EXPERIMENTS IN DEVELOPMENT ECONOMICS

ANA CORREA

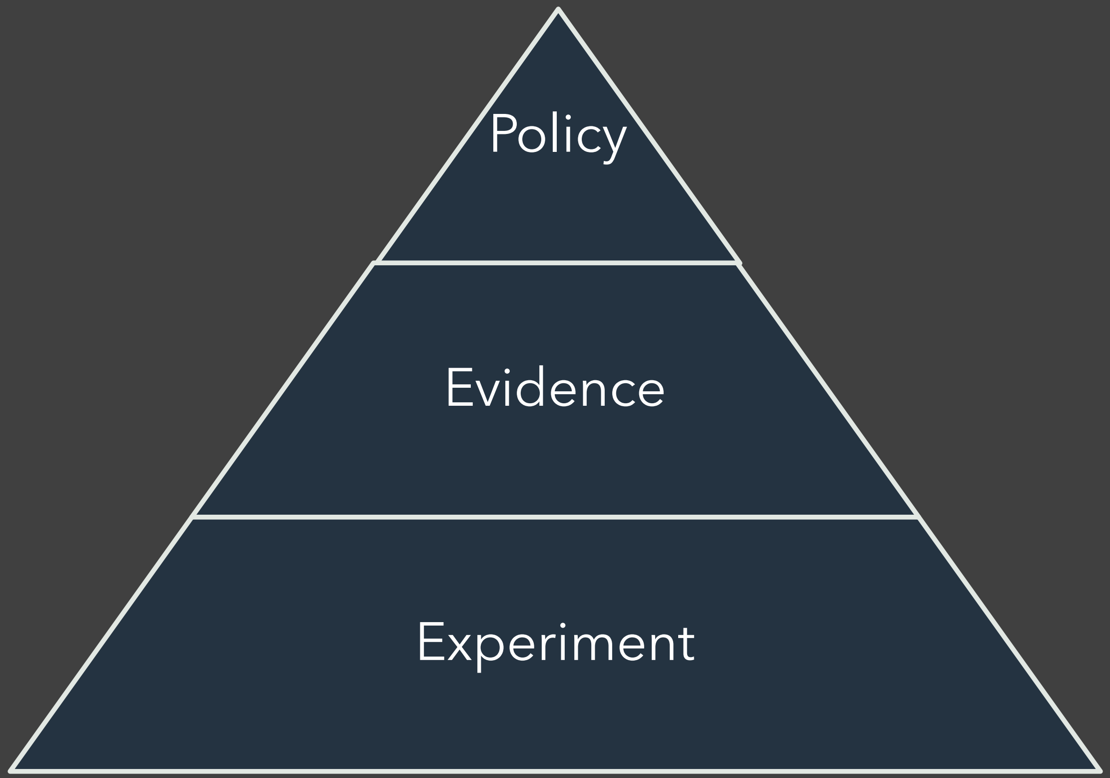
OUTLINE

- Why do we need experiments?
- Method 1: Natural experiments
- Method 2: RCTs
- Method 3: Quasi experimental designs
- What can it apply to?
- Conclusion

OUTLINE

- Why do we need experiments?
- Method 1: Natural experiments
- Method 2: RCTs
- Method 3: Quasi experimental designs
- What can it apply to?
- Conclusion

WHY DO WE NEED EXPERIMENTS?



