Aid, Education and Development

Maria Carmela Perrotta

ISSN 0346-6892
ISBN 978-91-7447-166-3

Cover Picture: *Italian Children Eating Breakfast in Kindergarten*
© Alinari Archives/Corbis/Scanpix
Printed in Sweden by PrintCenter US-AB, Stockholm 2010

Distributor: Institute for International Economic Studies

Doctoral Dissertation
Department of Economics
Stockholm University

Abstract

This thesis consists of four essays in development and political economics.

The first essay, "Aid Effectiveness: New Instrument, New Results?" asks the question of whether aid leads to growth, which, despite a voluminous literature, is still controversial. To observe the effect of aid, researchers have used instrumental variables that must be exogenous to growth and explain well aid flows. This paper argues that instruments used in the past do not satisfy these conditions. We propose a new instrument based on aid quantities as predicted by the priority that different recipients are given by the donors. We find a significant and relatively big effect of aid: a one standard deviation increase in received aid is associated with a .46 to 3.2 percentage points higher growth rate.

The second essay, "Hidden Redistribution in Higher Education", investigates one potential motive behind public expenditures in higher education in Sub-Saharan Africa. Low income countries, and in particular countries in Sub-Saharan Africa, have invested huge resources over the last 40 years in financing higher education, compared with the number of students enrolled at that level and with the corresponding expenditures for lower levels of education. I propose and test the hypothesis that overspending in higher education reflects patterns of redistribution towards the elites close to the political leaders, when this level of education is accessible exclusively or mostly to such groups. I find support for this hypothesis, but the bulk of the Sub-Saharan Africa spending anomaly remains to be explained.

The third essay, "The Impact of a Food for Education Program on Schooling in Cambodia", is a program evaluation. Food for Education (FFE) programs, which consist of meals served in school and in some cases take-home

rations, are considered a powerful tool to improve education and health outcomes for children in the developing world. Compared to other programs, such as conditional cash transfers and scholarships, school meals provide a stronger incentive to attend school because children must be in school to receive the rations. In this paper, we find that the Cambodia FFE, implemented in 6 Cambodian regions between 1999 and 2003, increased enrollment rates, school attendance and achieved education. We also investigate who benefited the most, and how cost-effective such a program is compared to other types of interventions.

The fourth essay, "Constitutions and the Growth Elasticity of Poverty", explores, with help of new data, the heterogeneity around a well known average relationship, the one between growth and poverty reduction. Increasing per capita incomes are generally associated with decreasing poverty rates. After the UN Millennium Declaration, a big research effort has focused on the responsiveness of poverty to growth using the concept of growth-elasticity of poverty: the percentage change in poverty associated with a 1 percent growth in per capita income. The main focus is on the effect of the constitution, in particular the form of government and the electoral rule, which has not previously been explored in the literature.

Alle mie nonne

Acknowledgments

> *Neque porro quisquam est qui dolorem ipsum quia dolor sit amet, consectetur, adipisci velit, sed quia non numquam [do] eius modi tempora inci[di]dunt, ut labore et dolore magnam aliquam quaerat voluptatem.* Nor again is there anyone who loves or pursues or desires to obtain pain of itself, because it is pain, but occasionally circumstances occur in which toil and pain can procure him some great pleasure.
>
> (M. T. Cicero, 45 b.C. *De Finibus Bonorum et Malorum*)

I used this excerpt to provide a filler text during typesetting of this section. A variation of it has been used for the same purpose since at least the 1960s, and possibly since the sixteenth century.[1] And now somehow it seemed appropriate to begin talking about the PhD. It has been a long journey; to be honest, it has not really always been toil and pain, and this is mostly thanks to some good company met on the way.

But I should start from the start, and begin thanking prof. Guido Tabellini, my advisor during undergraduate studies, and prof. Torsten Persson, then director of the Institute for International Economic Studies, who first proposed the idea and gave me the possibility to continue my studies at the PhD level here in Stockholm. Prof. Tabellini's lectures sparked in me the initial interest for Political Economics. And I still remember the shock when Torsten Persson, from being a name on a book, an extraordinary book, a masterpiece that I was completely fascinated for and could almost quote by heart at the time, became unexpectedly a voice on the phone, asking me, Mema Perrotta, if I wanted to come to Stockholm and do the PhD. I had to sit down on the floor some minutes.

Almost at the end of the course work, a couple of years into the PhD, prof. Jakob Svensson's lectures opened my eyes on an interesting field for apply-

[1] Adams, C. and E. Zotti (1988). *The straight dope.* Ballantine Books.

ing those Political Economy theories to questions that were, in my view, extremely important and urgent: the questions of development, poverty, hunger, how to help. I asked him to be my advisor, and he said he would be happy to. Although perhaps I have not been the perfect match for his personal supervision style, for my part it was an excellent choice. Prof. Svensson has been a precious advisor. None of these four essays would have come even close to look like papers in Economics, had it not been for his indispensable input.

Many of the ideas developed in these four papers, and many many more that were never developed, originated during my only spurt of creativity so far, which happened during the fourth year. I was fortunate to have as co-advisor, during that only year, Ethan Kaplan. We would meet weekly, usually over ärtsoppa and pannkakor at Lantis. "Every week you have to come with 5 new ideas, it doesn't matter if they are completely crazy and we trash them directly. Maybe one out of hundred will become something." This was more or less his method. I guess it worked out. Ethan was an invaluable presence at the Institute, I am very grateful to have met him and have had the opportunity to work with him.

I also acknowledge many inspirational conversations, during this creative period, with Heng Henry Chen, Jan Klingelhöfer, Anders Åkerman and prof. Philippe Aghion when he was visiting the Institute.

Two of the papers in this book are coauthored. I learned extremely much from my first coauthor, Emmanuel Frot, then at SITE, Stockholm School of Economics. Writing together with someone more experienced is the best way to go, and I feel this is the best piece of advice I can give to someone who has just started or plans to start a PhD in Economics (someone, in other words, who has not listened to my *first* best piece of advice). Later on, writing with a fellow student is also a good alternative, definitely faster and more fun than sitting alone with your computer. I am grateful to Maria Cheung for being a good coauthor, driven, generous with her time, definitely fun!

A big thanks to Christina Lönnblad is in order for excellent editorial advice towards the final shape of these papers and all-encompassing support ever since day-one at the Institute. Thanks also to Annika, Åsa, Astrid, Hanna and Ingela, who were always there for me, even if this was not in their job description.

Now to someone that did not help at all with research or with the successful completion of this thesis, but a lot with being able to carry on during all these years of student life, starting with my classmates. An unusually social batch. I want to thank in particular "the girls", Gülay Özcan, Sara Formai, Maria Cheung, Carol Li-Ju Chen, Linnea Källqvist, Marie Gartell, Ylva Johansson, for many (though not really enough!) dinners and nights out. David von Below deserves special mention for his kindness and niceness in general, but also in particular for Stata and LaTeX support. Christina Håkanson and Ettore Panetti, with whom I shared the office on the eight floor during the last times, consistently provided nice faces and high spirits. I also wish to remember, as expressly requested on facebook, my previous office-mate on the ninth floor, Camilo Von Greiff - who by the way also introduced me to innebandy, my plan-b career in case it does not work out as economist - and all the other fellow students at the Department of Economics, IIES, SOFI and HHS. And how to forget the wine tasting club, Gülay Özcan, Sara Åhlén, Anders Fredriksson and Anders Åkerman, and the Italians in and out the PhD program, Pamela Campa, Elena Mattana, Tiziana Melchiorre, Caterina Mendicino, Dario Caldara and others that have come and gone over the years.

Did I have any friends outside university? The answer would be no if it was not for some extremely patient individuals who put up with being almost ignored by me for months in a row and still turn up when I called on them: Caterina Mecca, my very special childhood friend and phone pal, Pol Ilag, with whom I shared many inspiring conversations on human nature over tea, Chris Shioya and Natali Barbaliou, always available for fancy dinners, Sam

Asazu, someone to count on and Jonathan Klasén, to whom I owe, among other things, the best ski-trip ever!

Thanks to my family, my parents Vita and Rocco and my siblings Francesca and Giampaolo, to whom I talk almost daily. They had to bear with all my ups and downs, always putting up with my evening mood - end of day tiredness and frustration, irritability and impatience - always cheering me up and making me feel cherished. You are the best family anyone could wish for!

Finally, to someone who has come along relatively recently but has quickly become an important source of support, happiness and meaning of life. Martin, min kära, jag älskar dig!

Thanks to you all for making these six years ways better than the unobservable counterfactual where I did not meet any of you. All remaining errors are my own.

Stockholm, December 2010

Mema Perrotta

Table of Contents

Chapter 1

Introduction

This thesis consists of four essays in the areas of Development and Political Economics. Although they are quite different both in terms of the questions investigated and the methods of analysis, there are some common themes.

The first one is development. Two essays, chapters 3 and 4, look at one specific area of development, crucial both at the individual and at the society level, namely education. The other two, chapters 2 and 5, consider the broadest and most general characterization of the concept of development: economic growth. Chapter 2 looks at economic growth as an outcome, and asks if foreign aid, one of the many bonds between richer and poorer countries, helps foster economic growth. The last chapter investigates, instead, the effects of economic growth on another development outcome: poverty reduction.

Both these questions are far from novel, and these essays build on two strands of literature that are very large but nevertheless do not have all the answers. In particular, the question on aid effectiveness has been debated, with different approaches and methods, for over three decades now, but is still unsettled. To isolate the effect of aid on growth, researchers have to deal with the familiar problem of reverse causality. Aid is to a larger extent allocated to poorer and worse performing countries, so that low growth "causes" high aid

quantities. This simple observation makes the causal link from aid to GDP growth impossible to establish by looking at simple correlations between these two variables. To observe the causal effect of aid, researchers have used instrumental variable techniques. Instruments must be *exogenous* to growth, i. e. not have any influence on growth except through aid flows, and explain aid flows well. The paper "Aid Effectiveness: New Instrument, New Results?", presents a new instrumental variable to isolate the unconfounded effect of aid on growth, which we argue improves upon past approaches. We are glad to be able to offer, on this basis, a positive answer to the research question: yes, aid contributes to growth in recipient countries, although the effect is very small.

The debate regarding the relationship between economic growth and poverty reduction is of a different nature. The average positive relationship is established and well known. What is interesting to investigate in this case is how this relationship varies *beyond averages*,[1] how it changes under different conditions. We would like to know why the effect of growth on poverty is so much larger in some economies than in others. The main contribution of the paper in chapter 5, "Constitutions and the Growth-Elasticity of Poverty", is to investigate for the first time how the relationship between growth in average incomes and the share of absolute poor in the population changes with a country's constitution. There are reasons to believe that the shape of the constitution might be one factor that affects this relationship. These reasons can be found in another very big and expanding strand of literature, the one on the economic effects of constitutions. This area of research builds on a very long and honored tradition in political science, but is relatively recent within economics. During the last 15 years, and just between Stockholm and Milan[2], this literature went through a systematization and a big push forward thanks to the works of Torsten Persson and Guido Tabellini.

[1] Banerjee (2008)

[2] Incidentally, Bocconi Universtity in Milan and Stockholm University are the two institutions where I got my undergraduate and graduate education in economics.

Based on this literature, that highlights how economic policies and other long-term outcomes are affected by the details of a country's constitution, I can formulate and test hypotheses on how the shape of the constitution can be expected to relate to the item of debate: the extent to which economic growth "trickles down" to lift low income earners out of poverty. The main result of the empirical analysis conducted here concerns two aspects of the constitution: the electoral rule and the form of government. Poverty appears to be less responsive to growth in countries where a majority of the legislators are elected under plurality rule. On the other hand, poverty appears to be more responsive to growth in countries with a constitutional arrangement that tends to result in a strong executive. These findings suggest that the type of incentives provided to the political leaders by the constitutional arrangement, and the resulting types of policies, can make the efforts for poverty reduction more or less effective. This should raise awareness that, in some circumstances more than in others, it is necessary to invoke interventions in support of the poor to complement pro-growth policies.

The second running theme is development assistance. Official development assistance (ODA) is defined as the flows of financing administered with the promotion of the economic development and welfare of developing countries as the main objective.[3] The essays in chapters 2 and 4 look at development assistance from two different perspectives. The first essay, as mentioned, investigates the effects of aid in its generality, as an accounting measure: the raw sum of everything that enters this flow of money, goods and services between the high-income and low-income countries. This approach has been criticized, on the point that, being different components of ODA so heterogeneous in nature, aim and function, it is not only difficult but also misleading to search for an aggregate effect. More potential, is argued, lies in the question about specific projects. Interventions in a restricted geographic area with well defined inputs, outputs, objectives, subjects affected

[3]Marriott (2004).

and so on, are more suitable for evaluation. In particular, such interventions more easily meet the requirements for rigorous scientific evaluation with the method, recently adopted but already unmistakably dominant in the area, of the randomized trial.

Both the approaches have their respective strengths and limitations. The latest trend in the debate around them, quite hot at the moment, stresses the importance of theoretical thinking and modeling in support of the empirical analysis.[4]

The intervention that we evaluate in chapter 4, "The Impact of a Food For Education Program on Schooling in Cambodia", is not a randomized trial but a so-called natural experiment, because a conscious design created a setting that mimics a randomized trial. The program, which consisted of meals served in school, take-home rations for poor girls and deworming programs conditional on school attendance, was phased-in over time in geographically close and similar regions, creating a source of variation that can be exploited to ask counterfactual questions. With all the limitations and caution of the case, we come to the conclusion that the program boosted school enrollment in the short-run, but the large first-year impacts faded out when the intervention was expanded over time. Beyond enrollment, the intervention also increased the probability that the children stayed in school after one or two years, but in most cases it did not lead to higher education achievements. All of this might hint at the presence of negative countervailing effects of the type described by Acemoglu (2010) (general equilibrium effects and political economy responses).

We also try to fit this contribution into the "What works in development?" agenda. Many interventions have been tried in developing countries to improve education outcomes for the new generations. Food for Education programs, or at least the one implemented in Cambodia, are perhaps not the most effective and cheap way to attract more children to school or make them

[4]See Acemoglu (2010), Deaton (2010).

stay longer. However, they fill at the same time another function, that cannot be underestimated: they alleviate hunger, improve nutrition and health status for poor children. We did not consider these outcomes in the present paper. The impact on nutrition and general health, equally important if not more for poor and malnourished children, remains to be investigated.

The third common theme is education. Two of the UN "Millenium Development Goals" are related to education. This is but one example of the enthusiasm of development policymakers for education, which clearly stems from the potential role of education in improving the welfare of people living in developing countries. In chapter 4, as mentioned, we focus on the private decision about investment in education. In many countries, this is still severely constrained, especially for poor people. This can create a sort of trap[5] by which poverty and underdevelopment perpetuate themselves.

Education, or the human capital of the population, is also an asset for a country. Policy makers at the national level and international actors are aware of this, and encourage investments in the education sector in developing countries. Many aid-giving institutions, the World Bank and UNESCO among others, encourage university provision as a means to general aims such as expanding enrollment, preventing the brain drain and laying the basis for the scientific R&D sector. Recently, the Inter-American Development Bank suggested in a report that increased access to higher education could be a route to reduce inequality in Latin America. However, when it comes to public expenditures, political economy factors can and often do enter the picture to complicate things. It happens in rich countries and developing countries alike.[6] The burgeoning empirical literature on distributive politics is a major growth area in the field of political economy. The second essay in chapter 3, "Hidden Redistribution in Higher Education" fits in this lit-

[5]On this and other "traps", see the works of Dasgupta (1997, 1998).

[6]Besides the literature for developing countries quoted in the paper, see for example Golden and Picci (2008) about Italy and Dahlberg and Johansson (2002) about Sweden. A longer list is provided in Kramon and Posner (2010).

erature. It moves from the observation, well known but surprisingly little investigated, that some African countries' expenditures in higher education are disproportionate in levels, with respect to both the demand for this level of education (the number of students), and the average incomes in these countries. The paper tries hence to find out if further motives are reflected in these levels of spending, in particular, ethnic favoritism. The answer in the paper is both positive and negative: there is trace of ethnic patronage in the variation of expenditures, but the extent of this effect is not sufficient to explain the African "spending anomaly".

Bibliography

Acemoglu, D. (2010). Theory, General Equilibrium, and Political Economy in Development Economics. *The Journal of Economic Perspectives 24*(3), 17–32.

Banerjee, A. (2008). Big answers for big questions: the presumption of growth policy. In *Brookings conference "What Works in Development"*.

Dahlberg, M. and E. Johansson (2002). On the vote-purchasing behavior of incumbent governments. *American Political Science Review 96*(01), 27–40.

Dasgupta, P. (1997). Poverty traps. In *Advances in economics and econometrics: theory and applications: seventh world congress*. Cambridge University Press.

Deaton, A. (2010). Understanding the mechanisms of economic development. *The Journal of Economic Perspectives 24*(3), 3–16.

Golden, M. and L. Picci (2008). Pork-Barrel Politics in Postwar Italy, 1953–94. *American Journal of Political Science 52*(2), 268–289.

Kramon, E. and D. Posner (2010). Who is Favored? How the Outcomes One Studies Affect the Answers One Gets.

Marriott, F. H. C. (2004). *OECD: A dictionary of statistical terms (5th ed.).* Harlow, UK: Longman Scientific and Technical.

Chapter 2

Aid Effectiveness: New Instrument, New Results?*

1 Introduction

Foreign assistance has been disbursed for decades and is today still seen as a major tool of development policy, and while all promises of increasing aid flows are likely not to be fulfilled, the trend is clearly towards an expansion. If there seems to be near unanimity among policy makers about the positive role of aid,[1] the academic community has not found any robust evidence that

*This paper is coauthored with Emmanuel Frot, SITE/Stockholm School of Economics. The authors thank Ethan Kaplan, Jakob Svensson, Rajeev Dehejia, Philippe Aghion, Pamela Campa, Martin Berlin and all the participants at the IIES and Economics Department seminars at Stockholm University.

[1]For instance, the Monterrey Consensus, adopted by Heads of State and Government after the 2002 United Nations International Conference on Financing for Development, states that "Official Development Assistance (ODA) [...] is critical to the achievement of the development goals and targets of the Millennium Declaration", that "we recognize that a substantial increase in ODA and other resources will be required if developing countries are to achieve the internationally agreed development goals and objectives, including those contained in the Millennium Declaration", and that "we urge developed countries that have not done so to make concrete efforts towards the target of 0.7 per cent of gross national product (GNP) as ODA to developing countries".

aid contributes to development.[2] The aid effectiveness literature is large and mostly inconclusive. The results vary widely in size and sign, and have often been proven not to be robust and often reversed by new estimations. The so-called third generation of aid and growth studies, which established some influential and widely cited results in the 90s, has recently been criticized, mainly on two points: the unsatisfactory instrumentation strategies and the "black box" way in which they use General Method of Moments (GMM) estimations.[3] These two points relate to the two fundamental issues the researcher must confront when designing an empirical strategy to deal with the question of aid effectiveness. The first is the identification of the causal effect of aid on growth, unconfounded by simultaneity and reverse causality. The second is the consistent estimation in a dynamic panel setting. We offer our main contributions on these two points.

We propose a new instrument and argue that it is a significant improvement relative to past approaches. It takes the "supply side" approach, that makes use of variables linked to the aid allocation process (mostly historical and political variables), one step further. Our identification strategy is similarly based on predicted aid flows; however, unlike existing studies, we exploit a source of variation that we argue not to be subject to the same criticisms, namely that of being directly correlated with the outcome. This source of variation is related to the temporal order in which donor-recipient partnerships are established: Frot (2009) shows that *when* a partnership is established and *how long* it lasts are of importance for aid quantities. In addition to being exogenous to growth, we show that our instrument is highly correlated with actual aid levels.

On the second point, we keep our estimation strategy as simple and trans-

[2]Many authors argue that aid failed to achieve growth. Easterly (2006) gives a detailed presentation of the arguments. Easterly (2007) summarizes them in an article entitled "Was Development Assistance a Mistake?".

[3]Bazzi and Clemens (2009) make this point very effectively and provide many examples. In releasing his Stata package to perform GMM estimations, Roodman (2009a) warned about the risks of using it unwittingly.

parent as possible. Given that standard panel estimators (fixed effect estimators) are biased in dynamic settings, we make use of the GMM estimators in order to account for individual level fixed effects. But we rely exclusively on our "external" instrument for the identification of the aid coefficient. In addition, we test the validity of the instruments created by the GMM procedure and, as a consequence, we are able to comment on the validity of the GMM approach to estimate aid efficiency.

To give a preview of the results, we find a significant and moderate effect of aid on growth: in our sample, a 1 percent increase in received aid is associated with a 0.06 to 0.13 percent higher growth rate. This effect is one order of magnitude smaller than what has previously been estimated. Such a small effect might easily be missed or confused in the normal cycles of growth, so this might help explain why estimates have been so diverse in the past.

The paper is organized as follows: in the next section, we spell out what are the empirical challenges that the question of aid effectiveness presents, and highlight how the literature has dealt with them in some important contributions. In Section 3 we describe in detail how our instrument is built; we then briefly discuss our methodological choices in terms of estimators and present the results in Section 4. In Section 5 the robustness of the results is assessed. Section 6 concludes the paper. All variable definitions and data sources are to be found in a data appendix at the end of the paper.

2 Estimation pitfalls and previous literature

A vast literature has focused on the effect of aid on GDP growth, controlling for various variables. Some version of the following equation is implicitly or explicitly derived from a standard growth model a la Solow, and brought to

the data:

$$\Delta y_{it} = \alpha_t + \beta y_{it-1} + \gamma a_{it-1} + \sum_k \delta_k x_{kit} + \mu_i + \xi_{it}. \quad (1)$$

In equation (1), i and t respectively index the countries and time periods (five-year intervals, usually), y is the (natural logarithm of) GDP, and Δ indicates its variation, an approximation for the growth rate, α is a constant that might change over time, γ is the coefficient of interest, the effect of aid a (also in logs), x_k are additional explanatory variables, and the error term consists of an unobserved country-specific effect μ_i and a random noise ξ_{it}.[4]

To estimate this equation, researchers have to deal with the familiar problem of reverse causality. Aid is to a larger extent allocated to low performing countries, such that low growth 'causes' high aid quantities. This simple observation makes the causal link from aid to GDP growth impossible to establish by looking at simple partial correlations between these two variables. To observe the causal effect of aid, researchers used instruments that must be exogenous to growth and explain aid flows well. Rajan and Subramanian (2008) and Bazzi and Clemens (2009), among others, review the past literature and question the validity of the instruments used in past studies. Table 1 lists the instruments used in four influential papers.[5]

These papers typically instrument aid with many variables without any clear identification strategy. Burnside and Dollar (2000) explain that theirs is based on the aid allocation literature, the so-called *supply-side* approach, but it is difficult to argue that any of their instruments satisfies the required

[4]Most papers in the literature estimate the effect of aid, expressed as a share of GDP, on growth. We prefer to use aid levels, for reasons exposed later, and for consistency with the literature also run our regressions using aid as a share of GDP in Section 5.

[5]The instruments used in Boone (1996) are found in Table 4 of his paper, those of Burnside and Dollar (2000) in Table 1 and those of Hansen and Tarp (2001) in Table 1. Dalgaard et al. (2004) reproduce previous specifications but their own set of instruments is found in Table 3 of their paper, Clemens et al. (2004) use the same set of instruments as Hansen and Tarp (2001).

Table 1: Instruments in the aid effectiveness literature

Boone (1996)	Burnside and Dollar (2000)	Hansen and Tarp (2001)	Dalgaard et al. (2004)
Log population	Log of initial income	Egypt dummy	Aid (-1)
Friends of US	Policy index	Arms imports (-1)	Aid^2 (-1)
Friends of OPEC	Log population	Policy (-1)	Aid*inflation (-1)
Friends of France	Arms imports/Tot. imports, (-1)	$Policy^2$ (-1)	Aid*openness (-1)
Aid (-2)	Sub-Saharan African dummy	Policy*Log population	Aid*share of land in tropics (-1)
	Egypt dummy	Policy*Initial GDP per capita	M2/GDP (-1)
	Franc zone dummy	Policy*Initial GDP per $capita^2$	Budget surplus (-1)
	Central America dummy	Policy*aid (-1)	Inflation (-1)
	Policy*aid^2 (-1)	Openness (-1)	
	Aid(-1)		
	Aid^2 (-1)		

Note: Instrumental variables for aid used in four influential papers. -1 and -2 indicate lags.

exogeneity assumption. Deaton (2010) criticizes the whole literature by mentioning that neither the Egypt dummy nor population, though they are aid determinants, can plausibly be exogenous. These variables are *external* to growth but assuming that they do not have any influence on growth except through aid flows is not very plausible. Moreover, the Egypt dummy is problematic as the source of variation is unlikely to teach us anything about the effect of aid on growth in a general way. The variation between Egypt and non-Egypt countries, or for that matter between Franc-zone countries and non Franc-zone countries, is not very useful. Unfortunately, similar criticisms apply to all instruments listed in Table 1. None of them is exogenous to growth. Even the fraction of land in the tropics, used by Dalgaard et al. (2004), is correlated with institutions which, in turn, affect long-run development, as shown by Acemoglu et al. (2001). Lagged aid variables, either interacted with other exogenous regressors or not, also constitute a dubious choice if growth is serially correlated. Similarly, the assumption that a control such as *policy* has a contemporaneous effect on growth but none in the next period, except through aid, is hard to defend.

Rajan and Subramanian (2008) recognize these issues and adopt a slightly different approach based on donor-recipient pair characteristics, instead of using recipients' characteristics. Donors choose aid allocation based on poverty considerations, but also because of history and influence. The authors here

capture historical relationships through colonial links and commonality of language. Influence is proxied by the relative size of the donor and the recipient. The larger the donor, the larger its influence. Relative size is also interacted with historical variables as influence is likely to be further increased if historical links are strong. Aid quantities are estimated at the donor-recipient level and then summed across donors to find the recipient predicted aid quantity. Rajan and Subramanian (2008) then use this instrument to revisit most of the past evidence on aid effectiveness and find little robust evidence of any link between aid and growth.

This identification strategy improves upon past studies but is still not entirely convincing. Historical variables are unlikely to be exogenous to growth and are correlated with traditional growth determinants, as shown by Bertocchi and Canova (2002). Acemoglu et al. (2001) have also demonstrated how colonial origins are of importance for growth through institutional quality. A second concern with the instruments of Rajan and Subramanian (2008) is their limited variation since historical variables are simple dummies. In addition, they still include population in their set of instrumental variables, despite its drawbacks. In fact, Bazzi and Clemens (2009) show that their identification almost exclusively relies on population size because the other instruments are weak, to the point of being irrelevant. Therefore, Rajan and Subramanian (2008) face the same problem of invalid instruments as earlier papers.

