the effect of alternative policies?

The choice of a given policy reflects a researcher's implicit priors about what is the binding constraint or scarce input in a given setting. For example, a village that lacks a road that connects it to the market will not benefit much from other interventions. This highlights the importance of having a method of diagnosing what are the key frictions in a given setting, and in particular, what is the most binding constraint. For that, baseline surveys and some basic diagnostic theoretical framework is needed. Otherwise, there is a real risk of throwing darts in the dark, or to draw a closer analogy, applying a treatment on a patient without checking the symptoms.

Theory helps us in this endeavor in three distinct ways. First, it helps us ask the right questions: what are the causes of the persistence of poverty, what are the consequences or symptoms, and which one is likely to be salient under what parameter conditions? This helps us formulate empirical tests and design experiments. Second, theory allows us to do counterfactual analysis. What happened in the context of evaluating a specific policy is one of many possibilities. Having different arms of treatments is costly in terms of the required sample size to have statistical power. Also, external validity requires many experiments in different settings, which while essential, is not feasible in the short to medium run. A theoretical framework allows us to generate alternative hypothetical scenarios, and coupled with quantitative analysis with existing data, can help tell us what alternative policies could have done, thereby helping suggest new directions for empirical research. Finally, theory allows us to do welfare analysis. Once we know a particular program leads to a specific outcome, we need a normative framework where the cost of funds and the benefits to the target group are all taken into account to do a proper social cost-benefit analysis.

This feeds into a broader policy lesson that there is no unique policy that will help remove poverty or achieve development. Even when overall average treatment effects are not impressive, we cannot abandon a particular policy as a policy tool because of the potential importance of heterogeneous treatment effects. Otherwise, it would be the same as abandoning a particular medication for the population at large, and not those subject to certain health conditions. What works presupposes that we know the problems of a given individual or an area well, and are simply trying to figure out which method works best. As much as different ailments require different treatments, rather than ask "what works," it is best to ask "what works for a particular problem for a given individual?"

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