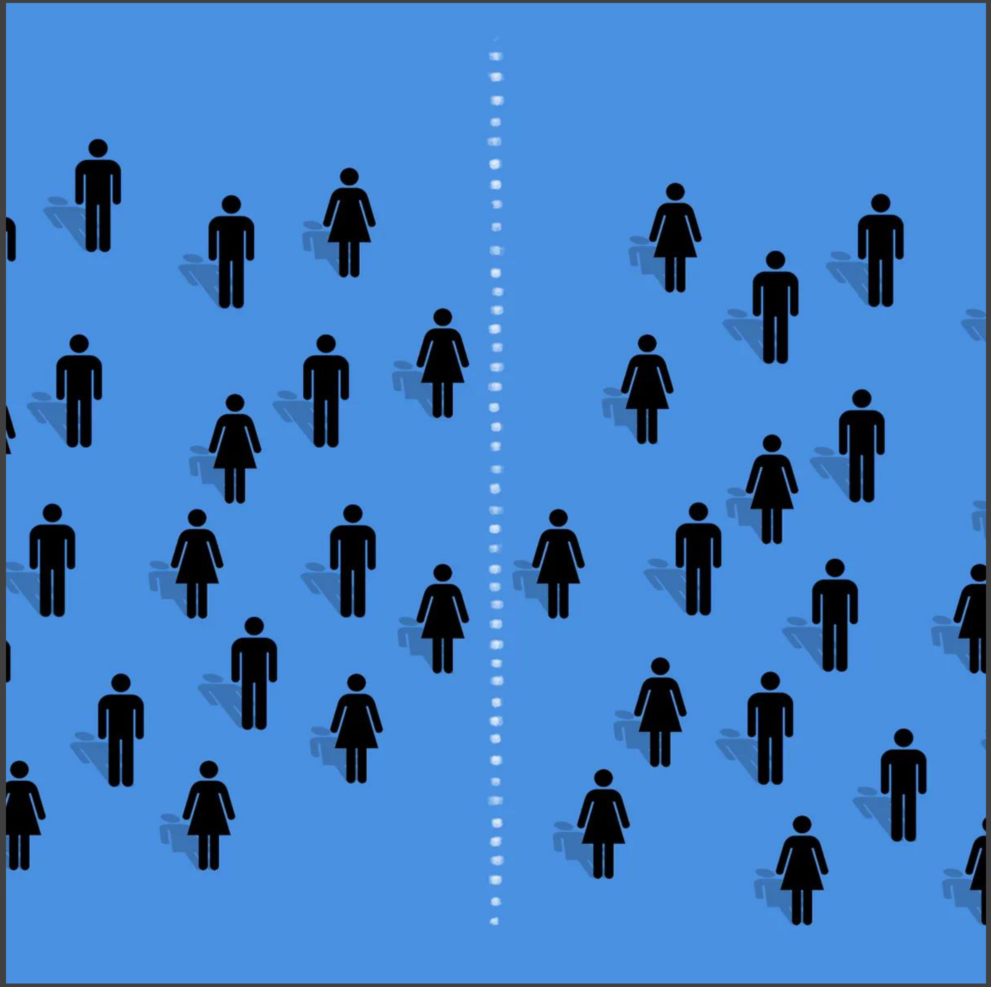
- Test theories to provide evidence for policy-making.
- Can we do experiments on people? Not always.
- Why are the simple results from a pilot implementation not enough?

WHY DO WE NEED EXPERIMENTS?

- Causal inference is difficult to assert.
  - Causal inference is when we draw a conclusion about the causes of an event.
  - For instance, employment increased as a result of a change in welfare policy – the **factual**.
  - But at the same time, global economic conditions may have improved, leading to the increase in employment.
  - How can we attribute the increase in employment to the change in policy?
  - We need to see what would have happened without the change in welfare policy – the **counterfactual**.

WHY DO WE NEED EXPERIMENTS?

- We can compare a treatment and control group.
  - For instance, if the policy was changed in some villages, but not others – and then employment increased just in the treated villages.
  - But what if the treated villages were also those with the highest economic potential?
  - The treatment allocation needs to be random or arising from an exogenous source.
  - If this is not possible, there are ways to make the two groups as comparable as possible.



OUTLINE

- Why do we need experiments?
- Method 1: Natural experiments
- Method 2: RCTs
- Method 3: Quasi experimental designs
- What can it apply to?
- Conclusion

METHOD 1: NATURAL EXPERIMENTS

- One of the most exciting circumstances in economics:
  - Exogenous circumstances assign treatment to individuals.
  - It’s just a lucky situation!
  - We can then study the outcome for the treated individuals compared to the non-treated individuals.

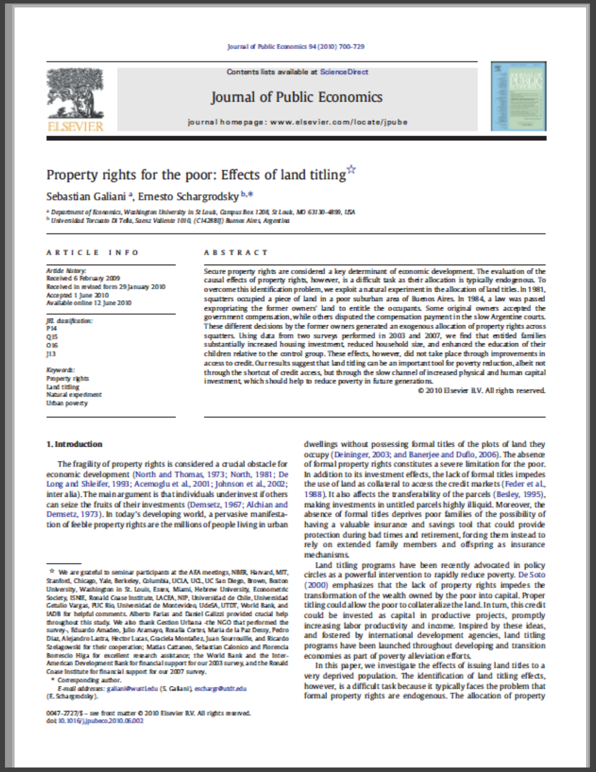
METHOD 1: NATURAL EXPERIMENTS

- Galiani, S., & Schargrodsky, E. (2010). Property rights for the poor: Effects of land titling. *Journal of Public Economics*, *94*(9-10), 700-729.