A second challenge for the researcher is the fact that the process of economic growth calls for a dynamic model, in which current values depend on past realizations. This is why the lag value of income figures as a regressor in equation (1). One immediate problem in the estimation of such a model is that lagged values of the dependent variable (and potentially of the other regressors) are correlated with the fixed effect in the error term. This makes the OLS and 2SLS estimators inconsistent.[6] Sure enough, the fixed

[6]Only the coefficient on income is plagued by this problem. On the other hand, the

effect estimator is consistent; but with five-year intervals over forty years of data it is not possible to rely on asymptotic properties[7], although this point has often been overlooked. To deal with this issue, researchers have made use of a class of estimators built for the purpose, namely the GMM estimators. This procedure consists of first-differencing the data, as opposed to the fixed effect transformation that demeans them (subtracts the sample mean). Endogenous variables are then instrumented using their own lagged values. The main advantages of these estimators are that they deal with individual level fixed effects without incurring the bias to which standard panel estimators (chiefly the fixed effect transformation) are subject in dynamic settings. Furthermore, they offer "internal" solutions for dealing with endogenous regressors. In particular, the Arellano and Bond (1991) original "difference" estimator instruments for current period differences in endogenous variables using their own multiple lagged levels. The more efficient Blundell and Bond (1998) "system" estimator, which exploits the moment conditions from a system of the differenced equation plus the original level equation, additionally instruments for current period levels using lagged differences. This wealth of plausibly valid instruments is never submitted to the standard weak-instrument diagnostics, so there is no guarantee for their relevance; and the problems for inference of using many weak instruments are very serious and very well known.[8] Moreover, the exclusion restrictions on which these methods rely are more demanding than what is often assumed (in particular for the "system" method; see Roodman (2009a) for a discussion of these issues).

Our approach is hence to exclusively rely on our external instrument for the identification of the aid coefficient. Endogenous variables for which

presence of one inconsistent coefficient also biases the other coefficients in the regression, moreover in a direction that is difficult to predict.

[7]Asymptotics require $t \to \infty$, while here we have $t = 8$ at most!

[8]See Stock and Yogo (2005) and Staiger and Stock (1997). Stock and Wright (2000) and Bun and Windmeijer (2010) look at this issue in the context of GMM estimators.

we do not have an external instrument[9], mainly income, are instrumented using their lagged values, but we are very careful in keeping the number of instruments as low as possible by collapsing the instrument matrix, as recommended in Roodman (2009a). Moreover, in Section 5, we replicate the GMM instrumentation in a traditional IV setting, so that we can use the whole standard battery of tests for instrument strength. In the absence of a test for instrument strength in a GMM setting, this approach is used by Bazzi and Clemens (2009), following Blundell and Bond (2000), Bun and Windmeijer (2010) and Roodman (2009a). The "difference" estimator has often been criticized on the grounds that it is biased because of weak instrumentation. It was then recommended, as in Bond et al. (2001), to use the "system" estimator, which is considered to be more robust to weak estimation. However, recent research (see Bun and Windmeijer (2010) and Hayakawa (2007)) suggests that "system" GMM estimators may not fare any better and can be seriously biased. An additional contribution of the paper is therefore to assess the validity of the GMM approach in the aid effectiveness literature. In addition to not taking instrument strength for granted, we also statistically test the exclusion restrictions on which the "system" estimator relies. Papers on aid effectiveness typically eschewed these tests, whereas the restrictions are far from trivial.[10] Our results cast serious doubts on the ability of GMM estimators to identify the relevant effects, and suggest that the (consistent) two-stage least squares estimator, biased but free from weak instrumentation issues, should be considered first.

[9]We use partnership characteristics to build instruments for aid but also for trade flows, see sections 3 and 5.1.

[10]On the other hand, Bond et al. (2001) argue that they must be satisfied when estimating a Solow growth model.

3 The instrument

This section focuses on describing in more detail our new instrument, which is the main contribution of this work.

3.1 Design

Total aid A_{it} to recipient i in year t can be decomposed as

$$A_{it} = \sum_j s_{ijt} D_{jt}, \tag{2}$$

where donors are indexed by j, D_{jt} is j's total aid budget in year t and s_{ijt} is the share of this budget allocated to recipient i. Each donor-recipient pair (i, j) in a given year t is characterized by two features: the date when the partnership was established, and how long this partnership existed. The latter is the difference between t and the entry date and is referred to as τ_{ijt}. We call κ_{ij} i's entry date position in an ordered sequence of all partnerships established by j. For instance, $\kappa_{ij} = 1$ for recipients that received aid from j in the first year j started to give aid, and so on.[11] More formally, define η_{ij} as the first year j gives aid to i and π_j as the first year the donor disburses aid to any country. The entry date order κ_{ij} is then defined as

$$\kappa_{ij} = \eta_{ij} - \pi_j + 1. \tag{3}$$

Donor portfolio expansion implies that aid shares are bound to fall on average. In order to make aid shares neutral with respect to portfolio size

[11] To be precise, our data only starts in 1960, so the ordered sequence of recipients' cohorts is approximate. This is a data limitation which is akin to censoring, but on an independent variable; the econometric literature has surprisingly little to say about how to deal with this issue; see Manski and Tamer (2002) and Rigobon and Stoker (2009) for contributions on this issue.

we define normalized aid shares σ_{ijt}

$$\sigma_{ijt} = s_{ijt} - \frac{1}{N_{jt}}, \tag{4}$$

where N_{jt} is the number of recipients that have received aid from donor j at least once before year t. Normalized shares are hence deviations from an equal sharing rule among all recipients.

Predicted aid shares are then the OLS fitted values of

$$\sigma_{ijt} = a + b\kappa_{ij} + c\tau_{ijt} + u_{ijt}. \tag{5}$$

Predicted aid shares for any observation (i.e. a given (i, j) pair in a given year) are fully defined by their entry date order and partnership length. In other words, to any partnership characterized by entry date of order κ and length τ we associate a predicted aid share $\hat{\sigma}_{\kappa\tau} \equiv \hat{a} + \hat{b}\kappa + \hat{c}\tau$. $\hat{\sigma}_{\kappa\tau}$ is not related to i, j, or t: it is the *typical* share (in fact, the average share) that any recipient gets from a donor if their partnership was established in the κ^{th} year of activity of this donor, τ years ago.

The instrument for aid is the predicted aid quantity

$$\hat{A}_{it} = \sum_j \hat{\sigma}_{\kappa_{ij}\tau_{ijt}} D_{jt}. \tag{6}$$

In words, we first estimate the predicted aid share each donor allocates to each recipient, based on the pair characteristics. We then multiply these predicted aid shares by the donors' aid budgets to obtain a predicted aid quantity for each recipient. The intuition is as follows. The instrument artificially recreates a situation where a country receives more aid in a given period, independently of the "fundamentals" of its economy, but rather for one or more of the following reasons: because it on average had an earlier order of entry with respect to other recipients in the donors' portfolio; because it was in the (average) partnership for a longer period of time; finally, because

the (average) donor's budget for aid happened to be larger that year. Unlike actual aid A_{it}, $\hat{A}_{it}$ is not influenced by shocks to economic performance in the recipient country,[12] so it is not affected by reverse causality; moreover, we will argue in the following section that it is a strong instrument, relevant for predicting actual aid flows, and that its only effect on growth occurs through the actual aid flows it proxies.

3.2 Properties

For $\hat{A}_{it}$ to be a good instrument, it must be the case that entry date order and length are strong determinants of aid shares. Frot (2009) shows that this is indeed the case, and we reproduce some of his results here. Using data on aid recipients, we group recipients into six cohorts based on entry dates: recipients with an entry date of one, then with entry dates between two and five, six and ten, eleven and fifteen, sixteen and twenty, and above twenty one. Figure 1 presents the average normalized share received by recipients in each cohort in each year.[13] In other words, Figure 1 shows how much recipients in each cohort get in deviation from equal sharing.

As shown by the figure, early entrants into donors' portfolios are on average receiving larger aid shares. There is some convergence across cohorts but even many years after portfolios have been formed, it is still the case that entry dates and aid shares are correlated. Stratification by cohorts is visible in any year, and seems to have reached a certain persistence level.

[12]On the other hand, it is affected by shocks to the donor's economy, through the aid budget. For example, a boom year for one or more donor countries can lead to larger aid budgets and at the same time larger trade flows; if some of the recipients are also trade partners, which is often the case, we might erroneously attribute to aid the beneficial effects that come from other channels. However, we think that year effects do a good job of controlling for these instances. Moreover, in the robustness checks, we control for trade, which we consider to be the main potential alternative channel from having a partnership to growth.

[13]Donors enter the market in different years, and sometimes exit the market. These changes make comparing the cohort averages difficult, so for Figure 1 we restrict the sample to donors that have been present from 1960 to 2007.

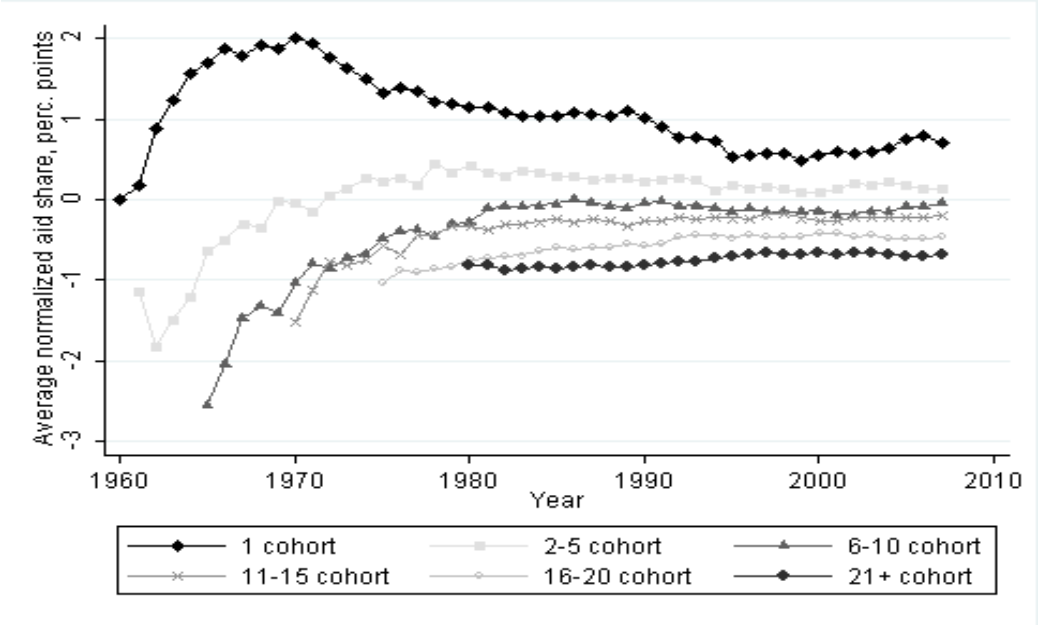


Figure 1: Average aid share in deviation from equal sharing, by recipient cohort

Figure 1 does not alone offer enough evidence that entry dates play a decisive role in determining aid shares, neither does it exclude the case that other factors are behind the correlation between entry date order and aid receipts. It is likely that donors created partnerships that prioritized poor countries and heavily populated countries, and that such countries have received larger aid shares because of these characteristics, and not because of their entry dates. However, Frot (2009) also shows that the explanatory power of entry dates is robust to controlling for these characteristics. In order to disentangle these different possible effects, the normalized aid share of each recipient is regressed on a set of controls. The following equation is estimated:

$$\sigma_{ijt} = \alpha + \beta\tau_{ijt} + \gamma\tau_{ijt}^2 + \delta\kappa_{ij} + \boldsymbol{x_{ijt}}\boldsymbol{\varphi} + \varepsilon_{ijt} \tag{7}$$

where κ_{ij} is entry date order, τ_{ijt} is the number of years the partnership has existed ($\tau_{ijt} = t - \eta_{ij} + 1$), $\boldsymbol{x_{ijt}}$ is a vector of controls including recipient

GDP per capita, recipient population size, a dummy variable for whether donor and recipient shared a colonial relationship, and the distance between i and j, and ε_{ijt} is an error term uncorrelated with the independent variables. The variable τ_{ijt}^2 enters the equation to allow for convergence among countries with different entry dates. The exact functional form of the dependence of the normalized share σ_{ijt} on κ_{ij} is debatable. Equation (7) assumes that it is linear. Figure 1 suggests something more complex, with a falling effect of entry dates on aid shares (curves get closer when one moves downward vertically). To capture such non-linearities we also estimate equation (7) by adding κ_{ij}^2 as a regressor. Table 2 presents the results. Column (1) shows that entry dates are indeed affected by recipient and recipient-donor characteristics, as expected: donors did prioritize countries with a larger population, lower GDP per capita, geographically closer to them and countries with which a colonial relationship had been in place.

The remaining columns indicate that, as suggested by Figure 1, earlier entrants indeed receive larger aid quantities, even after controlling for such recipient and recipient-donor characteristics. Columns (4) and (5) acknowledge the censored nature of aid shares that are bound to lie between 0 an 1, and thus present censored regression estimates. The effects are sizable. Consider two hypothetical aid recipients A and B from the same portfolio. A and B's characteristics are identical, except that A's entry date is one and B's is ten (corresponding roughly to a one-standard deviation difference). The difference in A and B's aid shares in year 20 (20 years after they started receiving aid) is 0.99 percent using estimates from column (3), and 1.45 percent from column (5). This is as large as between a quarter and 40 percent of the standard deviation of the shares distribution. To put this number into perspective, we compare it with the GDP differential that would result in such a difference. In other words, for B to have the same aid share as A, how much smaller should its per capita per capita GDP be? From the estimates of Table 2, B's income per capita would have to be USD 7071 to 5814 lower

Table 2: Determinants of aid shares

	(1) Entry	(2) Aid share	(3) Aid share	(4) Aid share	(5) Aid share
GDP per capita	.00036***		-.00014***		-.00025***
	(.000065)		(.000018)		(.0000075)
Population, mil	-.012***		.0055***		.0059***
	(.0010)		(.0013)		(.00013)
Colony	-3.91***		2.69**		2.96***
	(1.04)		(1.03)		(.061)
Distance	.17**		-.057**		-.083***
	(.077)		(.022)		(.0040)
Entry		-.12***	-.11***	-.17***	-.15***
		(.016)	(.019)	(.0041)	(.0054)
Entry, squared		.0030***	.0032***	.0035***	.0036***
		(.00048)	(.00054)	(.00015)	(.00019)
Length		.062***	.091***	.061***	.11***
		(.0084)	(.011)	(.0033)	(.0043)
Length, squared		-.0011***	-.0018***	-.00091***	-.0019***
		(.00017)	(.00020)	(.000079)	(.000096)
Constant	6.98***	-.12	.019	-.44***	-.12**
	(1.03)	(.082)	(.26)	(.033)	(.055)
Observations	71620	132798	71620	132798	71620
Recipients	113	130	113	130	113
Donors	29	56	29	56	29
R^2	.057	.019	.098	.007	.028

Note: Robust standard errors clustered at the donor level in parentheses. Columns (4) and (5) estimate a censored-normal regression. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

than that of A, using columns (3) and (5), respectively. The mean income per capita in the sample is USD 1712, with a standard deviation of USD 2043, so this difference is extremely large. This implies that entry dates have a large effect when compared to per capita GDPs. The small percentage difference is also significant in monetary terms, as it represents between USD 14 and 20 million (in 2006 USD). Entry dates, together with partnership length, are therefore good predictors of aid shares, on top of more traditional determinants of aid. In the next section, we will report more evidence of predicted aid indeed being a strong instrument for aid.

Returning to our question, we are ultimately interested in the effect of aid, as predicted by entry date order, on growth. Hence, we also need to ensure that there are no other confounding effects that go from entry dates to growth through other channels than aid, i.e. that exclusion restrictions are satisfied. For example, it might be the case that early entrants do not only receive more aid, but also larger trade flows, which in turn affect growth. In such a case, we would erroneously attribute to aid the better growth performance observed. A response to this concern is to control for those potential factors correlated with entry dates and affecting growth in the growth regression and show that aid has an independent effect on top of them. This is done for trade flows.[14] We also show that the direct correlation between entry order and growth, although present, is very weak, and there is no strong evidence against the claim that it might come entirely and only through aid.

[14] An issue with directly including trade flows in the estimation of equation (1) is that they, too, are affected by reverse feedback with the growth rate, the left-hand side variable. Our approach is to instrument them, too, in a similar way as we did for aid flows, using entry dates and partnership length. The predicted total trade flows are then included in the equation. Initially, we also took the same approach for inward foreign direct investment flows, but then abandoned this part of the analysis due to serious limitations in the bilateral FDI data.

4 Results

4.1 Preliminary stage

As mentioned above, our strategy consists of first estimating aid shares by regressing actual shares on entry dates and partnership length (and their squares).[15] We then compute predicted aid quantities $\hat{A}_{it}$ by summing up predicted aid shares multiplied by donors' aid budgets. The predicted aid quantity is then used as an external instrument in the "second stage" growth regression (i.e. in equation (1)).

4.2 Baseline results

Table 3 reports the OLS and IV estimation of equation (1) with and without country fixed effects.[16] The equation includes a number of controls which are frequently used in the literature: population size; a measure of schooling[17]; inflation as a measure of macroeconomic policies; liquid assets (M2/GDP), commonly used as a measure of financial depth; institutional quality, measured by the International Country Risk Guide (ICRGE) index; the Sachs et al. (1995) index of openness. We also include ethno-linguistic fractionalization and regional dummies for Sub-Saharan Africa and quickly growing East Asia, when possible. These controls are those most commonly used in the aid effectiveness literature, and allow us to draw comparisons with past studies.

The log-log specification adopted in equation (1) implies that the coefficient on aid is the elasticity of GDP with respect to aid. We start by not

[15]The specification we use to predict aid shares corresponds to Table 2 column (4). The correlation between predicted and actual shares is 46%, 48% between predicted and actual aid quantities.

[16]All regressions in the paper include year effects. Refer to the data appendix for all variable definitions and their sources.

[17]This is the Barro and Lee (2010) average years of primary schooling. Whether we use primary or secondary schooling does not make much difference.

Table 3: OLS and IV regressions

	(1) OLS	(2) FE OLS	(3) 2SLS	(4) FE 2SLS
Log GDP, lagged	-0.020	-0.23***	-0.030*	-0.23***
	(0.016)	(0.047)	(0.018)	(0.050)
Log aid, lagged	0.018	0.017	-0.017	0.10***
	(0.011)	(0.017)	(0.050)	(0.039)
Log population	0.023	-0.16	0.052	-0.23**
	(0.021)	(0.12)	(0.039)	(0.10)
Inflation	-0.10***	-0.11***	-0.11***	-0.10***
	(0.025)	(0.028)	(0.025)	(0.028)
Money, lagged	0.00069	0.0028**	0.0011	0.0029***
	(0.00060)	(0.0013)	(0.00086)	(0.0011)
Schooling	0.00029	-0.0013	-0.0028	0.033
	(0.012)	(0.036)	(0.012)	(0.037)
Institutional quality	0.013**	0.0077	0.014**	0.0042
	(0.0055)	(0.0073)	(0.0056)	(0.0070)
Openness	0.097***	0.075***	0.10***	0.078***
	(0.017)	(0.027)	(0.018)	(0.028)
Ethno. fractionalization	-0.083		-0.097	
	(0.057)		(0.063)	
East Asia	0.014		0.018	
	(0.026)		(0.028)	
Sub-Saharan Africa	-0.020		-0.0086	
	(0.038)		(0.047)	
Observations	347	347	347	344
Countries	61	61	61	58
AP test (p-val)			0.046	0.00088
KP F stat			4.16	12.3
R^2	0.32	0.38	0.29	0.28

Note: AP: Angrist-Pischke. KP: Kleibergen-Paap. DThe dependent variable is the growth rate. All regressions include year effects. Robust standard errors clustered at the recipient level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

instrumenting the aid variable, and present naive estimates, with and without country fixed effects in Table 3. Column (1) confirms the traditional finding that, when not instrumented, aid has no effect on GDP growth. The inclusion of country fixed effects only reinforces this conclusion. However, as argued above, there is little to learn from regressions where aid is not instrumented. We move on to columns (3) and (4) where aid is instrumented using our instrument of predicted aid quantities. Because a major concern in the literature is the weakness of instrumentation for aid, we provide two statistics. The first is the p-value of the Angrist and Pischke (2009) test of excluded instruments.[18] The second is the Kleibergen and Paap (2006) Wald statistic. Both are tests of instrument weakness.

Column (3) is the two-stage least square (2SLS) specification. It fails to find any significant effect of aid on GDP growth, but it is likely that omitted country fixed characteristics make the error term not orthogonal to the control variables, biasing the estimates. In addition, the Kleibergen and Paap Wald statistic is quite low.[19] In column (4), we include country fixed effects to avoid the bias due to their omission. The consequence for the aid coefficient is quite dramatic. It is much larger than in column (2) and comfortably passes the five percent significance threshold. The weak instruments statistics now confirm that our instrument is highly correlated with aid. The null hypothesis of the Angrist and Pischke test is strongly rejected, and the Kleibergen and Paap Wald statistic is much higher than in column (3). These results indicate that the inclusion of fixed effects is important for the validity of our approach.[20] The estimated effect implies

[18] With a single endogenous regressor, this statistic is simply the F-statistic of the first stage.

[19] Although critical values only exist for the Cragg-Donald Wald statistic, which is not robust to heteroskedasticity, the 25% maximal IV size value is 5.53, which suggests that the Kleibergen-Paap statistic is indeed low.

[20] This test is based on the F-statistic of the first stage, so the stark improvement is not surprising: the model including country fixed effects performs much better than that without.

an elasticity of GDP with respect to aid of 0.10. This elasticity is relatively moderate. Another way of interpreting the result is that a 1 percent change in aid increases GDP growth by approximately 0.10 percent. The first-stage regression for the fixed effect regression is shown in column (1) of Table 4. It confirms that predicted aid is a strong predictor of actual aid.

Table 4: First stages

	(1) Aid	(2) Aid	(3) Trade
Log GDP, lagged	-0.088	-0.11	1.07***
	(0.27)	(0.27)	(0.13)
Log predicted aid, lagged	1.42***	1.69***	-0.025
	(0.41)	(0.40)	(0.13)
Log predicted trade, lagged		-1.12***	1.06***
		(0.35)	(0.21)
Log population	0.63	0.71	-0.31
	(0.70)	(0.69)	(0.25)
Inflation	-0.073	-0.078	0.0097
	(0.10)	(0.10)	(0.048)
Money, lagged	0.0016	0.00067	0.0068***
	(0.0052)	(0.0052)	(0.0025)
Schooling	-0.28*	-0.30*	0.047
	(0.16)	(0.16)	(0.10)
Institutional quality	0.056***	0.056***	-0.0099
	(0.021)	(0.019)	(0.015)
Openness	0.053	0.023	0.11
	(0.13)	(0.13)	(0.081)
Countries	58	58	58
R^2	0.35	0.37	0.78
Observations	344	343	343

Note: Column (1) is the first stage of the regression in Table 3 column (4). Columns (2) and (3) are the first stages of the regression in Table 7 column (4). The instruments for aid and trade are built from fitted values of the preliminary stage estimated at the bilateral level, and then aggregated at the country level. All regressions include country and year effects. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results in Table 3 are problematic because of the correlation between lagged income and the error term, due to the strong persistence in income and the individual specific component in the error term. We can sign this bias for the lagged income coefficient, but not so easily for the other variables. It is nevertheless useful in order to evaluate the performance of the GMM estimator. In the OLS setting, the coefficient on lagged income is upward biased, whereas Nickell (1981) proved that the within group estimator is downward biased. We know that the true coefficient lies somewhere in this range, and this remark allows us to evaluate if the GMM estimator succeeds in removing the bias. In columns (1) and (2) of Table (5), we rely on the difference GMM estimator in order to remove the dynamic bias, in asymptotic terms. This method estimates the model in differences, to get rid of the fixed effects. The lags of endogenous regressors, which are exogenous to the first difference of the error term, are used to instrument for their first difference. In column (1), we instrument aid with its lags, as is usually done in the literature. In column (2), we use our instrument for aid. Both coefficients are insignificant, but the estimates of the GDP coefficient cast serious doubts on the validity of the approach. The first-differenced GMM estimate is well below the within groups estimate of Table 3, which can already be expected to be strongly downward biased, given the small time dimension of the dataset. This signals that the GMM estimate is also biased, possibly because of weak instruments.[21] The first-differenced GMM estimator is therefore not very informative and for this reason, we use the system GMM estimator in the next two columns. This is a more efficient method, developed by Blundell and Bond (2000); it uses the moment conditions from the same difference equation *but also* from the original level equation at the same time. This method is valid under the assumption that the GMM instruments (i.e. the lagged differences) are exogenous to the error term in the level equation.

[21] Here we mean the GMM instruments. We do not rely on them for the aid variable, and we know that our instrument for aid is actually strong.

This can be tested using a Hansen test, denoted in the regression tables as "level eq."; we report the test p-value. If this test fails, the validity of the system GMM approach is questionable, and the results should be interpreted with caution. Finally, the p-value of the Hansen J test of overidentification is reported; its null hypothesis must not be rejected for the GMM exclusion restrictions to be valid.

The GDP coefficient, both in columns (3) and (4), now lies in the expected range, which confirms that system GMM estimators are more appropriate. Estimates in column (3) do not make use of our aid instrument. The null hypothesis of the exogeneity of the GMM instruments for the levels equation is not rejected by the Hansen test. On the other hand, the number of instruments and countries is of the same order of magnitude, such that the p-value of the test is likely to be upward biased, as underlined by Roodman (2009b). Column (4) instruments aid with our instrument. The coefficient of aid is now significant, although only with a p-value of 6.9 percent. It is also smaller than in Table 3. The Hansen tests of overidentification restrictions and of exogeneity that the GMM instruments for the levels equation fail to reject their null hypotheses, suggesting that the assumptions required for the estimators to be valid are satisfied. Taken together, columns (4) of Tables 3 and 5 indicate an elasticity of GDP to aid between 0.057 and 0.10.

