

Comment

Felipe Barrera-Osorio: This paper has several strengths. First, though there are numerous evaluations of the impact of conditional cash transfers in education, it is critical to replicate results in different contexts and with different designs. This article serves that goal.

Second, the paper does not find effects of the program on educational outcomes, in contrast to the general findings of conditional cash transfers around the world (Saavedra and García 2012; Fiszbein and Schady 2009). At the risk of stating the obvious, a no result is an important result. Too often, journals ignore this fact.

Finally, the paper investigates three mechanisms by which it is possible to explain the lack of results. It is clear that one of the most important frontiers in evaluation is the quest to find mechanisms (à la Jens, Kling, and Mullainathan 2011), in addition to just finding effects. Again, this paper contributes to this agenda.

The authors do not find impacts from a typical conditional cash transfer in Uruguay on school attendance and child labor. As mentioned before, these results contrast sharply with the general empirical evidence elsewhere. In contexts with similar secondary school enrollment rates, conditional cash transfers have impacted schooling decisions (for example, my own research in Bogotá, Colombia; Barrera-Osorio and others 2011).

Moreover, it seems that the lack of results is not driven by adult labor responses. This paper corroborates an important finding of the literature on conditional cash transfers: these programs do not seem to trigger adult labor responses. Finally, the authors do not find a correlation between school enrollment and household's knowledge (or lack thereof) of the program's conditionality requirements.

Indeed, the lack of results on school attendance is very puzzling. Given that labor supply and conditionalities cannot explain the lack of results, the authors advance two potential hypotheses. First, it may be that the program does not complement the demand intervention with supply policies. However, it is difficult to accept this explanation since the vast majority of conditional cash transfer programs around the world are not complemented with supply interventions, and they have shown effects on school enrollment and attendance. Second, the authors advance the hypothesis that the lack of results can be explained by the lump-sum nature of the cash transfer, that is, the family receives a certain amount of money independent of the number of minors in the household. Again, it is difficult to accept this hypothesis: we know that the amount of cash matters (Fernald, Gertler, and Neufeld 2009), but we also know that the elasticity of education is quite high (Fiszbein and Schady 2009). Even if the amount is a lump sum, based on the vast empirical literature, income and price effects should trigger an education response.

Let me advance another explanation. The evaluation covers, in the first follow-up, only six months of treatment; in the second follow-up, approximately 1.5 years of treatment. It is a very short period of treatment. Also, the targeted population of the program—the very poor—was getting out of a deep recession. The timing of these two events can explain the lack of responses: families received the money and were trying to come back to their long-term consumption and investment trajectory—which would explain the lack of response—and the evaluation covered a very brief period of time. It would be extremely important to see the long-term effects of the program. However, the program was terminated in December 2007, and therefore it is quite difficult to assess long-term effects.

Another interesting aspect of this particular program is that the government changed it substantially in December 2007. Actually, the program was finished and replaced by another. I would like to believe that the evaluation triggered that change, that, given the lack of results, the government decided to change the design of the program.

Let me finish by proposing two future lines of research. First, as I mentioned, the measurement of long-term effects of conditional cash transfers, especially in eliminating poverty, would be the acid test of these types of policies. For randomized controlled trials, research on long-term impacts is challenging, since the original sample may suffer drastic changes through time. Second, I believe that the next generation of conditional cash transfers should be conditioned on performance and not on attendance to services. Conditioning on performance has the challenge that the individuals best suited to reach

the conditionality are presumably the more apt ones in the target population, raising questions of equity.

Sometimes the work of economists is like the work of detectives. The detective arrives at a scene and tries to follow clues to construct a plausible story of events. In economics, we have a context (scene) in which a program is implemented. We try to follow clues to reconstruct the effects of the program. With some luck, we find the smoking gun. In this specific case, the story seems very difficult to read. It is akin to a detective who knows that something happened in a place, but on arrival at the scene finds nothing that indicates any change. Moreover, the detective cannot find any clues. This paper did not find any smoking gun—or, in fact, any gun. It is quite unsatisfactory and puzzling at the same time.