- Card, D. (1990). The impact of the Mariel boatlift on the Miami labor market. *ILR Review*, *43*(2), 245-257.

METHOD 1: NATURAL EXPERIMENTS

- Galiani, S., & Schargrodsky, E. (2010). Property rights for the poor: Effects of land titling. *Journal of Public Economics*, *94*(9-10), 700-729.
- What are the effects of issuing land titles to the poor?
- Problem: Most people that gain land titles are wealthier, more educated, etc. This is called endogeneity.
- Solution: Use a natural experiment that gives an exogenous source of the land title.



Journal of Public Economics 94 (2010) 700–729

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

Property rights for the poor: Effects of land titling[☆]

Sebastian Galiani [a], Ernesto Schargrodsky [b,*]

[a] Department of Economics, Washington University in St Louis, Campus Box 1208, St Louis, MO 63130-4899, USA
[b] Universidad Torcuato Di Tella, Saenz Valiente 1010, (C1428BIJ) Buenos Aires, Argentina

ARTICLE INFO

Article history:
Received 6 February 2009
Received in revised form 29 January 2010
Accepted 1 June 2010
Available online 12 June 2010

JEL classification:
P14
Q15
O16
J13

Keywords:
Property rights
Land titling
Natural experiment
Urban poverty

ABSTRACT

Secure property rights are considered a key determinant of economic development. The evaluation of the causal effects of property rights, however, is a difficult task as their allocation is typically endogenous. To overcome this identification problem, we exploit a natural experiment in the allocation of land titles. In 1981, squatters occupied a piece of land in a poor suburban area of Buenos Aires. In 1984, a law was passed expropriating the former owners' land to entitle the occupants. Some original owners accepted the government compensation, while others disputed the compensation payment in the slow Argentine courts. These different decisions by the former owners generated an exogenous allocation of property rights across squatters. Using data from two surveys performed in 2003 and 2007, we find that entitled families substantially increased housing investment, reduced household size, and enhanced the education of their children relative to the control group. These effects, however, did not take place through improvements in access to credit. Our results suggest that land titling can be an important tool for poverty reduction, albeit not through the shortcut of credit access, but through the slow channel of increased physical and human capital investment, which should help to reduce poverty in future generations.

1. Introduction

The fragility of property rights is considered a crucial obstacle for economic development (North and Thomas, 1973; North, 1981; De Long and Shleifer, 1993; Acemoglu et al., 2001; Johnson et al., 2002; inter alia). The main argument is that individuals underinvest if others can seize the fruits of their investments (Demsetz, 1967; Alchian and Demsetz, 1973). In today's developing world, a pervasive manifestation of feeble property rights are the millions of people living in urban dwellings without possessing formal titles of the plots of land they occupy (Deininger, 2003; and Banerjee and Duflo, 2006). The absence of formal property rights constitutes a severe limitation for the poor. In addition to its investment effects, the lack of formal titles impedes the use of land as collateral to access the credit markets (Feder et al., 1988). It also affects the transferability of the parcels (Besley, 1995), making investments in untitled parcels highly illiquid. Moreover, the absence of formal titles deprives poor families of the possibility of having a valuable insurance and savings tool that could provide protection during bad times and retirement, forcing them instead to rely on extended family members and offspring as insurance mechanisms.

Land titling programs have been recently advocated in policy circles as a powerful intervention to rapidly reduce poverty. De Soto (2000) emphasizes that the lack of property rights impedes the transformation of the wealth owned by the poor into capital. Proper titling could allow the poor to collateralize the land. In turn, this credit could be invested as capital in productive projects, promptly increasing labor productivity and income. Inspired by these ideas, and fostered by international development agencies, land titling programs have been launched throughout developing and transition economies as part of poverty alleviation efforts.