GMM estimators come with several caveats about their validity, however. The first concerns the risk of having too many instruments. Roodman (2009b) showed how Hansen tests tend to fail to reject the null hypothesis when the instrument count is large. A rule of thumb is that instruments should not exceed the number of countries, which is the case in our estimations. Relying on our external aid instrument in column (4) reduces the instrument count, but it still remains close to the number of countries. Roodman (2009b) recommends that the instrument count is reduced as a robustness check. One way of doing this is to collapse the instrument matrix. Estimates with this collapsed matrix are reported in Column (1) of

Table 5: GMM regressions

	(1) Diff. GMM	(2) Diff. GMM	(3) Sys. GMM	(4) Sys. GMM
Log GDP, lagged	-0.38***	-0.54***	-0.016	-0.086*
	(0.12)	(0.14)	(0.027)	(0.050)
Log aid, lagged	-0.014	0.016	0.018	0.057*
	(0.030)	(0.040)	(0.017)	(0.031)
Log population	-0.073	-0.027	0.022	0.057
	(0.14)	(0.16)	(0.033)	(0.047)
Inflation	-0.11***	-0.076***	-0.086**	-0.080**
	(0.033)	(0.027)	(0.034)	(0.038)
Money, lagged	0.0019	0.0030**	0.0016**	0.0016*
	(0.0012)	(0.0012)	(0.00077)	(0.00090)
Schooling	-0.052	-0.047	-0.0072	0.058
	(0.050)	(0.063)	(0.027)	(0.051)
Institutional quality	0.0095	0.014**	0.013**	0.019**
	(0.0073)	(0.0072)	(0.0050)	(0.0085)
Openness	0.080***	0.088***	0.11***	0.11***
	(0.031)	(0.032)	(0.022)	(0.022)
Instruments	60	40	74	48
Countries	58	58	61	61
Hansen J test (p-val)	0.55	0.27	0.76	0.27
Hansen test (p-val), lev.			0.95	0.11
AR(1)	0.016	0.12	0.00081	0.0010
AR(2)	0.57	0.28	0.80	0.67
Observations	286	286	347	347

Note: Instruments for the differences equation are log GDP lagged twice in all specifications, and log aid lagged twice in columns (1) and (3). Instruments for the levels equation are log GDP lagged and differenced once in columns (3) and (4), and log aid lagged and differenced once in column (3). In columns (2) and (4), log predicted aid is used as an instrument. All regressions include year effects. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6. The coefficient on aid is now smaller, and insignificant, but maybe more importantly, the Hansen tests now strongly reject the overidentification restrictions and the validity of the instruments for the level equation.[22] This suggests that the p-value of these tests in Table (5) were inflated by the number of instruments, and that system GMM estimators are based on questionable assumptions.

There is one more concern: unlike IV regressions in Table 3, no test of instrument strength is available in a GMM setting. Bazzi and Clemens (2009) argue that weak instruments are a major concern with these estimators, and suggest a replication of the GMM instrumentation in a traditional IV setting, where such tests exist.[23] We follow their advice and re-create the matrix of GMM instruments for the difference and system equations, reporting the estimations in columns (2) and (3). We can now report the Kleibergen-Paap statistics about instrument strength and the Kleibergen-Paap LM test of underidentification. Column (2) shows that the Wald statistic is very low for the difference equation, and that even the underidentification null hypothesis cannot be rejected. Since we know from Table 3 that predicted aid is not a weak instrument for aid, these signs of weak instrumentation must be due to the GMM instruments. This implies, among other things, that lagged GDP levels are very weak instruments for GDP differences. This is not very surprising, given that the difference GMM estimators performed poorly. Such weakness usually justifies the use of system GMM, as it is believed to be more robust. The fact that difference GMM performed so poorly implies that the identification relies heavily on the levels equation. Instrument strength in this equation is therefore crucial for the whole system GMM. But column (3) actually reveals that the instrumentation of this equation is even worse than for the difference equation.

[22] Collapsing the instrument matrix when using the GMM instruments for aid also leads to the rejection of these null hypotheses.

[23] Blundell and Bond (2000), Bun and Windmeijer (2010), Hayakawa (2007) and Roodman (2009a) make the same recommendation.

Table 6: Instrument collapsing and weak instruments

	(1) Collapse	(2) Difference	(3) System
Log GDP, lagged	-0.044	-1.00***	1.35
	(0.062)	(0.21)	(8.47)
Log aid, lagged	0.023	0.054	-0.85
	(0.043)	(0.049)	(6.08)
Log population	0.035	0.33	-0.71
	(0.063)	(0.26)	(4.23)
Inflation	-0.088**	-0.075***	-0.93
	(0.040)	(0.019)	(5.35)
Money, lagged	0.0018**	0.0043***	-0.000099
	(0.00090)	(0.0011)	(0.012)
Schooling	0.013	0.067	-1.38
	(0.059)	(0.085)	(8.99)
Institutional quality	0.012	0.018***	-0.12
	(0.010)	(0.0054)	(0.84)
Openness	0.10***	0.049*	0.13
	(0.024)	(0.027)	(0.35)
Instruments	22		
Countries	61	58	61
Hansen J test (p-val)	0.013		
Hansen test (p-val), level	0.0044		
AR(1)	0.0014		
AR(2)	0.79		
KP LM test (p-val)		0.24	0.88
KP F stat		2.05	0.011
Observations	347	286	340

Note: KP: Kleibergen-Paap. In column (1) system GMM is used, like in Table 5, column (4), but the matrix of GMM-type instruments is collapsed. Column (2) and (3) are 2SLS regressions where variables are instrumented using GMM-type instruments. In column (2) all variables are differenced once, and instrumented using log predicted aid and lagged log GDP levels. In column (3), instruments are log predicted aid and differenced log GDP. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

All in all, from Table 6, we conclude that our system GMM estimates suffer from two severe drawbacks. First, exogeneity tests are rejected when the set of instruments is shrunk. Second, GMM instruments appear to be extremely weak. These points lead us to infer that the GMM approach may not improve the fixed effects specification. Instruments with this level of weakness imply that conclusions about estimated coefficients are fragile. Given these results, Table 3 column (4) remains our preferred specification.

5 Robustness

5.1 Instrument exogeneity

Exogeneity of our unique aid instrument cannot be statistically tested in the absence of other valid instruments. Exogeneity will be violated if predicted aid affects GDP growth through other channels than aid. This will happen if aid partnerships are correlated with other variables that influence GDP. Our concern is that our instrument captures a larger effect, from many causes, with aid only being one of its components. If we do not control for the other components, we will wrongly attribute the causal effect in its entirety to aid. For instance, countries engaged in a long-term aid partnership may also exchange valuable information about innovation or technological progress that have nothing to do with aid, but that reflect the specific nature of the relationship between the two countries. It is very difficult to directly control for these exchanges but other channels may capture these effects. An important variable very likely to be influenced by partnership characteristics is trade. We would expect that two countries engaged in a very strong aid partnership would also engage in other economic exchanges, and that trade would be a prominent one. If our instrument is just a correlate of trade, then it is likely that the effect we are measuring comes from trade, but not from aid.

To control for this possibility, we include trade, defined as the sum of

exports toward and imports from donor countries, in the previous specifications. We construct a trade instrument using the same strategy as for the aid instrument. Using aid entry dates, we compute a predicted trade quantity for each bilateral trade partnership and obtain a predicted trade quantity by summing these up.[24]

Table 7 shows that controlling for trade only marginally changes the results. The effect of aid in the 2SLS fixed-effect regression has a similar size and is significant. Our trade instrument also appears to be strong, as is confirmed by the Angrist-Pischke p-value of the first-stage regression for trade, and the relatively high Kleibergen-Paap F statistic. The first two stages are presented in columns (2) and (3) of Table 4. We also included our trade variable in the GMM estimations. As in Table 6, these results cast serious doubts on the validity of the GMM approach in this setting, but we gain no new insight from this exercise.[25]

A stronger concern would be that some unobserved trait of the recipient country that promotes growth also has a direct effect on the starting date and/or the duration of the donor-recipient relationship, i.e. the building blocks of our instrument. For example, if donors are reluctant or unable to establish a partnership in places with despotic rulers or in places with persistent conflicts, this could at the same time delay the entry of those countries into donors' portfolios and limit growth. This would result in a negative correlation between entry date and growth, biasing upward the coefficient on aid in the main regression. In the first column of Table 8 we see that, indeed, countries with a later entry date did experience a lower growth rate. This simple correlation disappears, though, after controlling for the initial level of GDP and population size, arguably strong determinants of subse-

[24]This is done because otherwise the simultaneity between trade and growth would once more bias the estimations. A reason for using the aid partnership entry dates to instrument trade flows is that we are especially interested in capturing the part of those flows that correlates with our aid instrument.

[25]Estimation tables are available in an appendix.

Table 7: OLS and IV regressions, with trade flows

	(1) OLS	(2) FE OLS	(3) 2SLS	(4) FE 2SLS
Log GDP, lagged	0.020	-0.20***	0.032	-0.33***
	(0.023)	(0.061)	(0.090)	(0.084)
Log aid, lagged	0.015	0.016	0.014	0.088***
	(0.011)	(0.017)	(0.021)	(0.033)
Log trade, lagged	-0.050**	-0.030	-0.065	0.092
	(0.024)	(0.040)	(0.10)	(0.059)
Log population	0.022	-0.17	0.022	-0.20*
	(0.022)	(0.12)	(0.023)	(0.10)
Inflation	-0.11***	-0.11***	-0.11***	-0.10***
	(0.023)	(0.028)	(0.025)	(0.028)
Money, lagged	0.0014**	0.0027**	0.0016	0.0021*
	(0.00069)	(0.0012)	(0.0016)	(0.0013)
Schooling	0.0030	-0.0047	0.0037	0.023
	(0.0100)	(0.036)	(0.012)	(0.037)
Institutional quality	0.013**	0.0077	0.013**	0.0065
	(0.0053)	(0.0077)	(0.0054)	(0.0065)
Openness	0.093***	0.075***	0.092***	0.069**
	(0.016)	(0.027)	(0.021)	(0.029)
Ethno. fractionalization	-0.046		-0.034	
	(0.049)		(0.099)	
East Asia	0.039		0.045	
	(0.031)		(0.055)	
Sub-Saharan Africa	-0.0090		-0.0055	
	(0.037)		(0.039)	
Observations	346	346	346	343
Countries	61	61	61	58
AP test (p-val), aid			0.000000061	0.00010
AP test (p-val), trade			0.030	0.0000036
KP F stat			2.49	12.3
R^2	0.33	0.39	0.33	0.27

Note: AP: Angrist-Pischke. KP: Kleibergen-Paap. All regressions include year effects. Robust standard errors clustered at the recipient level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

quent growth rates. In column (3) we also control for total aid received. The idea would be to check if, although the time of entry in a development cooperation partnership has an effect on growth, this effect goes through aid and only through aid. The direct inclusion of aid quantity in this regression is problematic, given the endogeneity of aid to growth, so we do not put too much weight on this last model.

In the regressions reported in columns (1) to (3), the observations are at the partnership level: this implies that each recipient country has many entry dates (one for each donor) and only one growth rate for each time period. In columns (4) to (6), we collapse the observations at the recipient country level, using the aid quantities as weight for donor countries: therefore, each recipient will only have one average entry date, which will be earlier if the most important donors in terms of aid given started their partnership with this country earlier, and vice versa. Even the simple correlation disappears in this setting. There results show that entrants with different entry dates do not on average differ from a GDP growth point of view and thus, is further suggestive evidence that our instrument is indeed exogenous.

Table 8: Correlation between entry date and growth

	Whole Sample			Collapsed		
	(1)	(2)	(3)	(4)	(5)	(6)
Entry	-0.00050**	-0.00025	-0.00018	-0.0024	-0.0030	-0.0028
	(0.00020)	(0.00022)	(0.00022)	(0.0023)	(0.0039)	(0.0040)
Log GDP, lagged		-0.0031**	0.0031*		0.00085	0.0034
		(0.0015)	(0.0018)		(0.0072)	(0.0085)
Log population		0.0054***	-0.011***		-0.0024	-0.0091
		(0.0013)	(0.0024)		(0.0086)	(0.012)
Log aid			0.021***			0.0092
			(0.0022)			(0.013)
Observations	20974	20974	20974	812	812	812
Countries	112	112	112	112	112	112
R^2	0.041	0.042	0.048	0.052	0.052	0.053

Note: The dependent variable is the growth rate over five years. All regressions include year effects. Columns (1)-(3) include one observation for each recipient-donor pair every five years; observations in columns (4)-(6) are the weighted average for each recipient and five-year period, where each donor is weighted with the total aid quantity donated to that specific recipient during the five-year period. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Outliers

Easterly et al. (2004) showed how aid effectiveness results could be sensitive to the exclusion of a few outliers. We make use of the Hadi (1992) procedure to exclude outliers from the sample. Both with the within groups estimator and in the system GMM regressions, we find larger effects of aid than when all observations are used. The elasticity of GDP with respect to aid increases by 60 percent, with and without controlling for trade.

Figure 2 shows the two partial regression plots of GDP growth on instrumented aid, with and without outliers. We conclude that outliers tended to bias our estimates downward and hence, the GDP elasticity with respect to aid is possibly much larger.[26]

5.3 Sample size

Our previous specifications include control variables that are commonly found in the aid effectiveness literature. However, limited data availability sharply reduces the sample size. Our dataset contains 130 countries but the regressions rely on 61 countries at most. Larger sample size comes at the cost of omitting some growth determinants and hence, potentially biases the aid coefficient. On the other hand, the aid instrument, if truly exogenous, should remove the correlation between aid and the error term even in the presence of omitted variables. This provides an indirect test of instrument validity, in addition to extending the estimation to many more countries. The most parsimonious specification with only lagged GDP, aid, and population as controls, allows us to use data on 108 countries, a dramatic increase. Aid is not significant in any of the regressions.

Because we include as few controls as possible in these regressions, there may be strong outliers in these specifications. We put this result to the test of excluding outliers, once more following the Hadi procedure. Table 9 confirms

[26]Estimation tables are available in an appendix.

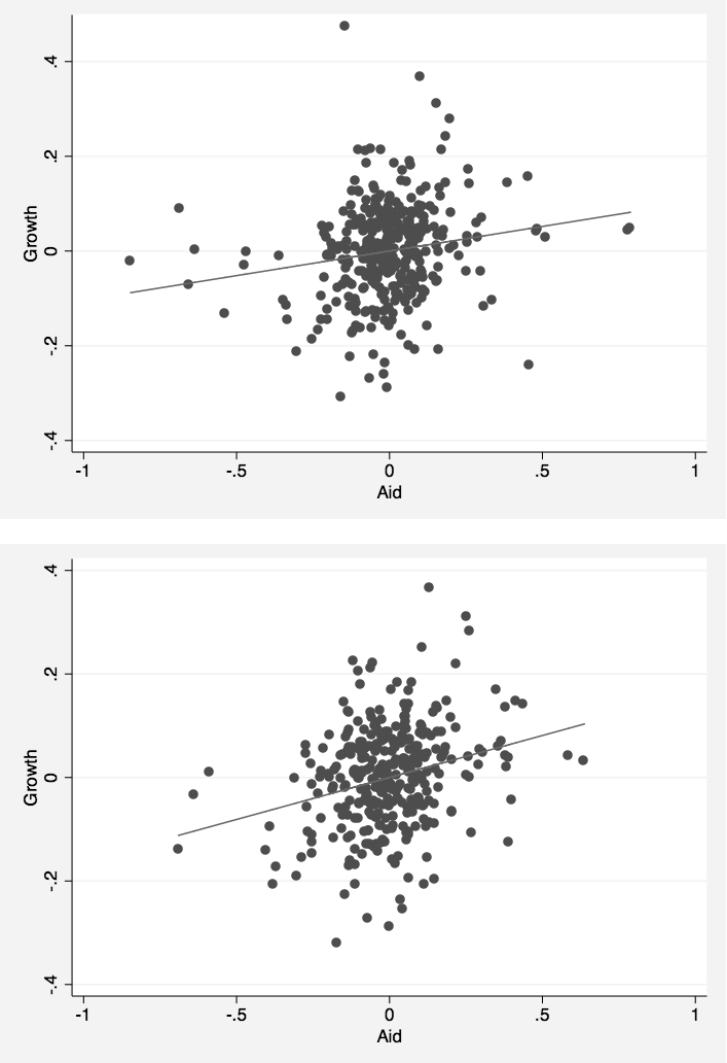


Figure 2: Partial regression plot of growth on aid, including and excluding outliers

that these data points strongly influence the results, despite representing a very small group of observations (the procedure excludes 7 observations). This is visually confirmed by the partial regression plots of growth on aid shown in the Appendix. First, aid becomes significant, even when it is not instrumented. This result is not robust to the inclusion of additional controls, as shown earlier, and thus has little meaning in itself. More interesting is the within-groups estimate when aid is instrumented, in column (4). The coefficient is significant, and its size almost the same as with the controls (see Table 7 column (4)). This is further encouraging evidence of our instrument being valid.[27]

Table 9: OLS and IV regressions excluding outliers, large sample

	(1) OLS	(2) FE	(3) 2SLS	(4) FE-2SLS
Log GDP, lagged	0.0046	-0.21***	-0.0057	-0.21***
	(0.0063)	(0.039)	(0.010)	(0.038)
Log aid, lagged	0.022***	0.023*	-0.022	0.085**
	(0.0073)	(0.013)	(0.033)	(0.037)
Log population	-0.011	0.022	0.021	-0.079
	(0.0097)	(0.089)	(0.026)	(0.11)
Countries	108	108	108	104
AP test (p-val)			0.00039	0.0000061
KP F stat			13.4	22.7
R^2	0.073	0.23	0.025	0.18
Observations	710	703	710	696

Note: AP: Angrist-Pischke. KP: Kleibergen-Paap. All regressions include year effects. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Finally, we use GMM estimators on the same large sample. The difference GMM estimator once more fails to produce correct estimates of the lagged GDP coefficient; while using system GMM, the aid coefficient is very close

[27]In fact, the coefficient on aid is virtually unchanged even without controlling for population and initial GDP. Tables can be obtained from the authors upon request.

to the one in Table 5. This once more tends to confirm that our results are robust. As before, though, GMM estimates appear to be quite fragile. On the other hand, we find it encouraging that all our system GMM specifications find an elasticity close to 0.05.

These robustness tests confirm our earlier results that aid has a significant and positive impact on growth. The elasticity of GDP with respect to aid is found to lie between 0.05 and 0.16, depending on the estimators used and the exclusion of outliers from the regression sample.

5.4 Aid as a share of GDP

We depart from the aid effectiveness literature by measuring aid in constant dollars, while past research traditionally used aid as a share of GDP.[28] This departure was done to avoid introducing additional endogeneity in the aid variable. It is indeed peculiar to strive to remove reverse causality from GDP to aid by using instrumental variables and then re-introducing GDP as a denominator. We prefer to instead use aid quantities. This offers other advantages: first, the log-log specification directly estimates the elasticity of GDP with respect to aid; moreover, since lagged log GDP enters equation (1), the particular case with aid as a share of GDP can be seen as a special case of equation (1), albeit in its log form, while our aid-quantity specification would be the more general case.

Nevertheless, and despite the fact that instrumentation is likely to be more problematic, we feel that we cannot completely ignore the past convention and, in Table 10, we present results where the aid variable is expressed in GDP percentage points. The trade variable is also computed as a share of GDP, while other controls are the same as in previous tables.

Columns (1) and (2) are based on the whole sample, and columns (3) and

[28] Another departure is the definition of the growth variable that can be measured between the beginning and the end of the time period, or as an average of yearly growth rates. We return to this point in Appendix A.2, as the results are not affected by this change of definition.

Table 10: OLS and IV regressions, aid as a share of GDP

	(1) FE	(2) FE 2SLS	(3) FE	(4) FE 2SLS
Log GDP, lagged	-0.23***	-0.28***	-0.23***	-0.15**
	(0.046)	(0.053)	(0.046)	(0.065)
Aid, share of GDP	0.15	-1.19	0.21	2.15**
	(0.25)	(0.80)	(0.31)	(1.02)
Log population	-0.15	-0.099	-0.16	-0.26***
	(0.12)	(0.15)	(0.12)	(0.081)
Inflation	-0.11***	-0.12***	-0.12***	-0.11***
	(0.027)	(0.024)	(0.028)	(0.030)
Money, lagged	0.0028**	0.0023	0.0024*	0.0026**
	(0.0013)	(0.0015)	(0.0012)	(0.0010)
Schooling	-0.0012	-0.061	0.0038	0.058
	(0.034)	(0.044)	(0.032)	(0.044)
Institutional quality	0.0085	0.0068	0.0066	0.0026
	(0.0072)	(0.0059)	(0.0061)	(0.0053)
Openness	0.075***	0.075***	0.064***	0.060**
	(0.027)	(0.026)	(0.023)	(0.024)
Countries	61	58	60	57
AP test (p-val)		0.028		0.0011
KP F stat		5.08		11.8
R^2	0.38	0.28	0.39	0.25
Observations	347	344	340	331

Note: AP: Angrist-Pischke. KP: Kleibergen-Paap. All regressions include year and country fixed effects. Outliers, identified through the Hadi procedure, are excluded from the sample in columns (3) and (4). Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(4) exclude outliers identified by the Hadi procedure. The first two columns show that aid has no effect on GDP, but the next two reveal that this is due to very few outliers (only 13 observations are excluded from column (2) to column (4)). As in Table 3, the aid coefficient differs significantly from zero only when instrumented. Removing outliers does not only increase the aid coefficient, it also improves the identification, as shown by the Angrist-Pischke test and the Kleibergen-Paap statistic.[29]

In Table 11, system GMM estimators are used to remove the bias induced by the dynamic nature of the specification.[30] Column (1) presents results based on the full sample, and column (2) excludes the outliers. Columns (3) and (4) control for trade. In both specifications, aid turns out to be significant once outliers are excluded, with p-values of 6.1 and 7.8 percent in columns (2) and (4), respectively. The size of the coefficient is smaller than with the within groups estimator. Although the Hansen tests do not reject the GMM approach, we remain wary of these estimations where instrumentation is very weak.

Finally, in Table 12, we check that using aid as a share of GDP does not solve the issue previously encountered in Table 6. We replicate those specifications using the new aid variable. Column (1) runs the system GMM estimation collapsing the instrument matrix, and fails to reject the validity of the system GMM assumption. On the other hand, columns (2) and (3) show that the GMM instruments, both for the difference and system equations, are very weak.[31]

Our conclusions are therefore mostly robust to the change in aid measurement. When properly instrumented for, aid has a positive and signifi-

[29]We do not present results using OLS and 2SLS estimators, however aid coefficients are not significantly different from zero in any of them, with and without outliers.

[30]We focus on system GMM rather than difference GMM for the same reason as in Section 4. The lagged log GDP coefficients with difference GMM are well below their FE estimates, such that the difference GMM estimator must be severely biased and thus is not reliable. Tables are available from the authors upon request.

[31]This is also the case when outliers are excluded.

Table 11: GMM regressions, aid and trade as shares of GDP

	(1) Sys. GMM	(2) Sys. GMM	(3) Sys. GMM	(4) Sys. GMM
Log GDP, lagged	0.037	0.028	0.041	0.038
	(0.041)	(0.037)	(0.034)	(0.039)
Aid, share of GDP	0.94	1.18*	0.89	1.26*
	(0.61)	(0.63)	(0.56)	(0.71)
Trade, share of GDP			-0.045	-0.060
			(0.16)	(0.21)
Log population	-0.0082	0.0040	-0.017	-0.0056
	(0.034)	(0.026)	(0.029)	(0.034)
Inflation	-0.11***	-0.10***	-0.11***	-0.11***
	(0.022)	(0.021)	(0.022)	(0.021)
Money, lagged	0.0015**	0.0012**	0.0015**	0.0011
	(0.00061)	(0.00063)	(0.00076)	(0.00080)
Schooling	-0.013	0.00095	-0.018	0.0038
	(0.029)	(0.021)	(0.028)	(0.023)
Institutional quality	0.0082	0.0088	0.0076	0.0069
	(0.0055)	(0.0054)	(0.0050)	(0.0061)
Openness	0.095***	0.097***	0.091***	0.089***
	(0.022)	(0.024)	(0.024)	(0.028)
Instruments	48	48	49	49
Countries	61	61	61	60
Hansen J test (p-val)	0.62	0.83	0.68	0.83
Hansen test (p-val), level	0.57	0.61	0.65	0.51
AR(1)	0.00095	0.00015	0.00082	0.00012
AR(2)	0.93	0.81	0.94	0.76
Observations	347	335	347	334

Note: Instruments for the differences equation are log GDP lagged twice. Instruments for the levels equation are log GDP lagged and differenced once. Log predicted aid and trade are used as instruments in all regressions. Columns (2) and (4) exclude outliers. All regressions include year effects. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Instrument collapsing and weak instruments, aid as share of GDP

	(1) GMM Collapse	(2) 2SLS-Difference	(3) 2SLS-System
Log GDP, lagged	0.20	-1.19***	0.27
	(0.20)	(0.20)	(0.22)
Aid, share of GDP	2.68	-0.57	4.04
	(2.52)	(1.06)	(3.44)
Log population	-0.12	0.44	-0.15
	(0.14)	(0.30)	(0.15)
Inflation	-0.14***	-0.066***	-0.23*
	(0.051)	(0.020)	(0.13)
Money, lagged	-0.00016	0.0039**	-0.0016
	(0.0019)	(0.0015)	(0.0025)
Schooling	-0.023	0.066	0.0065
	(0.048)	(0.10)	(0.065)
Institutional quality	-0.0037	0.019***	-0.0066
	(0.012)	(0.0051)	(0.016)
Openness	0.074*	0.045	0.020
	(0.041)	(0.035)	(0.072)
Instruments	22		
Countries	61	58	61
Hansen J test (p-val)	0.65		
Hansen test (p-val), level	0.41		
AR(1)	0.0032		
AR(2)	0.86		
KP LM test (p-val)		0.54	0.15
KP F stat		1.99	1.20
Observations	347	286	340

Note: KP: Kleibergen-Paap. Column (1) presents GMM estimations, (2) and (3) are 2SLS regressions where variables are instrumented using GMM-type instruments and log predicted aid. Instruments for the differences equation are log GDP lagged twice. Instruments for the levels equation are log GDP lagged and differenced once. Log predicted aid is used as an instrument in all the regressions. All the regressions include year effects. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

cant effect on GDP. Our estimates with this new variable range from 1.18 to 2.15. These can be related to our former estimates. If γ_1 and γ_2 are the aid coefficients using log aid and aid as a share of GDP, then computing marginal effects, we should have $\gamma_1 = \frac{A_{t-1}}{Y_{t-1}}\gamma_2$. The mean of aid per GDP in the regression sample is 0.052, such that the corresponding γ_1 lies between 0.062 and 0.11. The actual estimates are between 0.057 and 0.16, so the two specifications lead to similar results.