In this paper, we investigate the effects of issuing land titles to a very deprived population. The identification of land titling effects, however, is a difficult task because it typically faces the problem that formal property rights are endogenous. The allocation of property

☆ We are grateful to seminar participants at the AEA meetings, NBER, Harvard, MIT, Stanford, Chicago, Yale, Berkeley, Columbia, UCLA, UCL, UC San Diego, Brown, Boston University, Washington in St. Louis, Essex, Miami, Hebrew University, Econometric Society, ISNIE, Ronald Coase Institute, LACEA, NIP, Universidad de Chile, Universidad Getulio Vargas, PUC Rio, Universidad de Montevideo, UdeSA, UTDT, World Bank, and IADB for helpful comments. Alberto Farias and Daniel Galizzi provided crucial help throughout this study. We also thank Gestion Urbana -the NGO that performed the survey-, Eduardo Amadeo, Julio Aramayo, Rosalia Cortes, Maria de la Paz Dessy, Pedro Diaz, Alejandro Lastra, Hector Lucas, Graciela Montañez, Juan Sourrouille, and Ricardo Szelagowski for their cooperation; Matias Cattaneo, Sebastian Calonico and Florencia Borrescio Higa for excellent research assistance; the World Bank and the Inter-American Development Bank for financial support for our 2003 survey, and the Ronald Coase Institute for financial support for our 2007 survey.

* Corresponding author.
E-mail addresses: galiani@wustl.edu (S. Galiani), eschargr@utdt.edu (E. Schargrodsky).

0047-2727/$ – see front matter © 2010 Elsevier B.V. All rights reserved.
doi:10.1016/j.jpubeco.2010.06.002

METHOD 1: NATURAL EXPERIMENTS

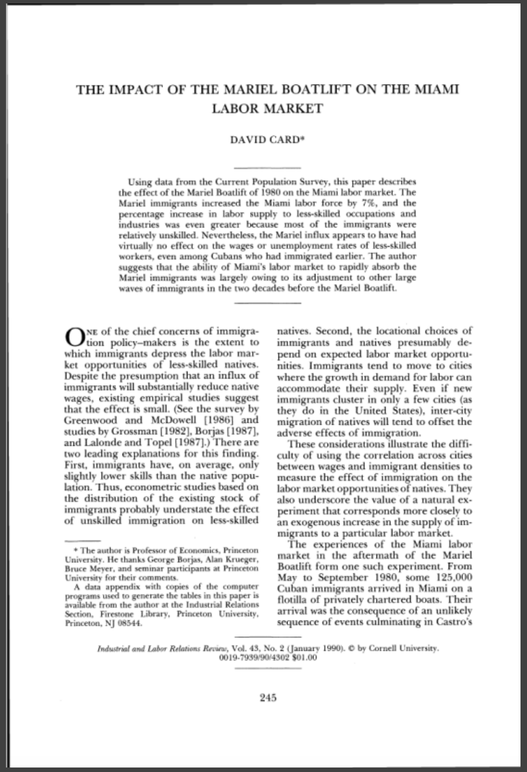
- In 1981, a group of squatters set up an informal settlement on the outskirts of Buenos Aires.
- The government passed a law expropriating the original landowners in exchange for compensation.
- The expropriated land would then be passed on to the squatters, with a legal title.
- Some landowners agreed, and some did not.
- Therefore, some squatters received land titles, and some did not.
- The allocation of land titles is exogenous!

METHOD 1: NATURAL EXPERIMENTS

- Galiani & Schargrodsky used this natural experiment (they did not plan it!) to test their hypothesis.
- The squatters without titles act as a control group (i.e. what would have happened without the titles).
  - They are identical to the squatters with titles in demographic characteristics.
- Galiani & Schargrodsky found that squatters with titles increased their housing quality, reduced fertility, and their children had better educational achievements.
- This is strong evidence for helping the poor access credit (think of microfinance!).

METHOD 1: NATURAL EXPERIMENTS

- Card, D. (1990). The impact of the Mariel boatlift on the Miami labor market. *ILR Review*, *43*(2), 245-257.
- What are the effects of immigration on local wages?
- Problem: Most economic migration is to cities with many job opportunities, which can absorb the employment demand without increase in wages.
- Solution: Use a natural experiment that gives an exogenous source of migration.



THE IMPACT OF THE MARIEL BOATLIFT ON THE MIAMI LABOR MARKET

DAVID CARD*

Using data from the Current Population Survey, this paper describes the effect of the Mariel Boatlift of 1980 on the Miami labor market. The Mariel immigrants increased the Miami labor force by 7%, and the percentage increase in labor supply to less-skilled occupations and industries was even greater because most of the immigrants were relatively unskilled. Nevertheless, the Mariel influx appears to have had virtually no effect on the wages or unemployment rates of less-skilled workers, even among Cubans who had immigrated earlier. The author suggests that the ability of Miami's labor market to rapidly absorb the Mariel immigrants was largely owing to its adjustment to other large waves of immigrants in the two decades before the Mariel Boatlift.

ONE of the chief concerns of immigration policy–makers is the extent to which immigrants depress the labor market opportunities of less-skilled natives. Despite the presumption that an influx of immigrants will substantially reduce native wages, existing empirical studies suggest that the effect is small. (See the survey by Greenwood and McDowell [1986] and studies by Grossman [1982], Borjas [1987], and Lalonde and Topel [1987].) There are two leading explanations for this finding. First, immigrants have, on average, only slightly lower skills than the native population. Thus, econometric studies based on the distribution of the existing stock of immigrants probably understate the effect of unskilled immigration on less-skilled natives. Second, the locational choices of immigrants and natives presumably depend on expected labor market opportunities. Immigrants tend to move to cities where the growth in demand for labor can accommodate their supply. Even if new immigrants cluster in only a few cities (as they do in the United States), inter-city migration of natives will tend to offset the adverse effects of immigration.

These considerations illustrate the difficulty of using the correlation across cities between wages and immigrant densities to measure the effect of immigration on the labor market opportunities of natives. They also underscore the value of a natural experiment that corresponds more closely to an exogenous increase in the supply of immigrants to a particular labor market.

The experiences of the Miami labor market in the aftermath of the Mariel Boatlift form one such experiment. From May to September 1980, some 125,000 Cuban immigrants arrived in Miami on a flotilla of privately chartered boats. Their arrival was the consequence of an unlikely sequence of events culminating in Castro's

* The author is Professor of Economics, Princeton University. He thanks George Borjas, Alan Krueger, Bruce Meyer, and seminar participants at Princeton University for their comments.

A data appendix with copies of the computer programs used to generate the tables in this paper is available from the author at the Industrial Relations Section, Firestone Library, Princeton University, Princeton, NJ 08544.

Industrial and Labor Relations Review, Vol. 43, No. 2 (January 1990). © by Cornell University.
0019-7939/90/4302 $01.00

METHOD 1: NATURAL EXPERIMENTS

- The Mariel boatlift was the result of a political decision from the Cuban government to allow emigration via the single port of Mariel, for people who could arrange their own transport.
- Cuban Americans in Miami arranged this transport.
- This resulted in an estimated 120,000-125,000 Cubans immigrating to Miami, an increase of 7% of the Miami workforce at the time.

METHOD 1: NATURAL EXPERIMENTS

- This provided an exogenous source of immigration to the workforce – particularly in low-skilled industries.
- This was used by Card to determine the effect of immigration on wages.
- Card found no effects on wages.
- However, no experiment is perfect!
  - Miami's labour market is very unique in the United States.
  - Structured around low-skilled industries.
  - Some evidence that the Mariel immigration displaced other migrants.

OUTLINE

- Why do we need experiments?
- Method 1: Natural experiments
- Method 2: RCTs
- Method 3: Quasi experimental designs
- What can it apply to?
- Conclusion

METHOD 2: RCTS

- Commonly used method in the natural sciences, especially in medicine.
- Treatment is randomly allocated:
  - Treatment group receives the treatment
  - Control group does not receive the treatment
  - Outcomes are compared
- Not always ethical!

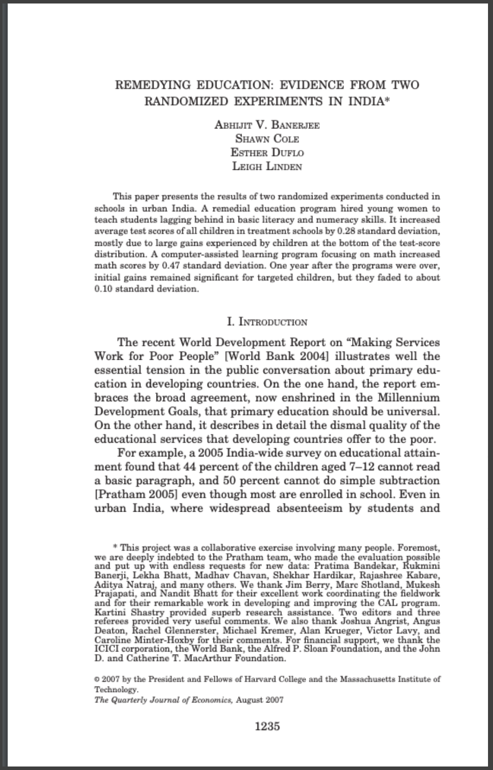
METHOD 2: RCTS

- Banerjee, A. V., Cole, S., Duflo, E., & Linden, L. (2007). Remedying education: Evidence from two randomized experiments in India. The Quarterly Journal of Economics, 122(3), 1235-1264.

- Miguel, E., & Kremer, M. (2004). Worms: identifying impacts on education and health in the presence of treatment externalities. *Econometrica*, *72*(1), 159-217.