5.5 Comparison with the existing literature

These estimates can be related to the existing literature. As mentioned above, most papers estimate the effect of aid, expressed as a percentage of GDP, on growth. Except Boone (1996), who does not look at the impact of aid on GDP growth, the results in Dalgaard et al. (2004), Hansen and Tarp (2001) and Burnside and Dollar (2000) are comparable in size, ranging from 0.09 respectively to 0.1 and 0.18, although this last result, from Burnside and Dollar (2000), only refers to the subsample of countries with good policies, according to their definition. A recent meta-analysis review, Doucouliagos and Paldam (2009), claims that "the best estimate we can make of the elasticity of the real product to aid is about 0.13". This elasticity is estimated using aid in percentage points of GDP, whereas we use the aid share, defined as aid divided by GDP. Our estimates from Section 5.4 must therefore be divided by 100 to be comparable to those found in the literature. They range from 0.0118 to 0.0215, using aid in GDP percentage points. Using the log-log specification of Section 4, and converting them to be comparable, yields estimates from 0.011 to 0.031. Both specifications generate elasticities that are four to ten times smaller than previous studies. The identification strategy adopted in this paper, based on an exogenous and strong instrument, uncovers significant effects, but much smaller than previously thought. To the extent that these results are more reliable than previous ones, the small size of the effect might help explain why previous estimates range so widely

in sign and size.

6 Conclusions

In this article, we proposed a new instrument for identifying the causal effect of aid on growth. This instrument takes the supply side approach that relates to the aid allocation decision a step further, for the first time using a source of variation that is not just external but exogenous to growth. As far as possible, the instrument is shown to be valid and strong. We claim that this is an improvement from a stream of papers that relied on weak and non-exogenous instruments.

When it comes to the estimation strategy and the choice of estimator, we make simple and clear methodological choices, explain and motivate them step by step and probe their validity as best as we can. In particular, we do not take it for granted that GMM estimators provide strong instruments and thus solve any dynamic bias. On the contrary, we show that they should be used with much caution as the cure may be worse than the disease. Instrument weakness is so prominent that estimates are at best fragile, at worst misleading.

The effects uncovered by our identification strategy are relatively small, yet statistically significant and robust to various specifications. They indicate an elasticity of GDP with respect to aid that lies around 0.10, about ten times smaller than previously estimated.

Bibliography

Acemoglu, D., S. Johnson, and J. A. Robinson (2001). The colonial origins of comparative development: An empirical investigation. *The American Economic Review 91*(5), 1369–1401.

Angrist, J. D. and J.-S. Pischke (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton: Princeton University Press.

Arellano, M. and S. Bond (1991). Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations. *The Review of Economic Studies 58*(2), 277–297.

Barro, R. J. and J.-W. Lee (2010, April). A new data set of educational attainment in the world, 1950-2010. Working Paper 15902, NBER.

Bazzi, S. and M. Clemens (2009, May). Blunt instruments: On establishing the causes of economic growth. Working Paper 171, Center for Global Development.

Bertocchi, G. and F. Canova (2002, December). Did colonization matter for growth? an empirical exploration into the historical causes of africa's underdevelopment. *European Economic Review 46*(10), 1851–1871.

Blundell, R. and S. Bond (1998). Initial conditions and moment restrictions in dynamic panel data models. *Journal of Econometrics 87*(1), 115–143.

Blundell, R. and S. Bond (2000). GMM estimation with persistent panel data: An application to production functions. *Econometric Reviews 19*(3), 321–340.

Bond, S., A. Hoeffler, and J. Temple (2001, September). GMM estimation of empirical growth models. Discussion Papers 01/525, Nuffield College.

Boone, P. (1996). Politics and the effectiveness of foreign aid. *European Economic Review 40*(2), 289 – 329.

Bun, M. and F. Windmeijer (2010). The weak instrument problem of the system GMM estimator in dynamic panel data models. *The Econometrics Journal 13*(1), 95–126.

Burnside, C. and D. Dollar (2000). Aid, policies, and growth. *The American Economic Review 90*(4), 847–868.

Clemens, M., S. Radelet, and R. Bhavnani (2004, November). Counting chickens when they hatch: the short term effect of aid on growth. Working Paper 44, Center for Global Development.

Dalgaard, C.-J., H. Hansen, and F. Tarp (2004). On the empirics of foreign aid and growth. *Economic Journal 114*(496), p191 – 216.

Deaton, A. (2010). Instruments, randomization, and learning about development. *Journal of Economic Literature 48*, 424–455.

Doucouliagos, H. and M. Paldam (2009). The aid effectiveness literature: The sad results of 40 years of research. *Journal of Economic Surveys 23*(3), 433–461.

Easterly, W. (2006, March). *The White Man's Burden: Why the West's Efforts to Aid the Rest Have Done So Much Ill and So Little Good*. New York: Penguin Press.

Easterly, W. (2007). Was development assistance a mistake? *American Economic Review 97*(2), 328 – 332.

Easterly, W., R. Levine, and D. Roodman (2004). Aid, policies, and growth: Comment. *The American Economic Review 94*(3), 774–780.

Frot, E. (2009). Early vs. late in aid partnerships and implications for tackling aid fragmentation. Working Paper No.1, SITE.

Hadi, A. S. (1992). Identifying multiple outliers in multivariate data. *Journal of the Royal Statistical Society 54*(2), 761 – 771.

Hansen, H. and F. Tarp (2001). Aid and growth regressions. *Journal of Development Economics 64*(2), 547 – 570.

Hayakawa, K. (2007). Small sample bias properties of the system GMM estimator in dynamic panel data models. *Economics Letters 95*(1), 32–38.

Kleibergen, F. and R. Paap (2006, May). Generalized reduced rank tests using the singular value decomposition. *Journal of Econometrics 133*(1), 97–126.

Manski, C. F. and E. Tamer (2002). Inference on regressions with interval data on a regressor or outcome. *Econometrica 70*(2), 519–546.

Nickell, S. (1981). Biases in dynamic models with fixed effects. *Econometrica 49*, 1417–1426.

Rajan, R. G. and A. Subramanian (2008, October). Aid and growth: What does the cross-country evidence really show? *Review of Economics and Statistics 90*(4), 643–665.

Rigobon, R. and T. Stoker (2009). Bias from censored regressors. *Journal of Business and Economic Statistics 27*(3), 340–353.

Roeder, P. G. (2001). Ethnolinguistic fractionalization (ELF) indices, 1961 and 1985. *Department of Political Science, University of California at San Diego*.

Roodman, D. (2007). The anarchy of numbers: aid, development, and cross-country empirics. *The World Bank Economic Review 21*(2), 255.

Roodman, D. (2009a). How to do xtabond2: An introduction to difference and system GMM in Stata. *Stata Journal 9*(1), 86–136.

Roodman, D. (2009b). A note on the theme of too many instruments. *Oxford Bulletin of Economics and Statistics 71*(1), 135–158.

Sachs, J. D., A. Warner, A. Åslund, and S. Fischer (1995). Economic Reform and the Process of Global Integration. *Brookings papers on economic activity 1*(1995), 1–95.

Staiger, D. and J. H. Stock (1997). Instrumental variables regression with weak instruments. *Econometrica 65*(3), 557–586.

Stock, J. H. and J. H. Wright (2000). GMM with weak identification. *Econometrica 68*(5), 331–386.

Stock, J. H. and M. Yogo (2005). Testing for weak instruments in linear iv regressions. In J. Stock and D. Andrews (Eds.), *Identification and Inference for Econometric Models: Essays in Honor of Thomas J. Rothenberg*. New York: Cambridge University Press.

Wacziarg, R. and K. H. Welch (2008). Trade Liberalization and Growth: New Evidence. *The World Bank Economic Review 22*(2), 187.

A Appendix

A.1 Data appendix

Time periods. Observations for all variables except GDP, aid and trade are five-year arithmetic averages. Time period 1 represents years 1961-1965. The last period (period 10) is 2001-2005.

Log aid. Aid is Official Development Assistance (ODA) and comes from the Donor Assistance Committee (DAC) database of the OECD, Table 2a. Because predicted aid is built from predicted aid shares, net ODA, which is the usual aid variable in the aid effectiveness literature and which is potentially negative, cannot be used. Aid is defined as gross ODA, minus gross debt relief. The latter is excluded because it artificially inflates aid numbers in very recent years, where large debt cancellations were granted. Aid is not averaged, but summed up over the time period. It is expressed in millions of 2006 USD. Aid from all donors whose activity is reported by DAC and to all developing countries, according to DAC definition, is considered.

Log trade. Trade at the bilateral level is defined as the sum of imports and exports. At the recipient country level, it is summed across donor countries. Data in current USD millions from the International Trade dataset, version 2.01, of the Correlates of War Project. It is converted in 2006 USD by deflating it with the Consumer Price Index of the US Bureau of Labor Statistics.

Aid and trade as shares of GDP. Data in current USD is divided by GDP in current USD.

Log GDP. GDP in 2000 USD is from the World Development Indicators. GDP is not averaged, but measured every fifth year (1965, 1970, 1975, etc.). We use this instead of averaging to avoid introducing serial correlation.

GDP per capita. In 2000 USD. Source: World Development Indicators.

Growth. Growth is defined as the difference $ln(y_{\tilde{t}}) - ln(y_{\tilde{t}-5})$, where $y_{\tilde{t}}$ is GDP in year $\tilde{t}$. Note here that $\tilde{t}$ indexes year and not time periods.

Log population. Population is measured in millions. Source: World Development Indicators.

Inflation. Natural logarithm of 1+consumer price inflation rate. Source: World Development Indicators.

Money. Ratio of M2 to GDP. Source: World Development Indicators.

Schooling. Average years of primary schooling attained. Source: Barro and Lee (2010).

Institutional Quality. Variable between 0 and 16, defined as the sum of "Corruption", "Law and Order", and "Bureaucracy Quality", from the International Country Risk Guide (ICRG) of the PRS Group. Data is not available before 1984. For earlier years, data from the first available year is used. By doing so we follow the practice in the literature (see Roodman (2007)).

Openness. Index constructed by Sachs et al. (1995) and Wacziarg and Welch (2008).

Ethnic fractionalization. Ethnolinguistic Fractionalization index. Source:

Roeder (2001).

Regional dummies. Dummies for East Asia and Pacific, and Sub-Saharan Africa. Region definitions are from the World Development Indicators.

Colony. Dummy variable equal to 1 if the pair has ever had a colonial link. Source: CEPII.

Distance. Distance in thousands of kilometers between the two main cities of the country. Source: CEPII.

Table A.1: Summary statistics and source of variables

Variable	Mean	s.d.	Unit
Growth	.18	.20	Percentage
Aid	1.86	3.28	Constant 2006 USD bn
Aid, GDP share	.087	.12	Percentage
GDP	27.1	76.4	Constant 2000 USD bn
GDP per capita	1711	2043	Constant 2000 USD
Population	20.5	74.9	Millions
Inflation	.15	.28	Annual change, perc. points
Openness	.22	.39	0-1 index
Money	32.5	27.5	M2 as perc. of GDP
Trade	41.9	113.8	Constant 2006 USD bn
Schooling	2.98	1.65	Year
Institutional quality	6.55	.2.61	1-16 continuous variable
East Asia	.12	.33	Identifier
Sub-Saharan Africa	.35	.48	Identifier
Ethno-linguistic frac.	.53	.27	Index (0 to 1)
Aid share	1.18	3.86	Percentage
Entry	8.84	9.07	year
Length	17.6	11.7	year
Colony	.038	.19	Index (0 to 1)
Distance	8.41	3.84	Thousands of km

A.2 Definition of growth

As indicated in Appendix A.1, growth is defined over five-year periods. The aid effectiveness literature traditionally measures growth as the average yearly growth rate during the time period, i.e. as $\frac{1}{5}\sum_{i=0}^{4}\frac{y_{\tilde{t}+i+1}-y_{\tilde{t}+i}}{y_{\tilde{t}+i}}$. The two growth rates are highly correlated so we do not expect this change to affect the results.[32] On the other hand, we want to ensure that our results are not driven by this modification, and for greater comparability with the existing literature, we here replicate some of our results with growth defined as the five-year average of yearly rates.

Panel A uses aid volumes, panel B aid as a share of GDP. To compare results with the five-year growth rate and with the average yearly growth rate, one should, using a first order approximation, multiply these by five. Column (1) of Table A.2 is the within groups estimator with aid instrumented. The coefficient on aid is still significant, and its size multiplied by five is equivalent to the same coefficient in Table 3, column (4). Column (2) presents results with the system GMM estimator, and once more they correspond to what we found with the five-year growth rate. The next two columns exclude outliers. In A.3, which reports the same estimations but with aid as a share of GDP, the aid coefficient is significant only after outliers are excluded from the sample, similarly to the results in Section 5.4. Tables A.2 and A.3 confirm that our findings are in no way driven by our alternative definition of growth.

A.3 Additional robustness checks

Instrument exogeneity

In Table A.4, we include our trade variable in the GMM estimations. In column (1), the GMM difference estimator is used: like in Table 5, the coefficient on lagged GDP is too low for the estimator to be valid. In column (2)

[32] The correlation is 0.99 in the data.

Table A.2: Growth as an average

	(1) FE-2SLS	(2) Sys. GMM	(3) FE-2SLS	(4) Sys. GMM
Log GDP, lagged	-0.047***	-0.013	-0.044***	-0.018*
	(0.010)	(0.011)	(0.013)	(0.0091)
Log aid, lagged	0.022***	0.011*	0.033***	0.011*
	(0.0078)	(0.0064)	(0.012)	(0.0060)
Log population	-0.054**	0.0077	-0.057**	0.014
	(0.021)	(0.0094)	(0.025)	(0.0098)
Inflation	-0.019***	-0.018*	-0.019***	-0.016**
	(0.0056)	(0.010)	(0.0064)	(0.0073)
Money, lagged	0.00058**	0.00030*	0.00049**	0.00032*
	(0.00023)	(0.00017)	(0.00025)	(0.00019)
Schooling	0.0058	0.0074	0.0092	0.012*
	(0.0075)	(0.013)	(0.0077)	(0.0069)
Institutional quality	0.00066	0.0035**	0.000031	0.0037**
	(0.0014)	(0.0016)	(0.0016)	(0.0017)
Openness	0.016***	0.023***	0.016**	0.024***
	(0.0058)	(0.0044)	(0.0068)	(0.0046)
Instruments		48		48
Countries	58	61	58	61
Hansen J test (p-val)		0.27		0.29
Hansen test (p-val), level		0.10		0.059
AR(1)		0.00086		0.000086
AR(2)		0.92		0.52
AP test (p-val)	0.00088		0.0017	
KP F stat	12.3		10.9	
R^2	0.27		0.081	
Observations	344	347	336	339

Note: KP: Kleibergen-Paap. AP: Angrist-Pischke. For the GMM estimations, instruments for the differences equation are log GDP lagged twice; instruments for the levels equation are log GDP lagged and differenced once. Log predicted aid is used as an instrument in all regressions. In columns (3) and (4), outliers are excluded using the Hadi procedure. All regressions include year effects. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Growth as an average, aid as sh. of GDP

	(1) FE-2SLS	(2) Sys. GMM	(3) FE-2SLS	(4) Sys. GMM
Log GDP, lagged	-0.057***	0.0072	-0.030**	0.0052
	(0.011)	(0.0076)	(0.013)	(0.0068)
Aid, share of GDP	-0.23	0.17	0.46**	0.23**
	(0.16)	(0.11)	(0.21)	(0.11)
Log population	-0.027	-0.0020	-0.060***	0.0010
	(0.030)	(0.0068)	(0.016)	(0.0049)
Inflation	-0.023***	-0.022***	-0.022***	-0.020***
	(0.0048)	(0.0044)	(0.0059)	(0.0041)
Money, lagged	0.00048	0.00028**	0.00053**	0.00025*
	(0.00031)	(0.00012)	(0.00022)	(0.00013)
Schooling	-0.013	-0.0038	0.012	-0.00047
	(0.0091)	(0.0055)	(0.0093)	(0.0041)
Institutional quality	0.0012	0.0016	0.00041	0.0018
	(0.0012)	(0.0011)	(0.0011)	(0.0011)
Openness	0.015***	0.020***	0.012**	0.020***
	(0.0053)	(0.0044)	(0.0049)	(0.0049)
Instruments		48		48
Countries	58	61	57	61
Hansen J test (p-val)		0.62		0.82
Hansen test (p-val), level		0.40		0.48
AR(1)		0.00070		0.000086
AR(2)		0.84		0.58
AP test (p-val)	0.028		0.0011	
KP F stat	5.08		11.8	
R^2	0.29		0.24	
Observations	344	347	331	335

Note: KP: Kleibergen-Paap. AP: Angrist-Pischke. For the GMM estimations, instruments for the differences equation are log GDP lagged twice; instruments for the levels equation are log GDP lagged and differenced once. Log predicted aid is used as an instrument in all regressions. In columns (3) and (4), outliers are excluded using the Hadi procedure. All regressions include year effects. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the system GMM estimator is used, and the coefficient on aid is smaller than with the within groups estimator, but still significant. The Hansen tests do not reject the required conditions. On the other hand, the relatively large number of instruments is likely to decrease the power of these tests. For this reason, we collapse the instrument matrix in column (3). The Hansen tests are still valid, but the aid coefficient is no longer significant.

Table A.4: GMM regressions, with trade flows

	(1) Diff. GMM	(2) Syst. GMM	(3) GMM Collapse	(4) 2SLS-Diff.	(5) 2SLS-Sys.
Log GDP, lagged	-0.50***	0.038	0.45	-1.18***	0.35
	(0.17)	(0.041)	(0.40)	(0.31)	(0.25)
Log aid, lagged	0.016	0.041*	0.019	0.041	0.026
	(0.040)	(0.024)	(0.10)	(0.048)	(0.052)
Log trade, lagged	-0.032	-0.096**	-0.41	0.11	-0.33
	(0.081)	(0.044)	(0.35)	(0.15)	(0.26)
Log population	-0.037	0.015	-0.12	0.44	-0.079
	(0.17)	(0.022)	(0.13)	(0.29)	(0.071)
Inflation	-0.078***	-0.095***	-0.13*	-0.079***	-0.21*
	(0.029)	(0.031)	(0.068)	(0.020)	(0.11)
Money, lagged	0.0029**	0.0025**	0.0052	0.0039***	0.0037
	(0.0012)	(0.0012)	(0.0047)	(0.0013)	(0.0035)
Schooling	-0.034	0.013	-0.11	0.077	-0.074
	(0.065)	(0.022)	(0.15)	(0.090)	(0.080)
Institutional quality	0.014*	0.014**	0.00094	0.017***	0.0030
	(0.0073)	(0.0060)	(0.017)	(0.0056)	(0.011)
Openness	0.078**	0.11***	0.095**	0.051*	0.084**
	(0.033)	(0.031)	(0.046)	(0.030)	(0.042)
Instruments	41	49	23		
Countries	58	61	61	58	61
Hansen J test (p-val)	0.25	0.63	0.44		
Hansen test (p-val), level		0.77	0.29		
AR(1)	0.11	0.00070	0.0031		
AR(2)	0.26	0.46	0.35		
KP LM test (p-val)				0.15	0.33
KP F stat				2.13	0.33
Observations	285	346	346	285	339

Note: KP: Kleibergen-Paap. Columns (1), (2), and (3) present GMM estimations, (4) and (5) are 2SLS regressions where variables are instrumented using GMM-type instruments, log predicted aid, and log predicted trade. Instruments for the differences equation are log GDP lagged twice. Instruments for the levels equation are log GDP lagged and differenced once. Log predicted aid and trade are used as instruments in all regressions. All regressions include year effects. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Collapsing the instruments is useful for having more accurate Hansen

tests, but since fewer moment conditions are used, the estimator becomes less efficient. Hence, we take the following approach: we keep in mind from the collapsing exercise that the Hansen tests do not reject the system GMM assumptions, but when it comes to point estimates, we consider those in column (2) as our preferred because they are more efficient. In columns (4) and (5), we test the strength of the GMM-type instruments, similarly to what is done in Table 6. We find that they are very weak, as shown by the extremely low Kleibergen and Paap F statistic. The null of underidentification cannot be rejected with a reasonable confidence level. As in Table 6, these results cast some serious doubt on the validity of the GMM approach in this setting.

Outliers

In Table A.5, we focus on our key regressions and run them without the Hadi-identified outliers in order to check their robustness. Columns (1) and (2) use the within groups estimator and find larger effects of aid than when all observations are used. The difference is sizable. The elasticity of GDP with respect to aid increases by 60 percent in both specifications, with and without controlling for trade. System GMM regressions in columns (4) and (5) yield similar results, but the aid coefficient becomes significant when controlling for trade. This is encouraging but we are reluctant to draw any firm conclusions from regressions based on very weak instrumentations. We take away from Table A.5 that outliers tended to bias our estimates downward, so that the GDP elasticity with respect to aid is possibly much larger.

Sample size

Table A.7 presents results from the most parsimonious specification with only lagged GDP, aid, and population as controls.

Column (1) of Table A.7 shows that the difference GMM estimator once more fails to produce correct estimates of the lagged GDP coefficient. Column (2) applies the system GMM estimator, and the aid coefficient is very

Table A.5: Excluding outliers

	(1) FE-2SLS	(2) FE-2SLS	(3) System GMM	(4) System GMM
Log GDP, lagged	-0.21***	-0.33***	-0.099**	0.018
	(0.063)	(0.12)	(0.049)	(0.048)
Log aid, lagged	0.16***	0.14***	0.050	0.049**
	(0.058)	(0.045)	(0.031)	(0.020)
Log trade, lagged		0.10		-0.099*
		(0.082)		(0.052)
Log population	-0.25**	-0.21*	0.085	0.042
	(0.12)	(0.12)	(0.053)	(0.036)
Inflation	-0.096***	-0.10***	-0.079**	-0.094***
	(0.033)	(0.032)	(0.035)	(0.035)
Money, lagged	0.0025**	0.0018	0.0017	0.0025**
	(0.0012)	(0.0013)	(0.0010)	(0.0012)
Schooling	0.049	0.038	0.065*	0.035
	(0.038)	(0.038)	(0.033)	(0.028)
Institutional quality	0.00087	0.0030	0.020**	0.019**
	(0.0082)	(0.0073)	(0.0094)	(0.0076)
Openness	0.080**	0.069**	0.12***	0.12***
	(0.034)	(0.035)	(0.024)	(0.026)
Instruments			48	49
Countries	58	58	61	61
Hansen			0.29	0.37
Hansen level			0.029	0.19
AR(1)			0.000094	0.000090
AR(2)			0.72	0.97
KP F stat	10.9	11.2		
Observations	336	336	339	339

Note: KP: Kleibergen-Paap. Instruments for the differences equation are log GDP lagged twice. Instruments for the levels equation are log GDP lagged and differenced once. Log predicted aid and trade are used as instruments in all regressions. All regressions include year effects. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: OLS and IV regressions, large sample

	(1) FE-2SLS	(2) FE-2SLS	(3) System GMM	(4) System GMM
Log GDP, lagged	-0.21***	-0.33***	-0.099**	0.018
	(0.063)	(0.12)	(0.049)	(0.048)
Log aid, lagged	0.16***	0.14***	0.050	0.049**
	(0.058)	(0.045)	(0.031)	(0.020)
Log trade, lagged		0.10		-0.099*
		(0.082)		(0.052)
Log population	-0.25**	-0.21*	0.085	0.042
	(0.12)	(0.12)	(0.053)	(0.036)
Inflation	-0.096***	-0.10***	-0.079**	-0.094***
	(0.033)	(0.032)	(0.035)	(0.035)
Money, lagged	0.0025**	0.0018	0.0017	0.0025**
	(0.0012)	(0.0013)	(0.0010)	(0.0012)
Schooling	0.049	0.038	0.065*	0.035
	(0.038)	(0.038)	(0.033)	(0.028)
Institutional quality	0.00087	0.0030	0.020**	0.019**
	(0.0082)	(0.0073)	(0.0094)	(0.0076)
Openness	0.080**	0.069**	0.12***	0.12***
	(0.034)	(0.035)	(0.024)	(0.026)
Instruments			48	49
Countries	58	58	61	61
Hansen			0.29	0.37
Hansen level			0.029	0.19
AR(1)			0.000094	0.000090
AR(2)			0.72	0.97
KP F stat	10.9	11.2		
Observations	336	336	339	339

Note: KP: Kleibergen-Paap. Instruments for the differences equation are log GDP lagged twice. Instruments for the levels equation are log GDP lagged and differenced once. Log predicted aid and trade are used as instruments in all regressions. All regressions include year effects. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

close to in Table 5. This once more tends to confirm that our results are robust. Column (3) collapses the instrument matrix, and reveals that the system GMM assumptions are likely to be violated. As previously, the GMM estimates appear to be quite fragile. On the other hand, we find it encouraging that all our system GMM specifications find an elasticity close to 0.05.

Table A.7: GMM, large sample

	(1) Diff. GMM	(2) Sys. GMM	(3) GMM-Collapse
Log GDP, lagged	-0.40**	-0.0036	0.067
	(0.18)	(0.053)	(0.053)
Log aid, lagged	0.071*	0.042**	0.0058
	(0.040)	(0.021)	(0.035)
Log population	0.65*	-0.013	-0.055
	(0.37)	(0.051)	(0.060)
Instruments	44	53	19
Countries	106	108	108
Hansen J test (p-val)	0.50	0.24	0.066
Hansen test (p-val), level		0.034	0.0053
AR(1)	0.13	0.012	0.011
AR(2)	0.52	0.25	0.22
Observations	609	717	717

Note: The dependent variable is the growth rate. Instruments for the differences equation are GDP lagged twice in all the specifications. Instruments for the levels equation are GDP lagged and differenced once. Predicted aid is used as an instrument. The matrix of instruments is collapsed in column (3). All regressions include year effects. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.7 illustrates the change in the estimated aid coefficient of the within group estimator when outliers are excluded from the sample. Figure A.7 reveals that a few observations lie very far from the main group and so drive the result. When these are excluded, the coefficient becomes positive and significant, as shown in section 5.3 and Table 9.

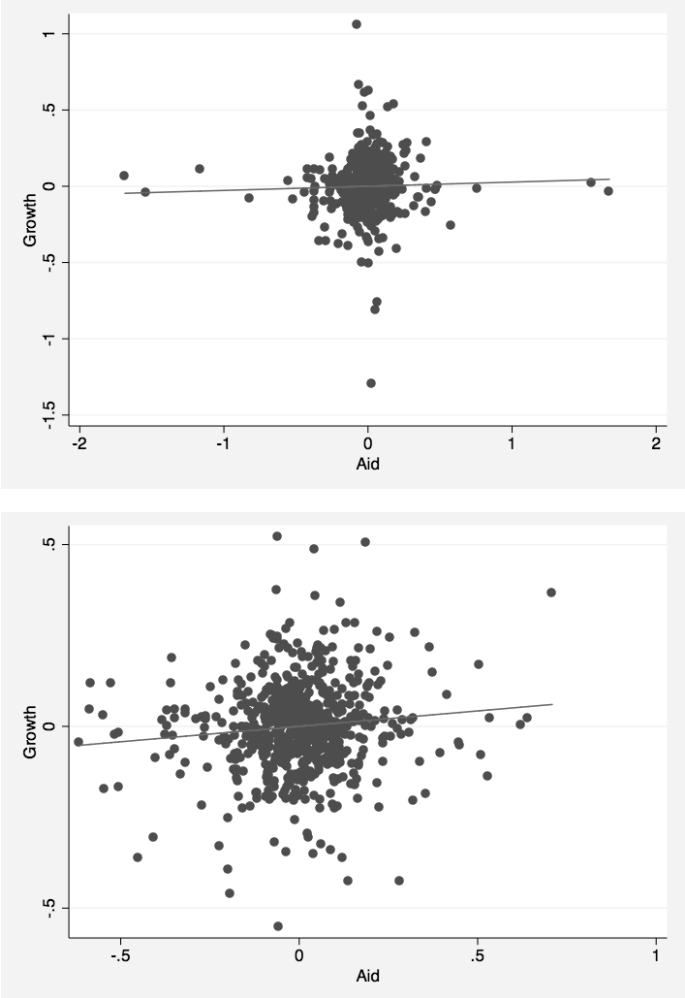


Figure A.7: Partial regression plot of growth on aid, including (top plot) and excluding (bottom plot) outliers, larger sample

Table A.8: List of countries

Sample with controls		**Large sample**	
Algeria	Malaysia	Angola	Palestinian Adm. Areas
Argentina	Mali	Barbados	Rwanda
Bangladesh	Mexico	Belize	Samoa
Bolivia	Morocco	Benin	Saudi Arabia
Botswana	Mozambique	Bhutan	Solomon Islands
Brazil	Niger	Burkina Faso	St. Lucia
Cameroon	Pakistan	Burundi	St.Vincent & Grenadines
Chile	Panama	Cambodia	Sudan
Colombia	Papua New Guinea	Cape Verde	Suriname
Congo, Dem. Rep.	Paraguay	Central African Rep.	Swaziland
Congo, Rep.	Peru	Chad	Timor-Leste
Costa Rica	Philippines	Comoros	Tonga
Cote d'Ivoire	Senegal	Djibouti	Vanuatu
Dominican Republic	Sierra Leone	Equatorial Guinea	Viet Nam
Ecuador	Sri Lanka	Ethiopia	
Egypt	Syria	Fiji	
El Salvador	Tanzania	Grenada	
Gabon	Thailand	Guinea	
Gambia	Togo	Guinea-Bissau	
Ghana	Trinidad and Tobago	Laos	
Guatemala	Tunisia	Lebanon	
Guyana	Turkey	Lesotho	
Haiti	Uganda	Libya	
Honduras	Uruguay	Madagascar	
India	Venezuela	Maldives	
Indonesia	Yemen	Mauritania	
Iran	Zambia	Mauritius	
Jamaica	Zimbabwe	Micronesia, Fed. States	
Jordan		Namibia	
Kazakhstan		Nepal	
Kenya		Nicaragua	
Liberia		Nigeria	
Malawi		Oman	

Note: The large sample corresponds to the regressions where the only controls are lagged log GDP, lagged log aid, and log population. Note that in addition to including more countries, the "large" sample also includes more observations for some countries than the sample with controls.

Chapter 3

Hidden Redistribution in Higher Education*

1 Introduction

Most countries spend more, in per student terms, on higher education[1] compared to the lower levels of education. The ratio of yearly expenditures per tertiary student to expenditures per student at lower levels is, however, much higher in less developed countries, and disproportionately high in Sub-Saharan Africa: Table 1 shows that it was 198.5 in 2003, more than 100 times as high as in the average OECD country. The amount spent on each university student in the average Sub-Saharan African country amounts to several times its GDP per capita (Figure 1).

*I am grateful to David Yanagizawa, Masayuki Kudamatsu, Jakob Svensson, Philippe Aghion, Martin Berlin, Julia Bird, Anders Olofsgård, Jesper Roine, Giancarlo Spagnolo, Niclas Berggren and the development study group at IIES - Stockholm University for contributions at different stages of this project. Thanks to Kimuli Kasara for sharing her data.

[1]While there is more international agreement on the definition of primary and secondary school levels, the types of schools and institutions that fall into the "higher education" category and the names given to them can differ considerably between countries. I include under the label of higher education, or tertiary education, everything that comes after the secondary level.

Table 1: Expenditures per tertiary student as a ratio of expenditures per student at lower levels, year 2003

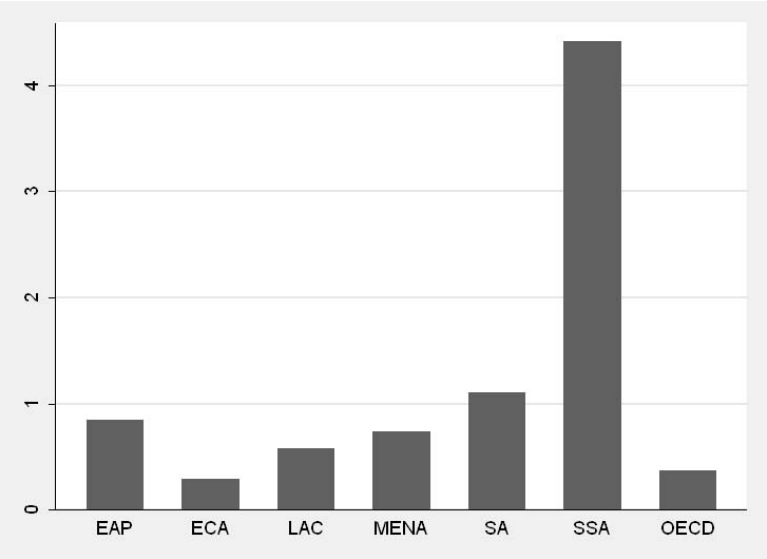
Region	*Primary*	*Secondary*
OECD	1.8	1.5
East Europe / FSU	2.0	1.5
Latin America	4.3	4.0
South Asia	5.6	3.3
Middle East / North Africa	5.4	5.3
East Asia	12.5	6.5
Sub-Saharan Africa	**198.5**	**81.1**

Source: Glewwe and Kremer (2006).

Many explanations can be offered to this phenomenon. This study focuses on the hypothesis that the extremely high levels of spending on tertiary education in 20 African countries partly reflect patterns of *hidden* redistribution. This label is used to describe situations where items of public expenditure or public projects are used as a disguise for redistributing resources to a particular constituency or special interest group. The premise of this argument is that, in these countries, higher education is not accessible to a large population base, and in some cases it is in fact markedly elitist.

Using an indirect method to construct long time series of university enrollment at the ethnic group level, I am able to observe the ethnic identities represented among students and relate them to the ethnicity of the political leader and the contemporaneous expenditure levels. Based on this strategy, I find that the hidden redistribution hypothesis can explain a substantial part of the within country variation in the level of expenditures. Leaders that belong to the ethnic group most represented among university students spend on average 1243 USD more per student enrolled, 62% of the standard devi-

Figure 1: Expenditure per student in tertiary education as a fraction of GDP per capita



ation of expenditure levels. Since the occurrence of such a leader in power is not very frequent, the cumulative effect over the whole period examined is not of big economic significance.

The remainder of the paper proceeds as follows: in the next section, I will refer to a theoretical framework to clarify when and why targeted redistribution is likely to take this disguised form. In Section 2.2, I will then motivate the particular choice of target groups on which this paper focuses, namely ethnic groups. Section 2 reviews some related literature. When it comes to the empirical strategy, in Section 4, I will discuss the methodology used to obtain ethnic-level panel data and describe some interesting patterns that can be observed in these data. Finally, I will present the specification for the regression analysis and the results in Section 5. Section 7 concludes the paper.

2 Theory

2.1 Inefficient redistribution

Why should redistribution be disguised, in particular if this implies an inefficiency? In other words, why should a political leader or government that wants to redistribute resources to a particular group prefer the indirect ways of implementing public projects? There must be "[...] surely an easier way of accomplishing that objective!", notes Rodrik (1994), with reference to charges of hidden redistribution in trade policy.

In a seminal contribution, Coate and Morris (1995) summarize the political economy debate on the form of transfers to special interest groups, and introduce a model of political competition where politicians that want to benefit a particular group can choose between direct cash transfers and a disguised transfer mechanism. This consists of a public project that benefits the target group but also, under some conditions, the general population. Two elements are key in their set-up: (1) the citizens have less information on the ex-ante conditions under which the project benefits them, and cannot observe even ex-post if the implementation was in their interest because the outcome is stochastic; (2) there are two types of politicians, one that only cares for the general welfare (good type) and one that also cares for some particular group (bad type), and the citizens cannot distinguish between them. Under these fairly general conditions, politicians will sometimes prefer to transfer resources to the special interest group by implementing public projects, even though making direct cash transfers would be more efficient due to reputational concerns.

On the first point, just like in the model, investment in higher education may or may not be beneficial to society in general. It certainly directly benefits those that are currently enrolled, or that have the possibility of enrolling - they must have completed the lower levels, and be able to afford it. This is often a restricted group, and not necessarily representative of the general

population in terms of socio-economic or ethnic background. Whether the benefits of this investment spill over to the rest of the population not attending depends on many other conditions, among which are demand for human capital in the labor market, matching investments in physical capital, and not least an efficient and meritocratic system for access to higher education. However, what is important is that these expenditures are *believed* to be beneficial. According to respondents to Afrobarometer surveys, education ranks very high among the services a democratic society is supposed to provide, even more than regular elections, majority rule, competing political parties, and freedom to criticize the government. Moreover, two-thirds (67%) of the respondents think that their governments are doing "fairly well" or "very well" at addressing their country's educational needs, and less than 10% think that the educational system faces widespread corruption problems.[2]

On the second point, how should we think about reputational concerns in this setting? Where do they come from? On the one hand, the set of constraints and incentives associated with the electoral game is virtually or literally absent in many of these societies. Still, the reputational concerns that are the engine of this model can come from elsewhere. These are highly heterogeneous societies, characterized by high ethnic fractionalization[3], see Table 2. The political leader, even an autocrat, might face the threat of social unrest or even riots or civil conflicts if she were to openly discriminate, or favor, one particular group.

Table 2 also shows that the share of official development assistance (ODA) to government expenditures averages 50.6% in Sub-Saharan Africa (SSA), and in some countries it exceeds 100%. Many of these countries are heavily dependent on their good standing with the international community, through

[2]As a reference, about half of the respondents think that corruption practices are common among civil servants and public officials, and only 47% expressed trust in the police.

[3]The measure shown, ELF, is the widely used ethno-linguistic fractionalization index by Roeder (2001).

Table 2: Summary statistics at the country level, 2000-2005, by region

Region	GDP p/c (2000 USD)	Public expenditures in tertiary ed.	Number of students in tertiary ed.	ELF	ODA
EAP	1527	390	1506206	.462	22.3
ECA	1940	510	939732	.401	20.6
LAC	3482	1004	580833	.437	18
MENA	3216	415	587042	.334	6.08
SA	904	1379	1822142	.471	24.8
SSA	1026	79.3	83768	.663	50.6
OECD	23842	11320	1485715	.237	.

Source: Education data from UNESCO, GDP and ODA from World Development Indicators. GDP per capita is in 2000 USD. Public expenditures are measured in 2000 USD million.

many channels, one of which is development assistance. And many aid-giving institutions, the World Bank and UNESCO among others, encourage university provision in SSA countries, as a means to general aims such as expanding enrollment, preventing the brain drain and laying the basis for the scientific R&D sector. Recently, the Inter-American Development Bank suggested in a report that increased access to higher education could be a route to reduce inequality in Latin America. More generally, as expenditure items are concerned, investments in higher education certainly meet less opposition from aid-giving institutions, as compared to open redistribution.

One final point relates to the actual possibility, or lack thereof, of political discretionality in the allocation of public funds. The way in which education systems are financed necessarily varies between countries. I cannot say much on the specific processes that lead to this result, but limit myself to note what the data show, namely that there is indeed an average effect and allocations to higher education do increase in connection with my redistribution hypothesis.

Summing up, this is the hypothesis we are testing: due to electoral con-

cerns, threat of social unrest, pressure from international actors, or more, the political leader may be using public expenditure on higher education to benefit a special interest group. The next section will discuss the specific choice of group on which this study focuses .

2.2 Ethnic politics

It is often argued that African leaders use public expenditure to support individuals from their region of origin or who share their ethnicity.[4] It is easy to list reasons why political support to a leader is often organized around the ethnic identity. An ethnic group is typically easy to mobilize, due to language and kinship ties, while at the same time the ascriptive nature of ethnic identity limits the size of the group in a natural way and partly screens opportunistic behavior. Moreover, from the individual supporter's perspective, ethnicity can be used as a proxy for the candidate's preferences, otherwise imperfectly observable, and this might give the coethnic candidate an edge over an opponent of different ethnicity.

On top of these theoretical arguments, there is a very practical perception of material benefits that can arise for a group when coethnics hold political power. The belief that people benefit from patronage in such situations emerges from the politicians' rhetorics, and from the observed patterns of voters' support, besides a few conspicuous examples (see Kasara (2007)). Founded or not, this is a very widespread belief in many African societies. But why would a leader target her own coethnics for benefits? On altruism or reciprocity grounds, holds the *core supporters* or pure patronage hypothesis: the leader derives indirect utility from benefiting her coethnics; or alternatively, she "owes" her position in power to her supporters, who hence must be retributed. Moreover, it is easier for the leader to please her coethnics, due to better information on their preferences, and better contacts and intermediaries among the group. On the other hand, it might be more politically

[4]Posner (2005) and Bates (2008).

efficient to target others than core supporters. In a standard probabilistic competition model (Lindbeck and Weibull (1987)), if voters derive "psychic" benefits from having a coethnic in power, their vote becomes cheaper in terms of material benefits. This is ultimately an empirical question, and a few attempts have been made to assess it, some of which are discussed in the next section. By linking elite groups and ethnicity, the specific approach of this paper offers one more ground for evaluating the conventional wisdom on patronage politics in Africa.

3 Literature

> "In the case of Sub-Saharan Africa, economic growth is associated with low schooling, political instability, underdeveloped financial systems, distorted foreign exchange markets, high government deficits, and insufficient infrastructure. Africa's high ethnic fragmentation explains a significant part of most of these characteristics." (Easterly and Levine (1997))

Ethnic diversity has become a very popular right-hand-side variable. A large literature connects it to a wide range of outcomes, and a substantial part of it lets the link work through politics, and ethnic favoritism in particular. This work has been relying on the theoretical arguments and the anecdotal accounts referred to in the previous section. Only recently have there been some attempts to empirically assess ethnic favoritism. Franck and Rainer (2009) relate the individual probability of completing primary school or losing a child aged below one to the contemporaneous rule of a coethnic. They find that respondents whose primary school years fully coincided with the rule of a coethnic leader were 2.47 percentage points more likely to attend and 2.04 percentage points more likely to complete primary school. Moreover, they find that children born during their mother's coethnic rule were .53 percentage points less likely to die during their first year of life.

However, they do not convincingly address the endogeneity of changes in leadership. Kudamatsu (2009) exploits a plausibly exogenous change in the leader's ethnicity for Guinea 1984, and finds no effect on infant mortality.

Although they reach different conclusions, there is a common point in these two approaches: they focus on outcomes at the population level, bypassing the implicit link through policies. This is not completely straightforward in the case of outcomes such as school participation, that require many levels of incentives and individual choices. The authors argue that the relatively better performance of the leader's coethnics with respect to primary education must reflect a relatively higher quality or quantity of services provided to them by the public sector. However, the effect identified cannot be distinguished from a pure participation response in the population. Seeing a member of one's ethnic group reaching the top leadership of the country may inspire and motivate, give a perception of better future prospects and, for these reasons, induce higher participation in school, somewhat like the so-called Obama effect[5] in the US.

It is more straightforward to say something about ethnic favoritism by directly looking at policies rather than outcomes. Moser (2008) divides the ethnic groups of Madagascar into *swing*, *supporting* or *opposing*, with respect to the two presidential candidates in the 2001 elections, and finds some evidence of both swing voter targeting, according to the public choice theory, and ethnic patronage in the district allocation of public projects, depending on the type of projects. Burgess et al. (2010) find that the President's district of birth received an additional 46.33 km of paved roads every three years in Kenya, over the period 1961-1992. Kasara (2007) instead shows that African leaders tax their coethnics more heavily. This result is also interpreted in the spirit of swing voter targeting: the politician does not need to favor *core supporters* who would vote for her anyway, like in the case of her coethnics, and can even extract surplus from them to instead benefit groups that would

[5]Marx et al. (2009).

not support her without a *quid pro quo*.

The approach of this paper is similarly related to policy. An important difference is that I look at a public good that is not targetable to a specific district or group. In cases of geographic targeting, ethnic favoritism can lead to high regional inequalities and political instability, with long-term consequences being as dramatic as ethnic riots or civil conflicts (Easterly and Levine 1997, Montalvo and Reynal-Querol 2005). This provides an argument against the idea that clientelistic redistribution via public goods is politically inefficient, because it does not allow to distinguish between supporters and dissidents (Bates (2008)). A public good such as higher education is beneficial, at least in theory, to the general population, and is more easily tenable against charges of favoritism. In practice, though, given the accessibility of this level of education and the actual ethnic composition of students, in some cases such a public good can become a form of *de facto* targeted transfer.

The empirical methodology I use is instead closely related to that used in Franck and Rainer (2009), as will be detailed in the next section.

4 Data and method

To test the hypothesis, one would ideally like to know the ethnic composition of tertiary level students in every country and year. Unfortunately, there has been no collection of panel data at the ethnic-group level. Therefore, I use an indirect method to retrieve this information, starting from Demographic and Health Surveys (DHS). The DHS are publicly available nationally-representative household surveys that provide data for a wide range of indicators in the areas of population, health, and nutrition. I use the information on ethnicity, attained education and age in the DHS to predict the share of each ethnic group in the total number of students, by country and year. In other words, I proxy the ethnic composition of students in different years with the composition of different age cohorts in the survey. For

example, to predict the number of university students belonging to ethnic group j in 1980, I use the number of respondents that in the survey year, f. i. 2006, are between 44 and 51 of age (18 to 25 back in 1980), belong to group j and report to have been enrolled in tertiary education. I do the same for all three levels of education, and also for a number of occupations that are in some sense closer to the public sector: civil servants, custom and tax office employees, education professionals, police and military.[6]

These projections may deviate from the actual shares for a number of reasons. The survey sample is representative of the population in the survey year, but there is no guarantee that each age cohort is equally representative. Moreover, we only know if a respondent has attended tertiary education, but we do not know where[7]: this can potentially induce systematic biases in connection with the patterns of political power. I will discuss this later, after presenting the results.

The information on ethnicity, which is not present in all DHS surveys, limits the sample to 20 countries. Table 3 reports the period covered for each country, which is the period for which at least some respondents are in the age bracket 18-25. There is an average of 15 ethnic groups in each country which, over 44 years, generates about 15000 observations.

[6]The civil servants heading includes government officials and MPs, when applicable, as well as administrative and bureaucratic personnel; the second heading groups together custom officers and tax authority employees; the education sector includes teachers and administrative or other support staff, although for this category I cannot distinguish between public and private schools; finally the last group includes police and military personnel. The occupation classification from the DHS can differ between countries, and some of these categories are missing in some countries; the shares computed in this way should hence be viewed as approximations.

[7]We do not know when, either. I assume that all respondents attended university when they were between 18 and 25.

Table 3: Time period covered, ethnic groups in power, number and type of transitions

Country	Year of survey	Period covered	Total groups	Groups in power	Changes in power	Democratic changes	Violent changes	Leader from dominant group	Leader from overrepresented group
Benin	2001	1963-2006	8	3	6	2	3	10	15
Burkina Faso	2003	1963-2006	10	2	4	0	3	17	14
CAR	1994	1963-2001	9	4	3	1	2	2	24
Chad	2004	1963-2006	12	3	2	0	0	4	15
Congo, Rep.	2005	1963-2006	63	4	4	1	2	0	23
Cote d'Ivoire	2005	1973-2006	6	2	1	0	1	0	0
Ethiopia	1997	1963-2004	68	3	3	1	3	27	41
Ghana	2003	1963-2006	8	3	5	3	3	11	39
Guinea	2005	1963-2006	6	2	1	0	1	0	28
Kenya	2003	1966-2006	13	2	2	1	0	4	17
Liberia	1986	1963-1993	16	4	5	1	2	0	0
Malawi	2004	1967-2006	8	3	3	2	0	2	8
Mali	2006	1964-2006	7	1	0	0	0	19	25
Namibia	2000	1963-2006	9	1	1	1	0	0	0
Niger	2006	1964-2006	8	3	2	2	1	11	34
Rwanda	1992	1963-1999	2	2	1	0	1	27	17
Senegal	2006	1963-2006	6	2	1	1	0	3	26
South Africa	1998	1966-2005	4	2	1	1	0	19	30
Togo	1998	1963-2005	5	2	0	0	0	2	5
Zambia	2002	1963-2006	54	3	3	3	0	0	23

Source: Ethnicity of the leader from Kasara (2007). "Period covered" refers to the years in which at least some respondents are between 18 and 25 year old. "Dominant group" refers to the ethnic group to which the most tertiary students belong, if any group exceeds 50%. Any group whose share in tertiary students exceeds the population share is "overrepresented". A power transition is defined as democratic if there was a contested election, according to the coding by Cheibub et al. (2010). Violent transitions are coded according to data from INSCR.

In the table, the predicted data are combined with information about the ethnicity of the leader in power, defined as the President, or the head of a Cabinet, or an autocratic leader, from Fearon et al. (2007). Their sources include country histories, general and country-specific reference works, press reports, government websites and official biographies. Although most of the countries in SSA and in my sample have a presidential constitution and the President is a strong power, it would be ideal to know the ethnic composition of the government and the parliament, or wherever lies the decisional power in terms of budget allocations. This information is not currently available, although a large collection project is in progress.

According to the information about the leaders, there were at most six changes in power for any given country during this period. Only a few of them occurred under democratic rule: I consider this to be the case if democratic elections were held, allowing parties outside of the regime front, according to a new dataset by Cheibub et al. (2010). Many of the power transitions, on average more than one per country, happened as coups or in connection with violent incidents.[8] As for the ethnic identity of the leader, in three cases it never changed over the whole time period; for the remaining countries, up to four different groups were in power during the period considered. Finally, the last two columns in Table 3 report information on how often the leader belongs to the group that I call *dominant* and how often the leader comes from an *overrepresented* group. *Dominant* is a group that has the largest share of university students, given that it is larger than 50%; any group that has a larger share of students than its population share is labeled as *overrepresented*. In many cases, no group exceeded the threshold, but only Gabon and Zambia never had any dominant group; on the other hand, only in Rwanda has there always been one dominant group: Hutu before 1994, and Tutsi after 1994. Figure 2 reports the aggregate patterns of persistence of groups in positions of power and dominance in universities.

[8]This information comes from the Integrated Network for Societal Conflict Research.

Figure 2: Persistence of group positions over time

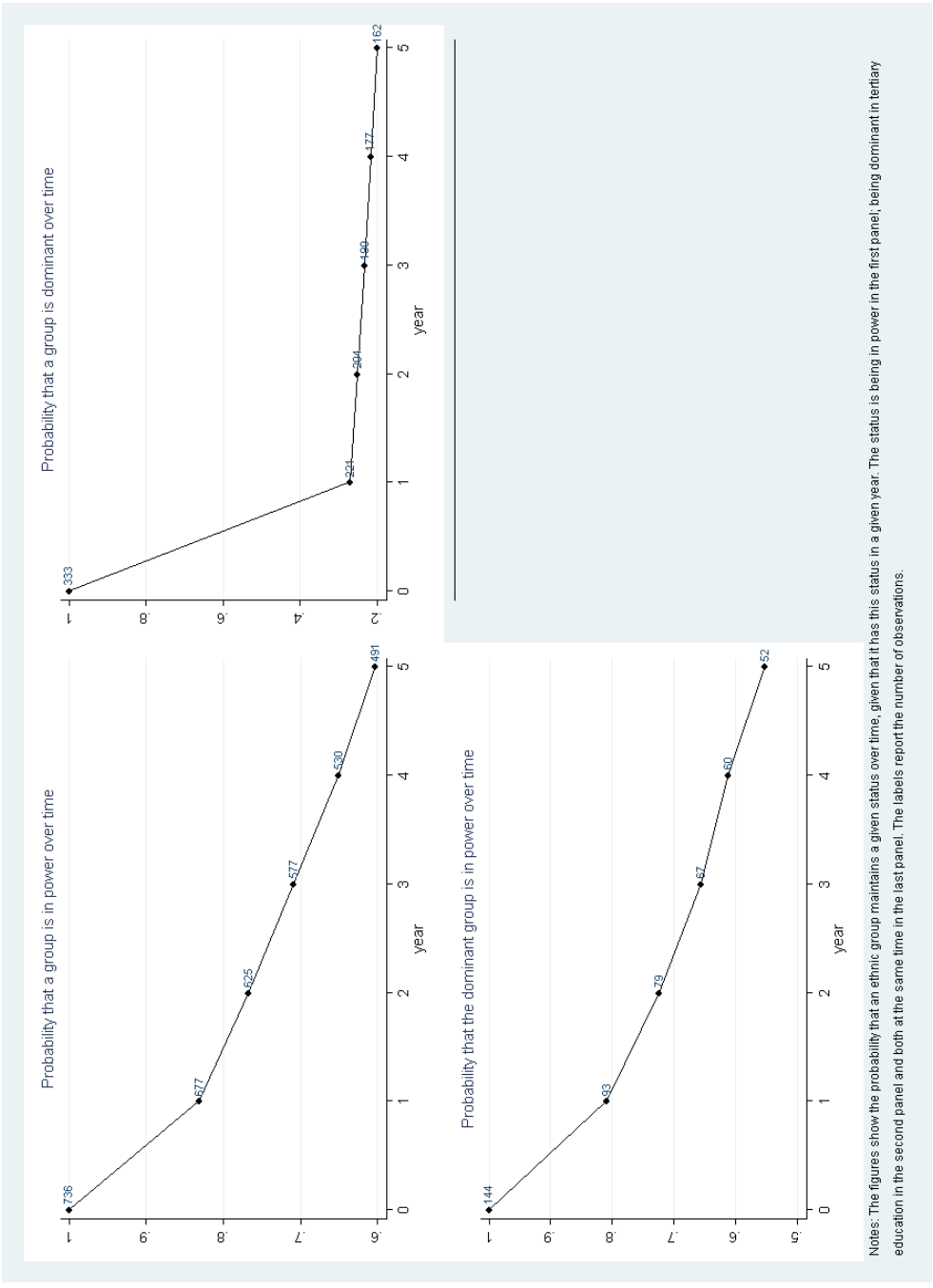


Table 4: Shares of enrolled or employed in the relevant age group (mean and sd) for different ethnic groups

	Primary educ	Secondary educ	Tertiary educ	Civil servants	Customs or tax officers	Educ sector	Police and military
Never in power	.667	.292	.0383	.00214	.000343	.0292	.00148
	(.343)	(.296)	(.111)	(.00851)	(.00846)	(.0958)	(.018)
At least once in p.	.659	.307**	.0432**	.00856***	.000549	.0239*	.00108
	(.305)	(.247)	(.0741)	(.0284)	(.00252)	(.0358)	(.00444)
Currently not in p.	.661	.305	.0404	.00722	.000785	.0252	.00104
	(.304)	(.24)	(.0694)	(.0226)	(.00303)	(.0391)	(.00484)
Currently in p.	.654	.311**	.0478***	.0107***	.000162	.0218*	.00114
	(.307)	(.257)	(.0812)	(.0359)	(.00122)	(.0296)	(.00369)
Dominant groups	.629	.298	.0686	.00201	.00176	.0169	.000616
	(.279)	(.236)	(.125)	(.00606)	(.00388)	(.021)	(.00132)
Other groups	.667**	.295	.0383***	.0031	.000343**	.0287*	.00144
	(.338)	(.289)	(.105)	(.0137)	(.00794)	(.0906)	(.0169)

Source: Elaborations on the DHS surveys.