METHOD 2: RCTS

- Banerjee, A. V., Cole, S., Duflo, E., & Linden, L. (2007). Remedying education: Evidence from two randomized experiments in India. The Quarterly Journal of Economics, 122(3), 1235-1264.
- What are the relative effects of two school programmes on score tests?
- Problem: The programme that is implemented may be affected by the quality of teaching at each school.
- Solution: Randomise the treatment allocation and include a control group.



REMEDYING EDUCATION: EVIDENCE FROM TWO RANDOMIZED EXPERIMENTS IN INDIA*

ABHIJIT V. BANERJEE
SHAWN COLE
ESTHER DUFLO
LEIGH LINDEN

This paper presents the results of two randomized experiments conducted in schools in urban India. A remedial education program hired young women to teach students lagging behind in basic literacy and numeracy skills. It increased average test scores of all children in treatment schools by 0.28 standard deviation, mostly due to large gains experienced by children at the bottom of the test-score distribution. A computer-assisted learning program focusing on math increased math scores by 0.47 standard deviation. One year after the programs were over, initial gains remained significant for targeted children, but they faded to about 0.10 standard deviation.

I. INTRODUCTION

The recent World Development Report on "Making Services Work for Poor People" [World Bank 2004] illustrates well the essential tension in the public conversation about primary education in developing countries. On the one hand, the report embraces the broad agreement, now enshrined in the Millennium Development Goals, that primary education should be universal. On the other hand, it describes in detail the dismal quality of the educational services that developing countries offer to the poor.

For example, a 2005 India-wide survey on educational attainment found that 44 percent of the children aged 7–12 cannot read a basic paragraph, and 50 percent cannot do simple subtraction [Pratham 2005] even though most are enrolled in school. Even in urban India, where widespread absenteeism by students and

* This project was a collaborative exercise involving many people. Foremost, we are deeply indebted to the Pratham team, who made the evaluation possible and put up with endless requests for new data: Pratima Bandekar, Rukmini Banerji, Lekha Bhatt, Madhav Chavan, Shekhar Hardikar, Rajashree Kabare, Aditya Natraj, and many others. We thank Jim Berry, Marc Shotland, Mukesh Prajapati, and Nandit Bhatt for their excellent work coordinating the fieldwork and for their remarkable work in developing and improving the CAL program. Kartini Shastry provided superb research assistance. Two editors and three referees provided very useful comments. We also thank Joshua Angrist, Angus Deaton, Rachel Glennerster, Michael Kremer, Alan Krueger, Victor Lavy, and Caroline Minter-Hoxby for their comments. For financial support, we thank the ICICI corporation, the World Bank, the Alfred P. Sloan Foundation, and the John D. and Catherine T. MacArthur Foundation.

The Quarterly Journal of Economics, August 2007

METHOD 2: RCTS

- Programme 1: For struggling children, provide them with remedial education conducted by a “Balsakhi”, a young person from the community.

- Programme 2: For all children, provide them with computer-assisted learning targeted to their ability. This is focused on math skills.

METHOD 2: RCTS

- Banerjee et al. found that both programmes had a positive effect.
  - The Balsakhi programme increased scores for struggling children by 0.14 standard deviations in the first year, and 0.28 in the second year.
  - The computer-assisted learning programme increased math scores for all children by 0.35 standard deviations in the first year, and 0.47 in the second year.

METHOD 2: RCTS

- Miguel, E., & Kremer, M. (2004). Worms: identifying impacts on education and health in the presence of treatment externalities. *Econometrica*, *72*(1), 159-217.
- What are the effects of a deworming programme on school attendance?
- Problem: What if there are externalities?
- Solution: Randomise at the school level and measure effects within distance of treated units.

Econometrica, Vol. 72, No. 1 (January, 2004), 159–217

WORMS: IDENTIFYING IMPACTS ON EDUCATION AND HEALTH IN THE PRESENCE OF TREATMENT EXTERNALITIES

BY EDWARD MIGUEL AND MICHAEL KREMER[1]

Intestinal helminths—including hookworm, roundworm, whipworm, and schistosomiasis—infect more than one-quarter of the world's population. Studies in which medical treatment is randomized at the individual level potentially doubly underestimate the benefits of treatment, missing externality benefits to the comparison group from reduced disease transmission, and therefore also underestimating benefits for the treatment group. We evaluate a Kenyan project in which school-based mass treatment with deworming drugs was randomly phased into schools, rather than to individuals, allowing estimation of overall program effects. The program reduced school absenteeism in treatment schools by one-quarter, and was far cheaper than alternative ways of boosting school participation. Deworming substantially improved health and school participation among untreated children in both treatment schools and neighboring schools, and these externalities are large enough to justify fully subsidizing treatment. Yet we do not find evidence that deworming improved academic test scores.