Notes: *Dominant group* refers to the ethnic group with the largest share of tertiary students, if larger than 0.5. The numbers in the table are shares of the population in the relevant age belonging to a specific group. The stars indicate that the difference in shares within a panel is statistically significant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

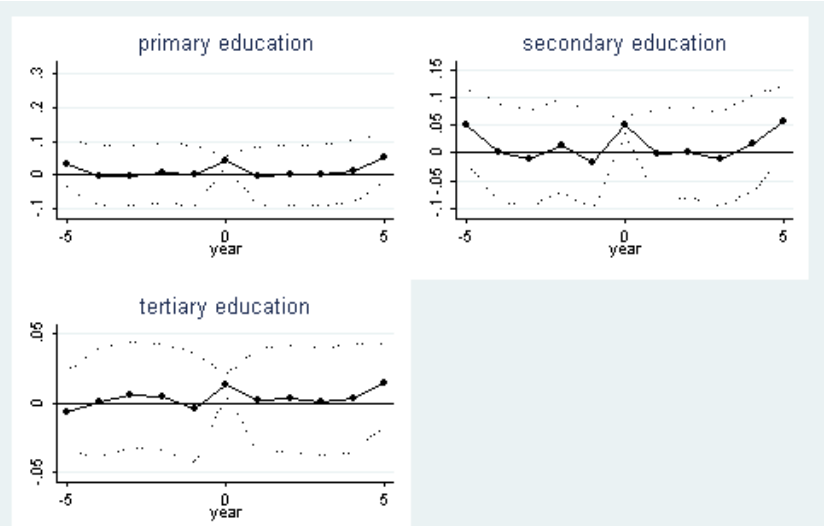
Table 4 shows the shares of different groups that are enrolled in the three levels of education and employed in the four occupational categories. In the first panel, ethnic groups that have never been in power are contrasted with groups that have been in power at least once. For the latter, the second panel shows separately the averages for the years when they were in power against the years when they were not in power. Finally, the third panel compares the dominant groups to all other groups. The numbers in the table are shares of the population in the relevant age belonging to a specific group; for example, the top-left cell says that, for groups that have never been in power, 64% of the children between 7 and 12 attend primary school on average over the whole time period; and so on. The stars indicate that the difference within the table panel is significant at conventional levels.

Groups that have been in power at least once seem to on average have more civil servants. As shown by the middle panel, groups that are currently in power (the coethnics of the current leader) are more represented among civil servants and military personnel, and also at the higher education level. Instead, the shares of the dominant groups in the four occupations do not differ significantly from the shares of the non-dominant groups; this suggests that obtaining a significantly larger share of public sector occupations does not simply follow from being the largest group in higher education.

The simple averages reported in the table pool together groups that belong to different countries and are observed at different points in time and hence, do not make use of the panel structure of the data. Figures 3 and 4 and Table 5 show the estimates from a distributed lags model of those same population shares, both in the three education levels and in the four occupational categories. The econometric model estimated is the following:

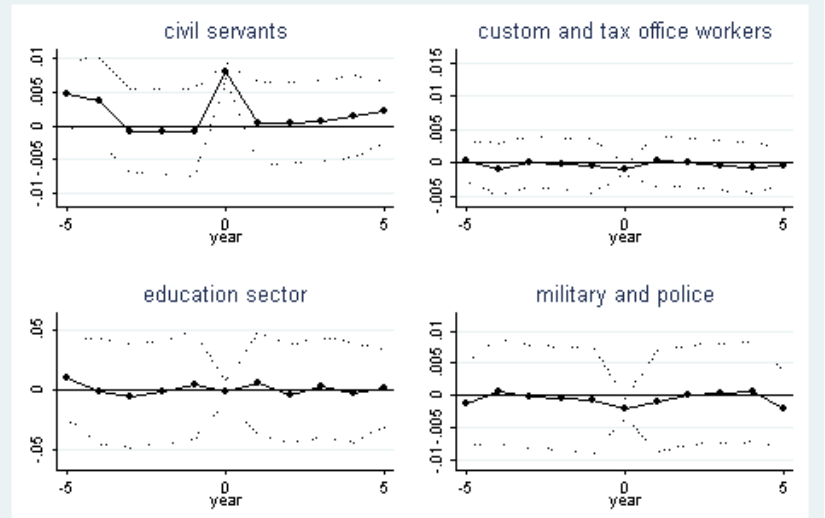
$$Share_{jit} = a_0 + aP_{it} + \sum_{k=1}^{5} b_k Lag_{kit} + \sum_{k=1}^{5} c_k Fwd_{kit} + \mu_i + \nu_t + \epsilon_{jit}, \quad (1)$$

Figure 3: Share of enrolled for each ethnic group



Notes: The three plots show the share of enrolled in each school level for the average ethnic group, in deviation from the country-year average, over time. Year 0 denotes periods when the group is in power.

Figure 4: Share of employed for each ethnic group



Notes: The four plots show the share of employed in each sector for the average ethnic group, in deviation from the country-year average, over time. Year 0 denotes periods when the group is in power.

Table 5: Average shares of enrolled and employed over time

	(1) Prim	(2) Sec	(3) Ter	(4) Civil	(5) Custom	(6) Teacher	(7) Police
lag5	.0328	.0501***	-.00613	.00471	.000351	.00933	-.00135
	(.0230)	(.0190)	(.00614)	(.00297)	(.000379)	(.0107)	(.00128)
lag4	-.000807	.00142	.000940	.00376	-.000888	-.00142	.000498
	(.0310)	(.0251)	(.00750)	(.00461)	(.000747)	(.0154)	(.00163)
lag3	-.00242	-.0104	.00620	-.000740	.000164	-.00531	-.000139
	(.0308)	(.0234)	(.00785)	(.00536)	(.000849)	(.0134)	(.00162)
lag2	.00477	.0143	.00425	-.000835	-.000161	-.00191	-.000531
	(.0299)	(.0232)	(.00927)	(.00470)	(.00108)	(.0128)	(.00164)
lag1	-.000635	-.0163	-.00363	-.000917	-.000417	.00349	-.000772
	(.0295)	(.0247)	(.00904)	(.00442)	(.000976)	(.0105)	(.00171)
Group is	.0402***	.0491***	.0136***	.00803***	-.000875***	-.00122	-.00202***
in power	(.00596)	(.00501)	(.00221)	(.00169)	(.000269)	(.00130)	(.000617)
fwd1	-.00180	-.00194	.00178	.000367	.000307	.00548	-.00102
	(.0342)	(.0247)	(.00852)	(.00458)	(.000628)	(.00662)	(.00156)
fwd2	.000835	.00163	.00356	.000414	.0000394	-.00379	.0000793
	(.0348)	(.0250)	(.00907)	(.00376)	(.000702)	(.00639)	(.00149)
fwd3	.000966	-.0110	.000631	.000640	-.000352	.00232	.000302
	(.0341)	(.0267)	(.00840)	(.00320)	(.000756)	(.00672)	(.00148)
fwd4	.0127	.0169	.00336	.00142	-.000689	-.00231	.000520
	(.0339)	(.0291)	(.00759)	(.00213)	(.000844)	(.00575)	(.00150)
fwd5	.0522**	.0572***	.0140**	.00206**	-.000431	.00120	-.00224*
	(.0260)	(.0222)	(.00568)	(.000976)	(.000684)	(.00433)	(.00118)
R^2	.605	.470	.078	.148	.068	.077	.084
Countries	22	22	22	14	14	14	14
Obs	10545	11411	12056	8514	8514	8514	8514

Note: The dependent variable is the share of each ethnic group enrolled in the relevant school level or employed in the relevant sector. Standard errors clustered at the country level in parentheses. Clustered standard errors are biased in small samples. Robust standard errors are anyway very similar in size. All regressions include country and year fixed effects, so shares can be interpreted as deviations from the country-year averages. Lags and fwds are hence expected to be 0. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

where P_{it} is a dummy for the group being in power in country i in year t while Lag_{kit} (respectively, Fwd_{kit}) is an indicator taking the value of 1 if the group was in power k periods ago (is going to be in power k periods from now) *given that* it is not in power in year t. Hence, a is the average change in the share across all years when the group is in power, relative to the average change in other groups' shares in the same country and year (the equation includes a country-specific effect and year fixed effects mu_i and nu_t). Each b_k and c_k instead represent a placebo test for the periods when group j is not supposed to differ from other groups in the country, on average, in terms of the change in the group's share of a particular level of education or occupation, because it is not in power. All these coefficients are expected to be 0.

The figures show that the annual change in a group's members that participate in a given level of education or are employed in a given occupation is significantly different in the years when the political leader is a coethnic as compared to other groups within the country. These results, that extend the recent findings in Franck and Rainer (2009), are meant to be suggestive about the presence and extent of "ethnic politics" in general, showing whether people actually react to having a coethnic in power, and whether they benefit from it, for example in terms of occupational prospects. They do not tell the whole story in terms of ethnic favoritism, though, as discussed above, because they might come entirely from a response of the population to the change in leadership, without any need for an actual change in policies. Hence, we now move back the focus from population outcomes to policies, namely the pattern of (over)spending in higher education.

4.1 Other data sources

The data on public expenditures, enrollment, school age, in the period 1970-2007, are from UNESCO's EdStats. Table 6 reports the summary statistics at the country level. These data are not complete and balanced over the whole time period: although the panel spans a 37-year period, there are only on

average 4.85 observations per country. This limits what can be tested empirically; however they remain the most comprehensive data available. Figure 5 shows the cross-country variation of expenditures within SSA. Although, as shown in Figure 1, the levels are on average higher than in the rest of the developing world, there is substantial amount of variation in expenditures across countries within SSA. More in particular, the cross-country variation in expenditures seems related to GDP per capita, and also to the number of enrolled students, as shown in the correlation table. Data on ODA, GDP and population, as well as the geographic classification, are from the World Development Indicators.

Table 6: Public expenditures in education and enrollment in the sample

Country	Expenditures per stud. (USD)			Total expenditures (% of GDP)			Number of stud. (th.)		
	Primary	Secondary	Tertiary	Primary	Secondary	Tertiary	Primary	Secondary	Tertiary
Benin	46.5	146	509	1.73	.909	.562	799	214	18.3
Burkina Faso	69.8	130	1796	2.55	.453	.565	714	149	12.1
CAR	23.2	131	1823	.969	.535	.506	340	49.7	3.87
Chad	14.6	57.6	719	.753	.49	.268	731	118	5.3
Congo, Rep.	108	214	3637	1.86	1.28	1.12	467	161	10.5
Cote d'Ivoire	107	780	1879	1.99	1.92	.92	1564	439	38.4
Ethiopia	32.5	52.4	886	1.65	.738	.722	4711	1219	74.1
Ghana	31.6	92.2	2259	1.57	1.3	.908	2261	964	52.6
Guinea	40.2	111	1178	.986	.559	.453	667	196	13.4
Kenya	81.9	141	1951	3.69	1.23	.639	4982	1426	57
Liberia	.	198	2015	.	.539	.387	300	69.9	18.6
Malawi	12	81.7	1938	1.37	.375	.658	2102	64	4.25
Mali	47.6	131	871	1.49	.882	.492	819	174	14.3
Namibia	413	620	2271	4.45	2.51	.723	380	81.8	11.4
Niger	45.4	200	625	2	.796	.303	544	95.4	5.46
Rwanda	27.6	171	2060	1.57	.419	.547	1266	97.3	11
Senegal	83.3	227	1582	1.85	1.17	.949	918	231	31.9
South Africa	473	579	1637	2.52	1.78	.777	6747	3854	601
Togo	24.3	78.2	1325	1.5	1	1.05	772	214	9.69
Zambia	23.9	203	2861	1.19	.76	.496	1599	212	15.1
Average	98.9	232	1745	1.93	.995	.702	1706	482	60.7

Figure 5: Average public expenditure per university student as a fraction of GDP per capita, SSA

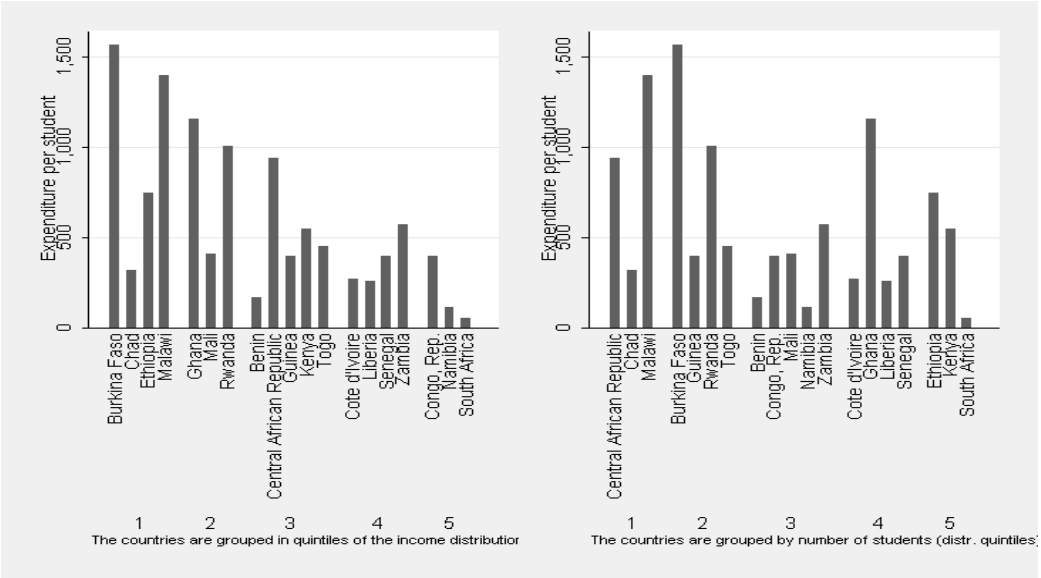


Table 7: Simple correlations

	Expenditures per tertiary student	
GDP per capita	-0.234***	
	(0.0460)	
Tertiary enrollment		-863.5***
		(228.4)
R^2	0.214	0.131
Countries	20	20
Observations	97	97

Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5 Empirical analysis

5.1 Specification

Figure 6 uses Kenya as an example of the variation that will be exploited by the regression model. The figure plots the share of tertiary students belonging to the two ethnic groups that alternate in power during this period: Kikuyu-Meru and Kalenjin. The expenditures pattern relates quite remarkably, in this case, to the rule of the two groups, which differ substantially in terms of student shares. Expenditures increase when the group with a higher share of students is in power, and fall under the rule of the other group.

Formally, I estimate the following equation:

$$Exp_{it} = \alpha + \beta Z_{it} + \sum_k \gamma_{1k} pow_{kit} + \sum_k \gamma_{2k} dom_{kit} + \sum_h \delta_h x_{hit} + tr_{it} + \mu_i + \xi_{it} \tag{2}$$

The dependent variable is public expenditures per tertiary student in country i, year t, measured in constant dollars. I use two alternative specifications for the variable of interest, Z_{it}. The first is an indicator that takes the value of 1 when the top political leader belongs to the dominant group, as defined above: the group with the largest share of tertiary students, provided that this share is larger than 50%. Since this threshold is arbitrary, as any other threshold would be, and given that this indicator is always 0 for many countries in my sample, I also use the share of tertiary students that are cohetnic with the leader in order to exploit all information in the sample. The control set x_{hit} includes GDP per capita and the number of students enrolled. Moreover, in all specifications, I include country-specific fixed effects and time trends.

The identification of the causal effect of a "dominant leader" relies on the presence of indicators for all ethnic groups in power and all ethnic groups that are in a dominant position, pow_{kit} and dom_{kit}. In other words, Z_{it} compares the expenditure level of a group in power in the years when the group is

dominant to the expenditure levels when the same group is in power but not dominant or dominant but not in power. This addresses a potential omitted variable problem, namely a group-specific preference for high education. If we observe high participation of group k in tertiary education (dominance), and high expenditures when the group is in power, this could simply be due to the fact that members of group i attach a high value to education: this group-specific preference would imply that, at the same time, more students from the group enroll in tertiary education, and leaders from the group spend more, without implying any redistribution. This effect is ruled out by the specification of equation (2).

All these indicators are based on my predicted data. As mentioned above, the predicted shares might be systematically biased in connection with the patterns of political power. For example, if members of a minority group which is strongly discriminated against go to university abroad, this group would be overrepresented in my panels, and induce me to underestimate the fraction of students that belong to the leader's group, which is the main variable of interest in my analysis. This would push the estimate towards zero, leaving a "lower bound" interpretation for the coefficient. If, on the other hand, it is the leader's coethnics that go abroad more often when their group is in power, for example because it is easier for them to get scholarships or funding, this could confound my results and make them difficult to interpret, because I would overestimate the share of coethnics that actually are at the university. This issue is reasonably a minor one, though, as relatively few students go abroad to study,[9] and moreover I can almost certainly exclude that they are in the pool of respondents of the DHS.

[9]According to UNESCO, international students from all of Africa, at all levels, totaled 161,877 in 1999, which gives an upper bound estimate of 8% of all enrolled tertiary students. Even if all of them came from the leader's ethnic group, this would not change my results to any considerable extent.

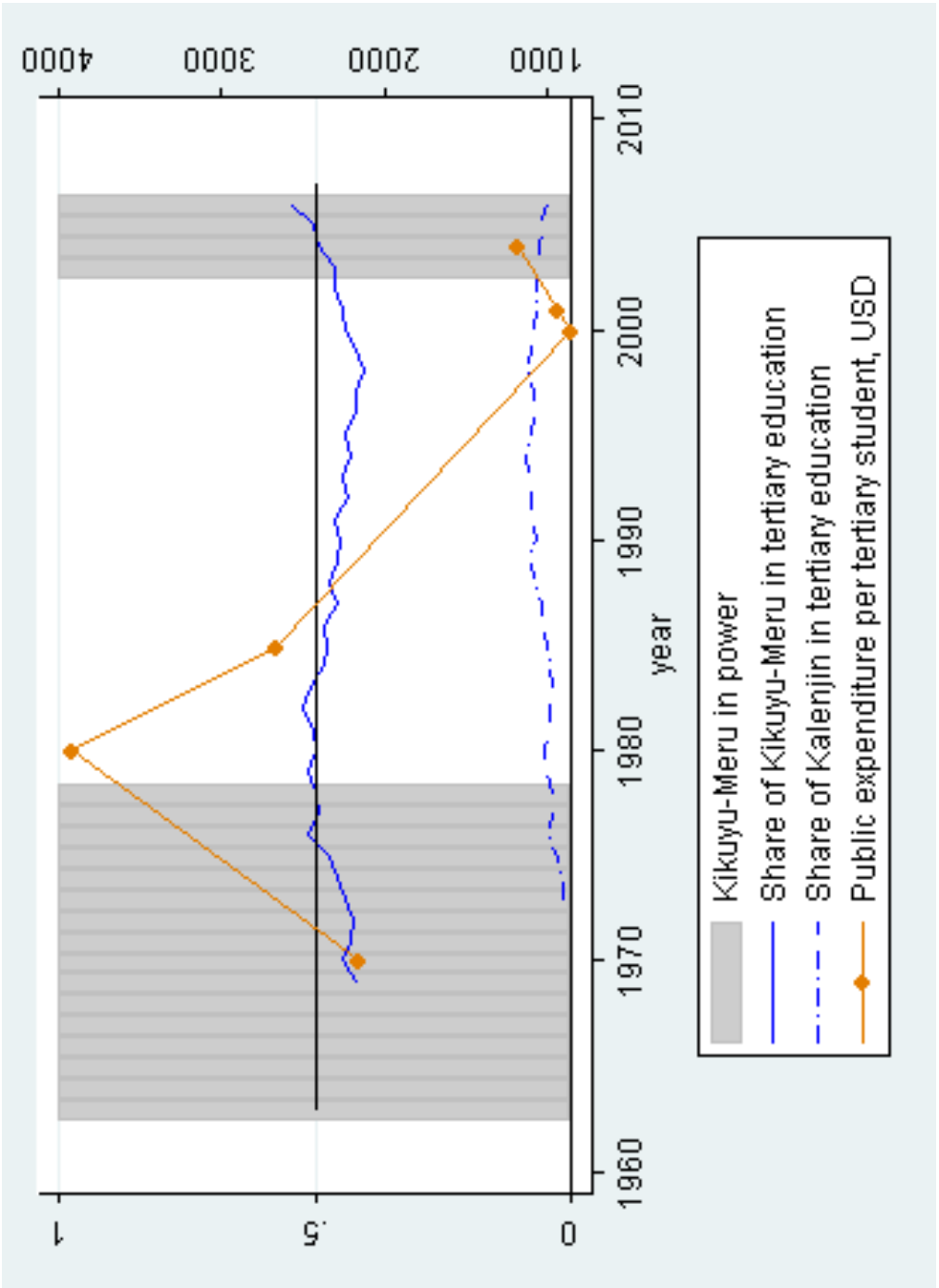


Figure 6: Ethnicity in power, ethnicity of students and expenditures in Kenya

5.2 Results

Table 8 reports the fixed effect estimations for the panel data on expenditures. The first column shows that the expenditures per tertiary student increase by about 1,243 USD on average, or a 57% increase, in the years when the leader belongs to the dominant group. The effect corresponds to about one standard deviation in this variable (1,220 USD). The regression includes dummies for the identity of the group in power and for the identity of the dominant group, to avoid a potential omitted variable, namely group-specific preferences about high education.

Variation in the indicator variable comes from only 4 out of 18 countries, Benin, Ethiopia, Ghana and South Africa. In the remaining countries, the indicator is either always 1 (Mali and Rwanda), or always 0, in most cases (46 out of 71 country-year observations) because there is no dominant group in that country and year, according to the (arbitrary) threshold of 50%. In order to exploit at best all the information in the sample, I hence use in column (2) the share of university students coethnic with the leader instead of the dummy variable. The size of the effect is as follows: a one standard deviation change in this share (.24) is associated with 15% of a standard deviation change in expenditures (318 USD). Columns (3) and (4) use the expenditures in logs, to smooth a variable that otherwise takes on quite extreme values.

As a placebo test, I look at two other categories of spending, with the same specification of Table 8. In Table 9, expenditures per student at the primary level seem to decrease slightly, while nothing happens at the secondary level. No change is found for total education expenditures either expressed as a share of GDP or in USD per student (result not shown). These figures once more suggest a redistribution pattern: the allocation of public funds is moved away from primary school, a general interest policy, to the tertiary level, which is much more elitist. Also note that tertiary students, as opposed to secondary students, have voting rights.

Given the limitations of the expenditures data discussed above, as a further test I collapse them at the country level, using the most recent data on expenditures. The idea is that current levels of expenditures are the result of a history of sequential public investments and budget allocations, which might have been affected by the political history of the country, so that a summary measure of this history should partly explain current cross-country differences. Figure 7 shows the relation between current expenditure levels and the share of years during which the dominant group has been in power, controlling for per capita income and number of students. This measure of expenditures for the university level is positively related to the share of years during which the dominant group has been in power, although the coefficient is not significant. In Figure 8, this relationship is shown to hold also when looking at the expenditures as a share of GDP per capita.

Figure 7: **Expenditures and frequency in power of the dominant group in a cross section, conditional on GDP p.c. and number of students**

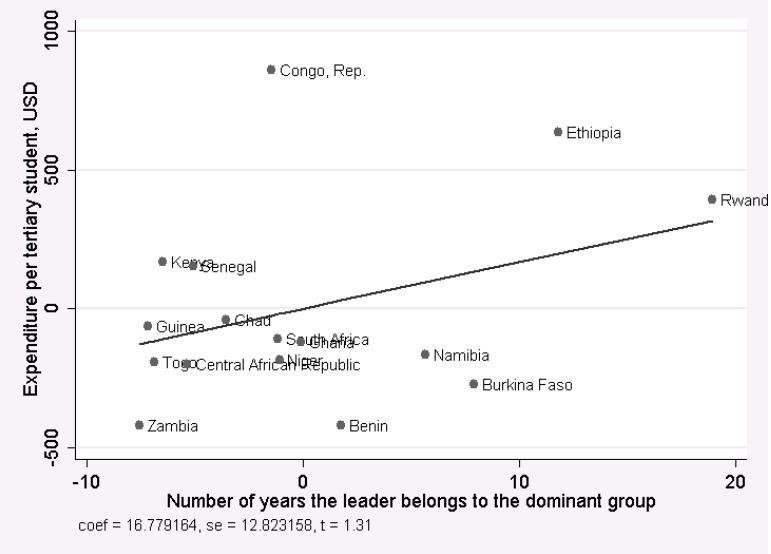


Table 8: Public expenditures per tertiary student

	(1)	(2)	(3)	(4)
	Expenditures in USD		Expenditures in logs	
Leader from dominant group	1243.3*		0.573*	
	(640.6)		(0.299)	
Share of coethnic students		1324.4**		0.728***
		(484.2)		(0.240)
Tertiary enrollment	-15253.7	28.55	-8.240	-1.012
	(11598.4)	(4969.2)	(5.417)	(2.081)
GDP per capita	3.828	3.020	0.00160	0.00122
	(3.118)	(2.247)	(0.00133)	(0.000843)
R^2	0.764	0.755	0.840	0.842
Countries	18	18	18	18
Observations	88	88	88	88

Note: Standard errors clustered at the country level in parentheses. Clustered standard errors are biased in small samples, although the extent and direction of the bias is not entirely clear. Robust standard errors are anyway very similar in size. All regressions include country fixed effects, country-specific time trends, as well as incumbent-group and dominant-group dummies. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Public expenditures at lower levels

	(1)	(2)	(3)	(4)
	Expenditures in USD		Expenditures in logs	
		Primary education		
Leader from dominant group	-21.77**		-0.155	
	(9.024)		(0.184)	
Share of coethnic students		0.621		0.0655
		(15.21)		(0.423)
Primary enrollment	-7.730	-9.471*	-0.110	-0.123
	(4.553)	(5.151)	(0.0948)	(0.101)
GDP per capita	0.302***	0.319***	0.00163	0.00178
	(0.0648)	(0.0690)	(0.00135)	(0.00127)
R^2	0.990	0.989	0.958	0.956
Countries	18	18	18	18
Observations	101	101	101	101
		Secondary education		
Leader from dominant group	53.38		0.551	
	(42.20)		(0.392)	
Share of coethnic students		-2.839		0.329
		(49.37)		(0.265)
Secondary enrollment	2.091	0.600	-0.472	-0.317
	(44.78)	(44.59)	(0.404)	(0.443)
GDP per capita	-0.0436	-0.0379	0.000360	0.00000914
	(0.649)	(0.687)	(0.00237)	(0.00263)
R^2	0.969	0.967	0.938	0.927
Countries	18	18	18	18
Observations	96	96	96	96

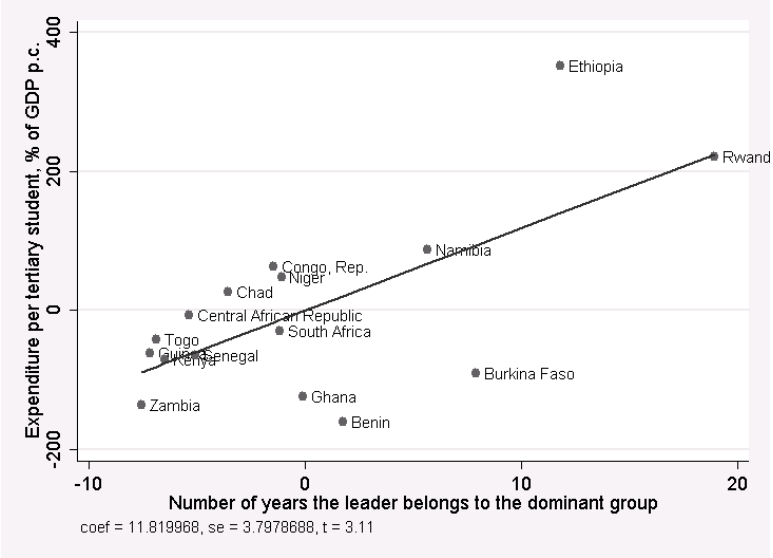
Note: Standard errors clustered at the country level in parentheses. Clustered standard errors are biased in small samples, although the extent and direction of the bias is not entirely clear. Robust standard errors are anyway very similar in size. All regressions include country fixed effects, country-specific time trends, as well as incumbent-group and dominant-group dummies. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Discussion

The empirical exercise in this paper identified an increase of about 1,243 USD in the level of per student expenditures in the years when a leader from the dominant group is in power. This is an actual policy choice by the leadership of these countries, and does not simply reflect a population response, as would an effect observed in participation. Moreover, the effect is not preference-driven. It is not the case that group-specific values or cultural factors lead the group members to participate more and the group leaders to spend more on higher education. These 1,243 USD seem to be an actual transfer from a political leader to her group. But how large is this transfer? How costly for society?

First of all, we can relate it to the anomaly in expenditures observed in Sub-Saharan Africa as compared to other regions of the world, which was highlighted at the beginning of the paper. A back of the envelop computation

Figure 8: **Expenditures and frequency in power of the dominant group in a cross section, conditional on GDP p.c. and number of students**



allows us to visualize the importance of this source of variation over the whole period, and return to the inter-regional comparison. If the political leader was never of the same ethnicity as the dominant group in tertiary education, or if there was no dominant group in the first place, expenditures in higher education would on average be lower by 1,243 USD for every year that the indicator dummy takes the value of 1. This simulated lower level of expenditures is plotted in 9, to be compared with Figure 1 discussed in the introduction. In cumulative terms, not very much of the SSA "expenditures anomaly" is accounted for by this particular source of variation, only 6.6%. Hence, more work is needed to shed some light on this fairly well known but surprisingly little explored puzzle.[10]

A number of alternative explanations can be proposed for future work. The most intuitive one is that setting up a higher education system requires some form of fixed cost that is at least partly independent from the number of students enrolled: from the physical infrastructure (buildings, labs, ...) to wages for faculty. Teachers' salaries in general are by far the largest component of government spending in education and, moreover, qualifications for this level must be relatively scarce in the population. The compensation must also be competitive with the high wages offered by international organizations, multinationals and the like.[11] Globalization of the labor market must also be pushing the standards for the education system upwards towards international levels and hence, the costs. The enthusiasm of policy makers and international institutions for high education, already referred to earlier in the paper, might also be a factor pushing upwards the investment levels, irrespective of the demand side (the number of students enrolled), maybe with the hope of also stimulating demand. It might also be the case that the amounts recorded as public expenditures for higher education do not, or do only partly, reach the intended objective. Reinikka and Svensson

[10]The scarcity of data on this level of education probably lies behind the lack of studies.

[11]Political economy factors behind a high wage bill for teachers are proposed by Pritchett and Filmer (1999).

(2004) document diversions of public expenditures for primary education in Uganda; something similar might be also happening for the higher education level.

Finally, we might want to try to assess how costly this particular form of (disguised) transfers is for society, for example by comparing it to other documented instances. 1,243 USD per student amount to about 74 million USD per year in the average country.[12] In comparison, the effect identified by Burgess et al. (2010), a transfer to the president's district in the form of 46.33 additional kilometers of paved roads, can be converted, by their own estimates, to a monetary cost of 18.5 million USD every three years. Moreover, the disguised transfers in higher education amount to 14% of the average total budget for education, which is a big waste given the serious needs that most of these countries still suffer in the educational sector.

[12]However, this does not happen every year. The leader belongs to the dominant group only for about three months in the average country, so the yearly amount of transfers is considerably smaller, 14 million USD, if smoothed over the whole period.

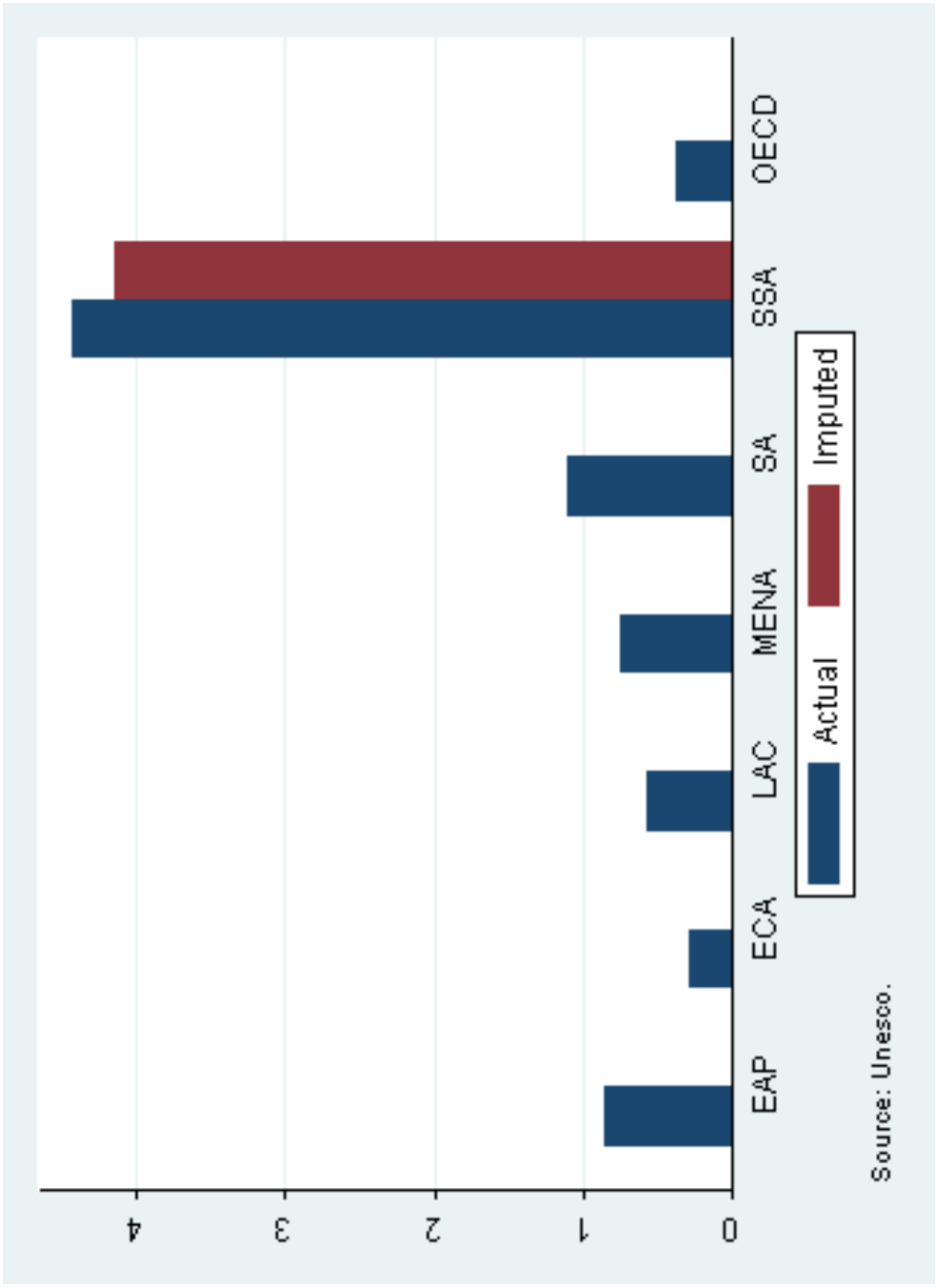


Figure 9: Expenditure per student in tertiary education as a fraction of GDP per capita, after removing the variation due to the leader's ethnicity

6 Conclusions

This study advances and tests the hypothesis that the patterns of (over)-spending in higher education observed in SSA partly reflect disguised redistribution along the lines of ethnic favoritism. This hypothesis finds support in the data, and accounts for a large portion of the within-country variation in expenditures levels. However, this result is limited to those countries where a specific ethnic group represents a majority of the tertiary students, and a political leader belonging to the same group happens to come into power in some years. The hypothesis according to which this leader increases the expenditures on tertiary education to benefit her ethnic group can be distinguished from a pure participation response from the group itself and also from group-specific preferences for tertiary education. Although this particular mechanism is shown to play a role in the within-country pattern of expenditures, it does not contribute very much to explain the Sub-Saharan Africa expenditures "anomaly".

Bibliography

Bates, R. H. (2008). *When things fell apart: state failure in late-century Africa*. Cambridge University Press.

Burgess, R., R. Jedwab, E. Miguel, and A. Morjaria (2010). Our Turn To Eat: The Political Economy of Roads in Kenya.

Cheibub, J., J. Gandhi, and J. Vreeland (2010). Democracy and dictatorship revisited. *Public Choice 143*(1), 67–101.

Coate, S. and S. Morris (1995). On the Form of Transfers to Special Interests. *The Journal of Political Economy 103*(6), 1210–1235.

Easterly, W. and R. Levine (1997). Africa's Growth Tragedy: Policies and Ethnic Divisions. *Quarterly Journal of Economics 112*(4), 1203–1250.

Fearon, J., K. Kasara, and D. Laitin (2007). Ethnic minority rule and civil war onset. *American Political Science Review 101*(01), 187–193.

Franck, R. and I. Rainer (2009). Does the Leader's Ethnicity Matter? Ethnic Favoritism, Education and Health in Sub-Saharan Africa. Unpublished manuscript, Department of Economics, Bar Ilan University and Department of Economics, George Mason University.

Glewwe, P. and M. Kremer (2006). Schools, teachers, and education outcomes in developing countries. *Handbook of the Economics of Education 2*, 945–1017.

Kasara, K. (2007). Tax me if you can: Ethnic geography, democracy, and the taxation of agriculture in Africa. *American Political Science Review 101*(01), 159–172.

Kudamatsu, M. (2009). Ethnic Favoritism: Micro Evidence from Guinea. IIES Working Paper Series.

Lindbeck, A. and J. Weibull (1987). Balanced-budget redistribution as the outcome of political competition. *Public Choice 52*(3), 273–297.

Marx, D., S. Ko, and R. Friedman (2009). The "Obama Effect": How a salient role model reduces race-based performance differences. *Journal of Experimental Social Psychology 45*(4), 953–956.

Moser, C. (2008). Poverty reduction, patronage, or vote buying? The allocation of public goods and the 2001 election in Madagascar. *Economic Development and Cultural Change 57*, 137–162.

Posner, D. (2005). *Institutions and ethnic politics in Africa*. Cambridge University Press.

Pritchett, L. and D. Filmer (1999). What education production functions really show: a positive theory of education expenditures. *Economics of Education Review 18*(2), 223–239.

Reinikka, R. and J. Svensson (2004). Local Capture: Evidence From a Central Government Transfer Program in Uganda. *Quarterly Journal of Economics 119*(2), 679–705.

Rodrik, D. (1994). What Does the Political Economy Literature on Trade Policy (Not) Tell UsThat We Ought To Know? NBER Working Papers 4870.

Chapter 4

The Impact of a Food For Education Program on Schooling in Cambodia*

1 Introduction

There is today a wealth of programs and policies generally designed to achieve the two Millennium Development Goals (MDGs) of universal primary gender disparities in education. Food for Education (FFE) programs, which consist of meals served in school and, in some cases, take-home rations and deworming programs conditional on school attendance, are considered as a powerful means for this aim, particularly in areas where school participation is initially low. Compared to other programs, such as conditional cash transfers and scholarships, school meals may provide a stronger incentive to attend school because children must go to school to receive the rations. Moreover,

*This paper is coauthored with Maria Cheung, Stockolm University. We are grateful to the World Food Programme in Cambodia for providing the data. The authors thank Jakob Svensson, David Strömberg, Andreas Madestam, Martin Berlin, Erik Lindqvist, Olof Johansson-Stenman and all participants at IIES and Department of Economics seminars at Stockholm University for valuable comments.

the provision of food can contribute to alleviate short-term hunger during the school day and thus improve learning and cognitive outcomes for undernourished children. The largest international implementer of these programs in the developing world is the World Food Programme (WFP) with 102 million beneficiaries in 78 countries in 2008. This study is an evaluation of the impact of WFP's Food for Education program in Cambodia, which was implemented in primary schools (grades 1 to 6) in six Cambodian regions between 1999 and 2003. Beyond the average impact of the program, we also investigate who benefits most. Finally, we tentatively assess how cost-effective such a program is compared to other types of interventions.

The program was phased-in across six provinces (of 24 in total) between 1999 and 2003, allowing us to examine three different forms of FFE programs: i) in-school breakfast, ii) in-school breakfast together with a take-home ration provided to families of poor girls in grades 4 to 6, and iii) the "full package" consisting of in-school breakfast, poor girls' take-home rations and deworming medicine to all participating schools. The identification of the effect is based on a difference-in-difference strategy exploiting the variation in the exposure to the program both across time (before and after) and across geographical location (treated and non-treated schools or communes).

We find that the impact of the program on enrollment varied according to the type of FFE program. School enrollment always increased during the first year of treatment, for any type of program, and this effect is largest from the full package program. The enrollment continued to increase at a somewhat slower pace in the following years, with the only exception of the 2000 treatment group, where enrollment increased less than in the control schools in the second year of treatment. This may point to supply constraints (the schools reach their full capacity after the enrollment increase of the first year). An alternative interpretation is that because of a general growth trend also involving control schools, the treatment only affects the timing of enrollment growth in treated schools.

Turning to the second set of results based on the individual level data, our intention-to-treat estimates reveal that children of primary school age who live in a commune with at least one treated school (regardless of what treatment group) on average have 1.8 months longer education and a 10 percentage point higher probability of being in school. We find that the probability of being in school in 2003/04 is highest for the group of children who started treatment the year before. For the same group of children, we do not find any strong evidence that they also stayed in school longer as compared to a control group: at the point when we observe them, they did, on average, complete the same grade. On the contrary, children who started treatment three years before (in 2000/2001) are not comparatively more likely to be in school in 2003/04 but, at the point when we observe them, have completed a higher grade than the control group. An intuitive explanation is that a longer duration of treatment (at least three years) is needed to keep the children in school for additional school years. Alternatively, it might be the case that we do not see any effect on highest completed grade for the 2001 and 2002 treatment groups because we observe them too soon after the treatment. Looking at the heterogeneous effects, we find that the program had a stronger impact on the highest grade achieved by girls, children of low educated fathers and children from middle income families and on the probability of being in school for children of low educated parents.

The contribution of our paper is threefold. First, we are able to make a comparison between three different types of FFE schemes, which has previously only been done in a few studies. The evidence on the central policy question of the cost-effectiveness of such programs is even more rare. We compare each type of FFE program to alternative programs and find that the full package scheme yielded the highest impact on enrollment per dollar spent. One plausible explanation, consistent with previous evidence,[1] is

[1]Miguel and Kremer (2004) find that the cost per additional year of school participation is only 3.50 USD which is very cost-effective compared to other programs.

that this is due to the deworming treatment, known to be very effective in attracting children to school and at the same time being extremely cheap. Moreover, most studies focused on enrollment as an outcome. Given our rich set of data, we are also able to investigate different measures of participation and attendance and say something about the class size effect. This is our second contribution.[2] Third, our study also links to a broader debate about alternative schemes aimed at reducing the cost, including the opportunity cost, of education for poor families. Although the school fee for primary school is completely subsidized in Cambodia, there is evidence that other cost burdens still dissuade the poorest families from sending their children to school. Policy interventions directly targeting the poor have been shown to be the most effective means of increasing participation rates in developing countries.[3] From a policy perspective, if the major objective is to increase short-term enrollment, then our findings are encouraging but if the objective is to make children stay longer in school as well as to improve their learning, more efforts are needed on the supply side (teachers and classrooms).

The remainder of the paper is organized as follows. The next section reviews the FFE programs in general and previous studies. Section 3 presents some general background and the details of the Cambodian FFE. Section 4 describes the data and the methods used, as well as providing the descriptive statistics. We present the quantitative results in section 5, and a cost-benefit analysis in section 6. Section 7 concludes the paper.

[2]Only one robust study looked into similar issues and found that school meals for preschool children displaced teaching time and led to larger class sizes (Vermeersch et al. (2005)). However, that study is confined to pre-school children.

[3]See Glewwe and Olinto (2004), Schultz et al. (2004), Attanasio et al. (2006), Todd and Wolpin (2006), Barrera-Osorio et al. (2008) on cash incentives; Miguel and Kremer (2004) on deworming programs.

2 FFE in general and previous studies

The objective of FFE programs is to promote households' investments in the human capital of their children. By comparing potential future benefits of education to current costs, parents decide how much to invest in the education of each child. There are two types of educational costs, direct costs (school fees, supplies, books, uniforms, and travel to school) and indirect costs, for example the opportunity cost of the child's time: instead of being in school, the child could be caring for other family members, working on a family farm or in a family business, or working outside the household to provide additional income. By subsidizing these schooling costs through FFE programs, greater investment in education may be achieved.

FFE programs generally take two forms: in-school meals and take-home rations. Compared to other demand-side incentive programs (conditional cash transfers and scholarship programs), school meals provide a stronger incentive to attend school because the child has to be in school in order to receive the meal. Moreover, take-home rations work as a complementary cash transfer, compensating the household for the foregone income that would be generated by the child if not in school. Take-home rations are food rations given to the household conditional on a child's enrollment in school and a minimum level of attendance. Take-home rations focus relatively more on improving food security at the household level (Pollitt (1995)). Sen (2002) argues that in-school meals are superior to take-home rations since the former contribute to the nutrition of children and thus complement teaching[4] as well as enhance school attendance. They might also reduce abuse and corruption that arise in a dry ration system due to the fungibility of the distributed rations. On the other hand, school meals may also disrupt teaching and learning if class time is substituted for meal time.[5] The major objectives are

[4]Because the meals are served before the school-day, the child learns more effectively, undistracted by short-term hunger and hence more able to focus.

[5]See Vermeersch et al. (2005). Breakfast programs designed to cause as little disruption

the same, however: to increase food consumption and improve educational outcomes and the nutritional status of the children. Many of the FFE interventions also offer other components, related to education, nutrition, or health including deworming programs.

The broad range of contexts in which FFE interventions have been implemented has led to an increasing awareness of the potential benefits of FFE for different outcomes including education, nutritional status, social equity and agricultural development. Given the growing popularity of such interventions across the developing world, and the resources targeted towards them, it is important that these hypotheses are rigorously evaluated.

The literature on the impacts of FFE programs is very large, and almost unanimous in suggesting that these programs have considerable impacts on primary school participation (Jacoby et al. (1996); Ahmed (2004); Ahmed and Del Ninno (2002)), in particular for girls (Kazianga et al. (2009); Afridi (2010)). School feeding coupled with take-home rations seems to have a greater impact on girls' enrollment compared to that of boys (Gelli et al. (2007); Kazianga et al. (2009)). The empirical investigations based on experimental or quasi-experimental designs providing causal evidence is relatively scant. Vermeersch et al. (2005) conducted a randomized evaluation of the impact of school meals on participation in Kenyan preschools, and found that school participation was 30 percent greater in the 25 Kenyan preschools where a free breakfast was introduced than in the 25 comparison schools. In schools where the teacher was relatively well trained prior to the program, the meals program led to higher test scores (0.4 of a standard deviation) on academic tests.

Despite these potential benefits, there is an ongoing debate among donors and policy-makers on the point that these programs are an expensive method for producing the stated education and nutrition objectives and that other

as possible (served outside the normal teaching time) may therefore be the best policy choice.

cost-effective mechanisms exist. Few studies investigate the cost-effectiveness of FFE programs and the types of school feeding programs that are most effective. There are also very few studies that look at the differential impacts of FFE on children by age and gender, and compare the impact on both enrollment and school attendance. There is a number of reasons why these two outcomes may differ. In some cases, enrollment numbers cannot be trusted, because the schools might have incentives to boost them in order to receive more funds. On the other hand, it is also possible for a child to attend school without being enrolled, maybe due to incomplete school records. Taking both these measures into account would give policy-makers a broader picture of the impact of the program.

Given our rich set of data, we are able to investigate different measures of participation and perform a deeper analysis about the effect of the program beyond enrollment. Moreover, the program studied in this paper takes, in the different waves, three different forms: i) on site meals, ii) on site meals and take-home rations, and iii) the "full package", i.e. on site meals and take-home rations together with a deworming program, which allows us to make comparisons.

3 Background

After decades of political unrest, Cambodia has in the last decade experienced political stability and high rates of sustained economic growth, at nearly 9 percent on average. Despite the progress, Cambodia remains one of the least developed countries in East Asia. Its GNI per capita was estimated at approximately 550 USD in 2007 and about 35 percent of the total population live below the poverty line.[6] Agriculture, mainly rice production and small-scale subsistence agriculture, is still the main economic activity for a majority of households. In primary education, enrollment is still far

[6]See Cambodia Demographic and Health Survey, DHS, 2005.

from being universal although the government is committed towards this goal. Most children enroll in primary school but a large share complete only two or three grades. Based on figures from the national school census,[7] the net enrollment rate for primary education was 89 percent in 2007, while the primary dropout rate was 46 percent.

The recent global economic crisis threatens to have a considerable negative impact on poverty reduction and educational outcomes. In 2008, the domestic price of rice doubled as compared to the previous year while meat and fish prices went up by 30-60 percent, whereby many children were withdrawn from school.[8] The children had to join the workforce in order to complement the reduced household incomes. Moreover, the FFE program, running since 2000, was cut due to the soaring global prices, increasing the cost of schooling for families.[9] Past instances of similar real income shocks in combination with increases in commodity prices have shown to constitute a significant risk to educational outcomes for the poor. For example, the 1997 economic crisis in Indonesia led to a doubling of the children out of school,[10] while droughts in Sub-Saharan Africa have been associated with declines in both schooling and child nutrition.[11] The global food, fuel, and financial crises have therefore created a new role for FFE programs as a potential safety net and as a social support measure that helps keep children in school.

3.1 The Cambodian FFE

The Cambodian FFE program started in 1999-2000 as a pilot project in the Takeo province[12] with only school feeding and was phased in during the

[7]Education Management Information System (EMIS) maintained by the Ministry of Education, Youth and Sports (MoEYS).

[8]See "Safety nets in Cambodia. Concept note and inventory"; CARD, WFP and WB (2009).

[9]Source: WFP Food Security Atlas for Cambodia.

[10]See Frankenberg et al. (1999).

[11]Schady (2008).

[12]See a map of Cambodian provinces in the appendix.

following three years. It was first undertaken by the WFP and the World Bank jointly with the Ministry of Education, Youth and Sports (MoEYS) as part of a larger WFP Relief and Recovery Operation.[13] The following year, the school feeding program was running in Takeo, Kampot and Kampong Cham provinces. Children were provided with one meal per day (breakfast) before school which contained the standard WFP ration of rice, canned fish, vitamin A fortified vegetable oil, and iodized salt in order to meet the minimal daily nutritional needs of students. The participating schools were required to provide fresh vegetables, water and fuel for the preparation of the WFP-supplied commodities. Parents and community members who volunteered to prepare the hot meal received a dry ration of rice for their help. The costs for providing the meals, apart from WFP's food provision, were born by the community.