KEYWORDS: Health, education, Africa, externalities, randomized evaluation, worms.

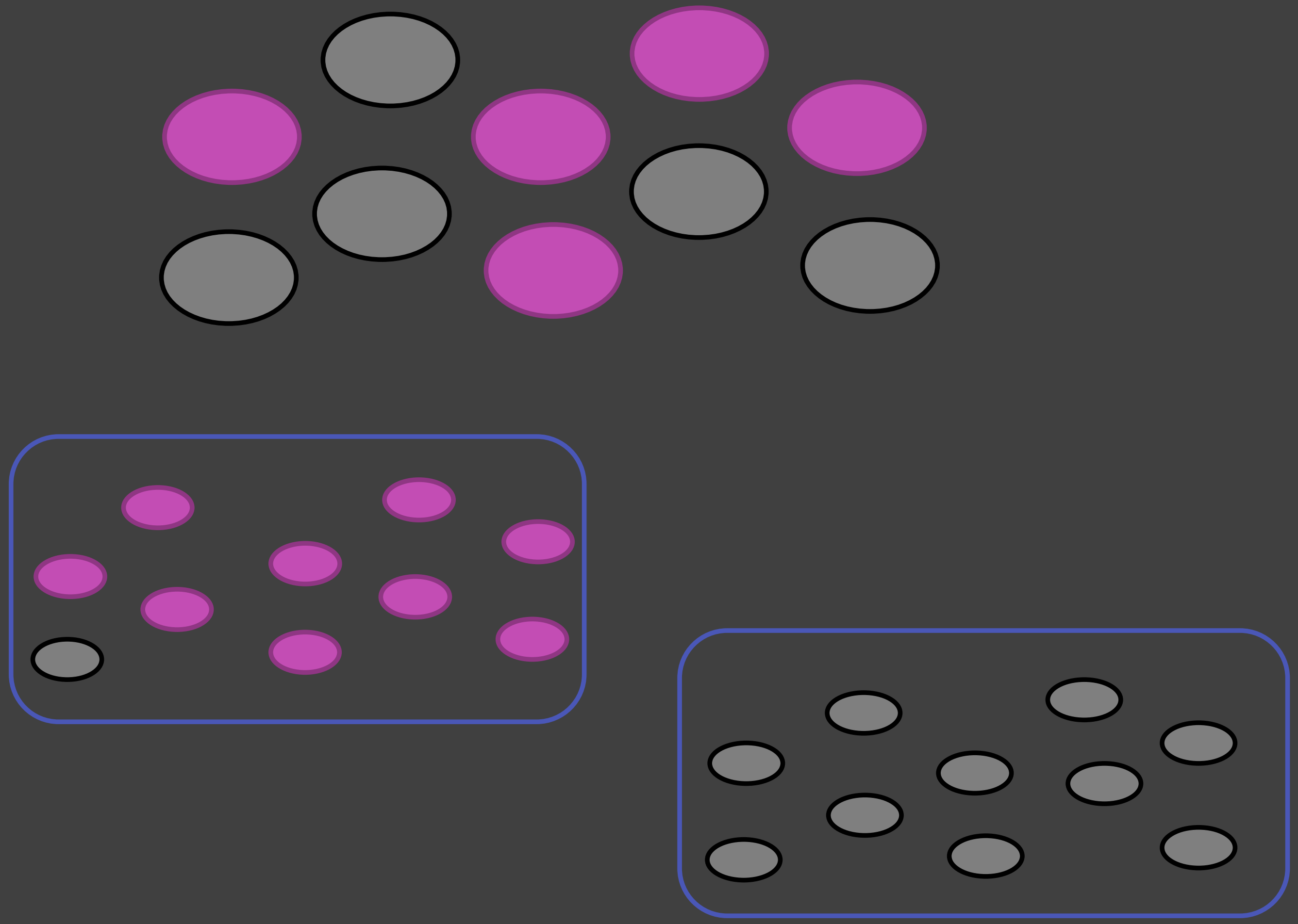
1. INTRODUCTION

HOOKWORM, ROUNDWORM, WHIPWORM, and schistosomiasis infect one in four people worldwide. They are particularly prevalent among school-age children in developing countries. We examine the impact of a program in which seventy-five rural Kenyan primary schools were phased into deworming treatment in a randomized order. We find that the program reduced school absenteeism by at least one-quarter, with particularly large participation gains among the youngest children, making deworming a highly effective way to boost school participation among young children. We then identify cross-school externalities—the impact of deworming for pupils in schools located near treatment schools—using exogenous variation in the local density of treatment school pupils generated by the school-level randomization, and find that deworming reduces worm burdens and increases school participation among

[1]The authors thank ICS Africa, the Kenya Ministry of Health Division of Vector Borne Diseases, Donald Bundy, and Paul Glewwe for their cooperation in all stages of the project, and would especially like to acknowledge the contributions of Elizabeth Beasley, Laban Benaya, Pascaline Dupas, Simon Brooker, Alfred Luoba, Sylvie Moulin, Robert Namunyu, Polycarp Waswa, and the PSDP field staff and data group, without whom the project would not have been possible. Gratitude is also extended to the teachers and school children of Busia for participating in the study. George Akerlof, Harold Alderman, Timothy Besley, Peter Hotez, Caroline Hoxby, Lawrence Katz, Doug Miller, Chris Udry, and the editor and four anonymous referees have provided valuable comments. Melissa Gonzalez-Brenes, Andrew Francis, Bryan Graham, Tina Green, Jessica Leino, Emily Oster, Anjali Oza, and Jon Robinson have provided excellent research assistance. The evaluation was sponsored by the World Bank and the Partnership for Child Development, but all viewpoints, as well as any errors, are our own.

METHOD 2: RCTS

- Randomisation at the child-level does not work if you have an infectious disease.
- Randomise at the school-level (not every child may receive the treatment) and see if there are cross-school externalities.



METHOD 2: RCTS

- Miguel and Kremer found that the treatment had positive externalities for untreated students, and even those in control schools nearby.
- This led to an average 7.5% increase in school participation.

OUTLINE

- Why do we need experiments?
- Method 1: Natural experiments
- Method 2: RCTs
- Method 3: Quasi experimental designs
- What can it apply to?
- Conclusion

METHOD 3: QUASI EXPERIMENTAL DESIGNS

- Propensity Score Matching (PSM) and Instrumental Variables (IV) are ways in which the comparison between treated and control groups can be improved if there are no RCTs or natural experiments.
- This is what is done in quasi-experimental designs - where there may be treatment and control units, but not randomised or exogenously assigned.

PROPENSITY SCORE MATCHING

- This is especially useful when there is a problem of self selection:
  - Healthier people going to the doctor to get vaccinated.
  - More educated people joining training schemes.
- Using a probit regression with variables that can affect treatment, you can create a "propensity score" – probability that they would have been treated.
- Match people in treatment and control groups with same propensity score.
- Paper to read: Jalan, J., & Ravallion, M. (2003). Estimating the benefit incidence of an antipoverty program by propensity-score matching. *Journal of Business & Economic Statistics*, *21*(1), 19-30.

INSTRUMENTAL VARIABLES

- Sometimes, there are unobservable variables that can affect both the explanatory and the outcome variables.
  - E.g. The effect of smoking on health
  - There are unobserved variables that affect both smoking and health, so causality of smoking cannot be isolated.
- An instrumental variable is a variable correlated with the explanatory variable, but independent from the outcome variable.
- This is then used as a new explanatory variable.
- Paper to read: Angrist, J., & Evans, W. (1998). Children and Their Parents' Labor Supply: Evidence from Exogenous Variation in Family Size. *The American Economic Review, 88*(3), 450-477.

OUTLINE

- Why do we need experiments?
- Method 1: Natural experiments
- Method 2: RCTs
- Method 3: Quasi experimental designs
- What can it apply to?
- Conclusion

WHAT CAN IT APPLY TO?

- My own PhD research:
  - A conditional cash transfer in Colombia called Familias en Accion.
  - I want to see if there are health externalities to this programme.
  - Three types of externalities: peer effects, local economy effects, and public health effects.

HOW TO ADDRESS IT?

- Methods similar to Miguel & Kremer’s (2004).
- However, the programme wasn’t randomised, so I need to compare units using propensity score matching.
- For externality effects on non-beneficiaries, I can compare non-eligible people in treated municipalities with non-eligible people in non-treated municipalities.
- Dependent on chosen mechanism and data!

OUTLINE

- Why do we need experiments?
- Method 1: Natural experiments
- Method 2: RCTs
- Method 3: Quasi experimental designs
- What can it apply to?
- Conclusion

CONCLUSION

- To find the effect of a policy, you need to find what would have happened in the absence of that policy – a **counterfactual**.
- You need to compare treatment and control groups:
  - You can get lucky and have a **natural experiment.**
  - You can design a **randomised controlled trial.**
  - You can match treated people with non-treated people using **propensity score matching.**
  - If there are unobserved factors affecting both the explanatory and outcome variables, you can use an **instrumental variable**.
- It all depends on the policy you want to measure, the mechanism, and the data.