In 2001-2002 the program continued in cluster schools[14] where additional inputs from the World Bank-supported Education Quality Improvement Project (EQIP) within the MoEYS together with other primary education, health, and community support programs were available. This expansion was undertaken in cooperation with a local NGO, Kampuchean Action for Primary education (KAPE), and UNICEF to include 407 schools and about 291,593 students in five provinces, Kampot, Kampong Cham, Kampong Speu and Prey Veng. In addition, take-home rations for families of 16,000 girls in grades 4 to 6 were being piloted this year as an incentive to keep these girls in school: girls of those ages are in fact more vulnerable to dropout. The program experienced a further expansion in 2002-2003 to include an additional province (Kampong Thom) and introduce a deworming

[13]The broad goal of this operation is to sustain food security among chronically hungry poor, along with the promotion of re-emerging social cohesion and support systems. Some of these activities include food for work which is a food-based safety net program to the chronically and transient poor, school feeding to primary schools, and rice-banks to counter the chronicle cycle of debt in rural areas.

[14]This definition refers to a particular administrative structure, in which different school levels are clustered under a common administration.

program to all participating schools: in collaboration with the Ministry of Health, WHO and UNICEF, WFP provided deworming medicine to students and infection prevention training for all teachers and students.

In addition to providing school meals during the day, WFP operations also helped establish complementary health and sanitation activities to improve the overall educational environment. These activities include the identification of safe drinking water and improvements in basic health, hygiene and sanitation practices for students at school and at home. HIV/AIDS prevention education was also a fundamental part of the educational package.

The phase-in structure of the program is summarized in Table 1.

Table 1: WFP School Feeding Coverage 1999-2003

	1999-2000 Pilot	**2000-2001**	**2001-2002**	**2002-2003**
PROVINCE (Partners)	Takeo (EQIP)	Takeo (EQIP) Kampot (EQIP) Kg Cham (KAPE)	Takeo (EQIP) Kampot (EQIP) Kg Cham (KAPE) Kg Speu (UNICEF) Prey Veng (UNICEF)	Takeo (EQIP) Kampot (EQIP) Kg Cham (KAPE) Kg Speu (UNICEF) Prey Veng (UNICEF) Kg Thom (UNICEF)
SCHOOLS	64/320	201/593	403/1,078	565/1,122
PUPILS	37,500	125,000	291,593	317,053
TYPE OF FFE	On-site	On-site	On-site Take-home	On-site Take-home Deworming

Note: The number of treated schools and pupils reported in this table is according to the ex-ante planning by the implementing institutions and may differ from the actual numbers that we observe in the data.

The selection criteria

The selection of schools in the pilot phase was based on the Cambodia Vulnerability Analysis and Mapping (VAM), which is a WFP technical tool used

worldwide to assess and analyze food security in order to target interventions. The analysis and mapping involve taking measures of human vulnerability[15] across the various geographical areas of the country, and creating maps to visually present the information. In general, two composite indexes are used for school feeding programs: an index of the need for basic education (that looks at primary and lower secondary school aged children) and an index of the need for adult education (that looks at the adult population aged above 15). The communes with the lowest values for these composite indexes have the highest levels of need for education and hence, should be given the highest priority for intervention.[16] However, given that the targeting required a significant amount of staff time and attention and that the criteria and procedures were changed almost annually, the implementers were recommended by their evaluation team to put less emphasis on commune targeting. These criteria were only supposed to work as broad guidelines and not function as the sole basis of selection. In fact, after the pilot year, the selection of schools was based on school clusters under the EQIP project, plus the formal submission and commitment by the schools themselves to prepare all cooking and storage facilities.[17]

As discussed later, the rule of prioritizing the most vulnerable schools was not followed. However, we found that the schools selected for treatment were systematically different in terms of lower repetition rates. The fact that treatment was given to better performing schools in this sense and that the self-selection connected to the formal submission and commitment to prepare the food might cause biases in our estimates: a selection bias might imply

[15]Vulnerability is defined as anything that increases the likelihood of a person suffering disadvantage or deprivation of any kind.

[16]For methodological details of the vulnerability analysis and mapping exercise, we refer the reader to the project technical reports, published by the RGC and WFP in 2002 and 2003.

[17]Source: WFP (2000), "Mid-term evaluation of PRRP Cambodia 6038.00", WFP/EB.2/2000/3/6; WFP (2000), "Full Report of the Evaluation of CAMBODIA PRRO 6038 - Food Aid for Recovery and Rehabilitation", Rome.

that we overestimate the effect of treatment, while mean reversion might lead us to underestimate it. We further discuss these potential biases and our approach for dealing with them later in the paper.

4 Data and methods

4.1 Data

The data used in this paper come from multiple sources. School level data are drawn from the Education Management Information System (EMIS) maintained by the MoEYS.[18] The main panel on which we base our analysis spans the whole length of the program, from 1998 to 2003, covering 8,443 schools from all 24 provinces.[19] The data can be perfectly merged with the information on treatment status that has the same school identification number. We have access to an additional EMIS panel (same source as the main one but lacking the school identification numbers) that covers 5,250 schools between the years 1997 and 2002. Information on treatment status is here merged based on the location name (province, district, commune and village) and school name. The merging may not be exact due to alternative spellings of location and school names, so we use the file for robustness checks.

Individual level data are taken from two waves of the Cambodia Socio-Economic Survey (CSES 1999 and CSES 2004), large-scale nationally representative household surveys collected by the National Institute of Statistics.[20] Using this dataset, we can analyze two more outcomes: the highest

[18]The EMIS includes information on enrollment and repetition rates broken down by grade and gender; teaching staff age, education, experience and gender; and various school characteristics such as number of classrooms and other facilities as well as school location, income, parents associations, etc.

[19]3089 schools in our six provinces of interest.

[20]CSES 1999 covers 6000 households and was carried out from January to August 1999. CSES 2004 covers 15000 households and spans from November 2003 to January 2005. Besides the socio-economic background variables (consumption, age, sex, income, etc.), this dataset contains more detailed information about schooling at the individual level:

educational achievement, which is based on the survey question "What is the highest level ..[NAME].. successfully completed?" and the probability of being in school in a given year which is based on the following survey question "Is ..[NAME].. currently in the school system?". The former is an indicator of whether the child actually completed the full school year. Although it measures the length or the quantity of education in a long-run perspective, it also says something about the quality of education, because it implies that the children did not just attend school for the sake of free food, but also completed the full school year. Given the huge influx of enrolled children due to the FFE program, if the schools adjust their resources (teachers and classrooms) according to the increased number of children in school, then countervailing effects from crowded classrooms negatively affecting teaching quality and learning are less likely to happen. Instead of a short-term enrollment and a high pupil turnover, we would rather observe an actual increase in the highest grade achieved for, in particular, the most vulnerable children that would otherwise have dropped out. The latter outcome is an indicator of enrollment that, in contrast to the school data, should be less subject to the overreporting problem, since it is self-reported by the household. Another difference is that it might not only capture the enrollment but rather the attendance since a child might have incomplete school records and be unable to enroll but still attend school. Unfortunately, there is no information on which specific school the individuals are attending. Based on the school data, we are able to see that there is only slightly more than one school in each commune and hence, the commune level would be the closest to the treatment assignment level. We merge the information on treatment status at the commune level and thus adopt an intention-to-treat approach.

The Cambodian Demographic and Health Survey (DHS) from 1998 is used to check the pre-treatment summary statistics at the village level. The

attendance and highest grade completed, literacy, but also reasons for not attending, as well as total costs (including school fees, text books, other school supplies, allowances for children studying away from home, transport costs, even gifts to teachers).

DHS is a nationally representative survey with a sample size of 5000 households.

4.2 Descriptive statistics

Table 2 reports the pre-treatment summary statistics from the main school panel and the DHS, showing differences in enrollment, repetition rates, school and village characteristics between treated and non-treated units in 1998. Selection bias might be a concern, due to the VAM criteria followed in prioritizing schools for treatment, as detailed in the previous section. However, the treated schools do not seem to be generally worse-off before the treatment: they have slightly lower repetition rates, if anything, and they are less likely to be defined as disadvantaged by the MoEYS, and more likely to have a parents' association. The average class size is not significantly different. Only the student/teacher proportion is slightly worse in treated schools. By and large, though, the data do not reveal that particularly badly performing schools were selected into the program. The village level data from the DHS 1998 show that the treatment and control villages did not differ significantly in terms of educational outcomes for the adult population either.

To control for potential unobservable confounding factors, we use school fixed effects. However, we cannot control for potential confounding factors that change over time. For example, it could be the case that less often being defined as disadvantaged and having more parents' associations gives these schools better prospects in terms of future performance. Table 2 reveals that these differences, though significant, are very small, however.

Table 2: Pre-treatment summary statistics

	Control	Treatment	Diff.	P-value	Obs.
		SCHOOL LEVEL			
Enrollment					
Grade 1	126.9	124.8	-2.168	0.625	2236
Grade 2	86.1	85.4	-0.616	0.856	2236
Grade 3	67.7	66.6	-1.160	0.700	2236
Grade 4	50.5	49.1	-1.352	0.596	2236
Grade 5	38.4	36.7	-1.743	0.420	2236
Grade 6	27.8	25.7	-2.105	0.220	2236
New intakes	72.3	71.1	-1.138	0.672	2236
Girls, grade 1	59.5	58.4	-1.117	0.601	2236
Girls, grade 2	39.4	39.3	-0.163	0.918	2236
Girls, grade 3	30.9	30.3	-0.674	0.638	2236
Girls, grade 4	22.8	22.1	-0.790	0.510	2236
Girls, grade 5	16.9	15.8	-1.095	0.280	2236
Girls, grade 6	11.6	10.1	-1.430	0.064	2236
Total	397.7	388.6	-9.144	0.567	2236
Girls, total	181.4	176.2	-5.270	0.476	2236
Girls /Boys	0.458	0.458	0	0.873	2236
Repetition rate					
Grade 1	0.40	0.39	-0.011	0.262	2236
Grade 2	0.25	0.22	-0.022	0.004	2083
Grade 3	0.19	0.16	-0.013	0.093	1834
Grade 4	0.12	0.09	-0.021	0.002	1620
Grade 5	0.07	0.06	-0.009	0.154	1485
Grade 6	0.04	0.03	-0.010	0.059	1344
Total	0.23	0.22	-0.017	0.007	2236
School characterstics					
Frac. disadvantaged	0.11	0.08	-0.032	0.041	2236
Frac. w parents assoiation	0.67	0.74	0.065	0.005	2389
Income p /c	26149	50780	25857	0.245	2389
Teachers /100 stud	2.23	2.13	-0.106	0.021	2236
Av. class size	54.6	55.5	1.17	0.326	2402
		VILLAGE LEVEL			
Frac. w primary edu.	0.49	0.48	-0.007	0.892	102
Education level, 15-24	4.7	4.0	-0.770	0.113	102
Literacy rate	0.67	0.57	-0.1	0.164	102

Source: DHS and EMIS.

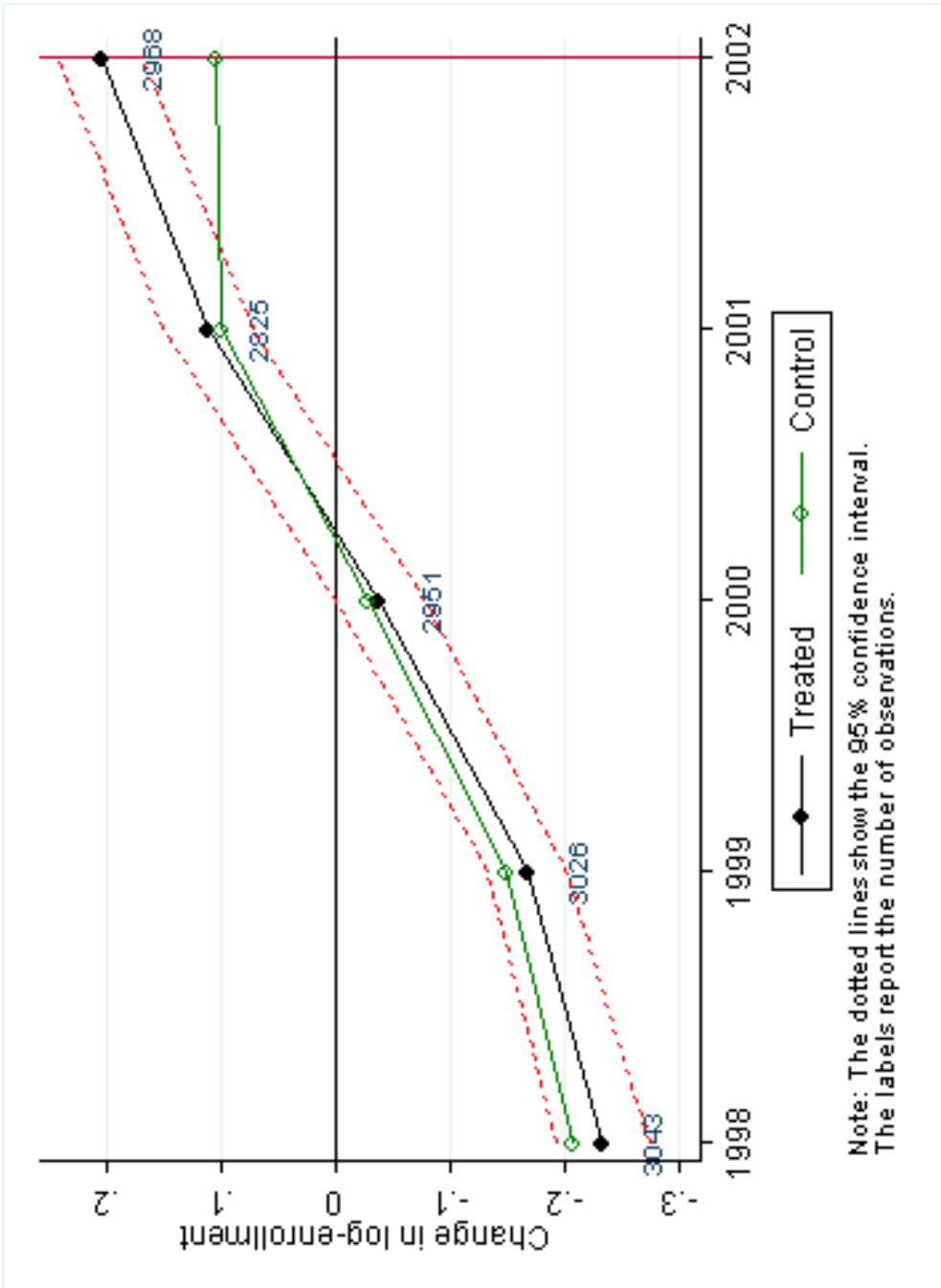


Figure 1: Enrollment trends before the treatment in treated and control schools

To further alleviate the concerns about selection, we look at enrollment trends before and after the treatment, in figure 1. The figure shows the enrollment trends plotted over time, pooling together all treatment and control groups. For each year after 1998, the schools treated in that year are dropped from the plot, so that all schools are observed exclusively before receiving the treatment, except for 2002. The series, purged of school fixed effects, are clearly parallel, and only diverge in the year of treatment.

Enrollment rates alone, as mentioned above, might not give a clear picture of the success of a policy. First of all, increased enrollment not matched by increased resources, like teachers orF classrooms, , might even lead to negative outcomes when it comes to school quality and learning. Moreover, the short-term availability of food in school might simply result in likewise short-term enrollment and a high turnover in pupils, rather than an actual increase in their total educational achievement. This point can be addressed by studying the household data. Figure 2 reports the average highest completed grade for each of the birth cohorts that were of primary school age between 1999 and 2002 in treated and non-treated communes.[21] These children, aged 8 to 15 in 2004 when the survey was conducted, are compared to children aged 8 to 15 in 1999, at the time of the previous survey. The upper graph shows that the highest grade achieved in general increased between the two survey waves. However, while the highest grade is always lower in treated communes as compared to non-treated communes in 1999, in 2004 this pattern is often reversed. In other words, educational achievement increased comparatively more in treated communes. The lower graph shows how the distribution of highest completed grade has changed between the two points in time, revealing a drastic reduction in the number of zeroes. In other words, the proportion of children that do not have any education at all went down and, once more, this effect is stronger in the treated communes.

[21]We here take an intention to treat approach. The oldest children that potentially received the treatment were 12 (and officially enrolled in 6th grade if they had started school at the official entry age of 6) in 1999 and the youngest were 6 (1st graders) in 2002.

These patterns are very similar when we investigate the subsample of girls (results not shown).

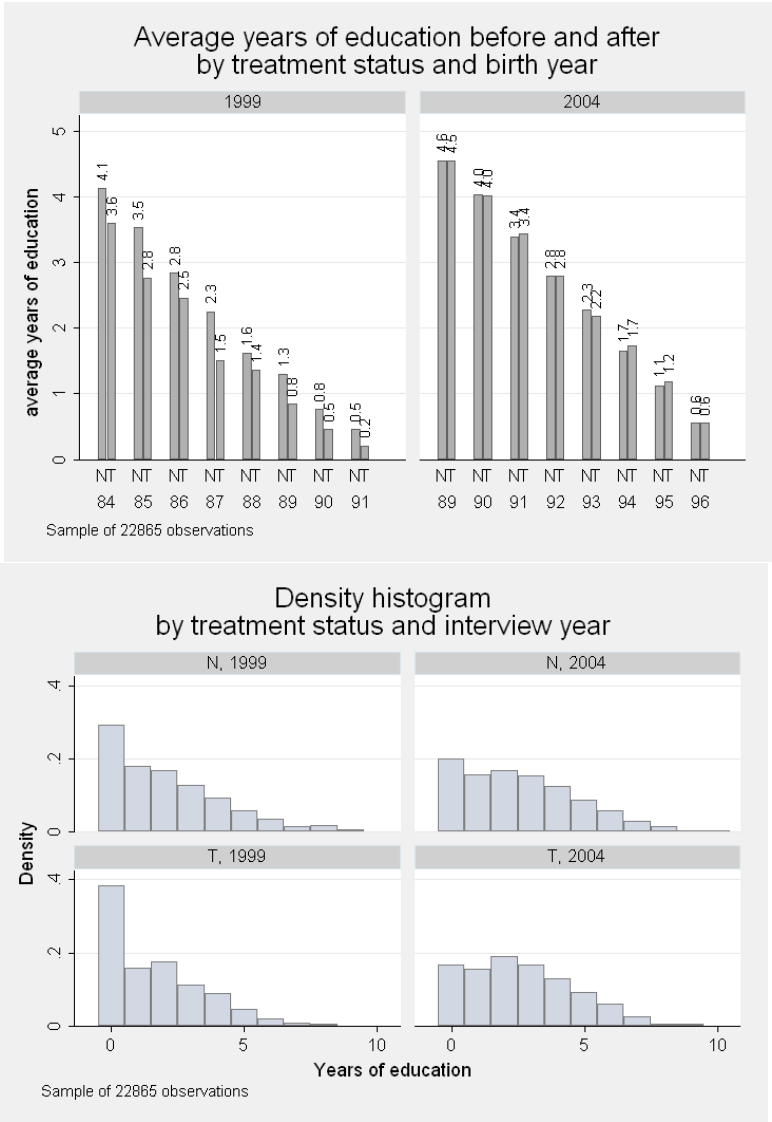


Figure 2: Distribution of highest grade completed.

4.3 Specifications

The identification of the effect is based on a difference-in-difference strategy which allows us to control for time invariant unobservables that are correlated with program placement and participation. For the school data, we use a fixed effect specification at the school level, looking separately at each treatment group. Given the panel structure, we can analyze the effect on enrollment for each year of treatment $g = (2000, 2001, 2002)$ using the following specification:

$$Enr_{itg} = \alpha + \beta * Aft_g + \gamma * Tr_g * Aft_g + \sum_{k=1999}^{g} \gamma_k Tr_g * Aft_g * TrGr_k + \mu_i + \sigma_{itg} \quad (1)$$

where subscript i denotes the school and t the year in which enrollment is observed. The dependent variable is the natural logarithm of total enrollment, in order to smooth a dependent variable that can otherwise take some rather extreme values. In a given year, among all treated schools (Tr), there will be two to four sets of schools that differ in terms of when they started receiving the treatment, i.e. which treatment group they belong to ($TrGr99$, $TrGr00$, $TrGr01$ or $TrGr02$[22]). As we want to observe the effect of treatment over time, we allow the estimate to have a separate intercept and slope for treated schools that differ in their length of treatment ($Tr * Aft * TrGr$). $g - k + 1$ hence indicates the number of years of treatment. Besides total enrollment (in logs, to take into account school size), we also look at enrollment by gender and grade.[23]A simple difference-in-difference specification, with treatment group dummies instead of school fixed effects, is also reported in table 4.

[22]To be more precise, the $TrGr99$ is defined as a group of treatment units (either schools or communes) that received treatment for the first time during the school year 1999/2000, and so on.

[23]Estimations by grade are not shown. The main patterns are summarized in the result section. Tables can be received from the authors upon request.

For the individual data, the ideal would be to use commune fixed effects to account for unobservable characteristics at the commune level, which is the closest to the treatment assignment level. But since most of the communes only appear in one of the surveys, making a within commune comparison over time impossible, we use a fixed effect specification at the district level. Most of the districts are, in fact, represented in both surveys. The following is estimated:

$$Educ_{idt} = a + b * Aft + c * Aft * Tr + m_d + e_{idt} \quad (2)$$

where i, d and t index individual respondent, district and year, respectively. Since the sample we are using contains children of different ages, all specifications include age dummies to account for any age-related differences in education. Moreover, given that the CSES 2004 survey was running over two school years (November 2003 to January 2005), we will observe children of the same age but born in different cohorts, according to when exactly they were interviewed. Therefore, we include birth year dummies taking the value of one for children born in a given year and observed in the CSES 2004. The outcome variables here are the highest grade achieved and the probability of being in school, in 2004 versus 1999. We further use the same specification with additional interaction terms for the per capita income quintile (proxied by per capita consumption), gender and parents' educational level. A simple difference-in-difference specification with treatment group dummies instead of the district fixed effects is also reported.

Selection bias

As mentioned earlier, according to the selection rule during the pilot phase, communes with the highest education needs were prioritized for the intervention. After the pilot phase, it was decided that schools with formal submission and commitment to prepare cooking and storage were more likely to be given the intervention. We test whether the rule was actually followed

by running a simple regression at the school level. The dependent variable is the treatment status indicator and a set of selection variables are tested as determinants: a dummy for whether the school is defined as disadvantaged, a dummy for having a parents' association, total primary enrollment, the poverty rate in the commune of the school, the repetition rate at the primary level. The regression is run for both 1997 and 1998, i.e. before the intervention. We find that (results not shown) the only factor significantly determining the treatment status before the intervention is the repetition rate: a negative coefficient implies that schools with lower repetition rates were prioritized for receiving the treatment. Hence, the rule of prioritizing the most vulnerable schools was not followed. But the fact that treatment was given to better performing schools in terms of repetition rates, and the self-selection connected to the formal submission and commitment to prepare the food might cause biases in our estimates: a selection bias might imply that we overestimate the effect of treatment, while mean reversion might lead to an underestimation. To deal with a potential mean reversion problem, we use an additional specification, where we interact the average repetition rate for 1997 and 1998 with the after-treatment indicator variable. The results are very similar suggesting that the bias is relatively small.

5 Results

We start with a placebo-like test: table 3 presents the effect of the treatment *before* the treatment, in other words, the change in enrollment between 1997 and 1998, comparing the various treatment groups to the respective control group. For this purpose, we use the additional EMIS panel for which wealso have data from 1997. We already ruled out that schools receiving treatment were ex-ante different in the levels of enrollment. If they had been ex-ante different in terms of their *rate* of increase in enrollment, then we would expect some positive coefficients in these placebo regressions. But we see that the

placebo treatment has no effect on any of the treatment groups, indicating that the parallel trend assumption holds for our identification.

Table 3: Placebo test - effect on enrollment between 1997 and 1998

	(1)	(2)	(3)	(4)	(5)
Treatment group	1999	2000	2001	2002	All
TreatXAfter	0.0241	0.00937	-0.00234	-0.00127	-0.00127
	(0.0185)	(0.0122)	(0.0108)	(0.0100)	(0.0100)
R^2	0.350	0.005	0.006	0.002	0.009
Schools	2236	2236	293	1212	1871
Observations	4443	4451	578	2409	3725

Note: The dependent variable is the natural logaritm of enrollment. The coefficients compare enrollment in 1998 with 1997. The control groups include all non-treated schools within the same provinces for each treatment group. Standard errors clustered at the school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.1 Effects on enrollment

Table 4 presents the results in a simple difference-in-difference setting for each treatment year, including only schools receiving treatment for the first time in that year.[24] The table shows means of (log) enrollment by year (comparing enrollment in the year of treatment with enrollment in the year before) and treatment status. The control groups, one for each treatment group, include all never-treated schools within the same provinces. We see significant increases in enrollment with respect to the control group and the relative increases are quite similar across treatment groups, with the exception of the 2002 "full package" group. These group-level difference-in-difference estimations are, however, very noisy, because schools can be very different in terms of size, location, income, infrastructures and many other

[24]If we instead look (results not shown) at all schools treated each year without considering that schools belong to different treatment groups, we do no see any effect of treatment, except in 1999. This happens because schools with different treatment lengths are pooled together, while the effect is not constant over the length of treatment, the enrollment increase being smaller for the schools in their second, third or fourth year of treatment.

fixed characteristics.

Given the panel structure of the data, we are able to observe the same schools after each year of treatment. Table 5 reveals that the effect on enrollment is, in fact, always positive and significant in the first year of treatment, even controlling for the school fixed effects, and decreases slightly over time. From column (3), we learn that the increase in enrollment due to the FFE program after the first year of treatment is 5.8% with only on-site feeding (treatment group 1999 and 2000), 5.2% when the take-home rations were also provided (treatment group 2001), and almost 19% with the full-package including deworming (treatment group 2002).

The impact on enrollment for each single treatment group can be followed over time by summing the coefficients corresponding to the interaction terms in equation 1. For example, in the 2000 treatment group, enrollment increases by 5.82% during the first year, then by 5.22 - 8.34 = -3.12% in the second year, and finally by 18.8 - 11.3 = 7.5% in the third year. Only in this particular case, the 2000 treatment group observed in 2001/02 (its second year of treatment), do we observe a negative effect (although it cannot be distinguished from zero), which means that enrollment in treatment schools increases less than in control schools. However, also in the other groups, enrollment increases clearly slow down in the following years as compared to the first year of treatment. One possible interpretation of the fading out of the effect is that all eligible children that were still out of school and are sensitive to the program (i.e., they live in households for which the program is sufficient to shift the balance of costs and benefits of school towards the benefit side), are already attracted to school during the first year of treatment. Another possibility is that the schools reach full capacity after the increase in the first year, and cannot enroll more children during the following years. In fact, the average class size in the treated schools in our data is 55 in 1999 and 70 in 2003. Similarly, there is one teacher for 57 pupils on average in 1999 and one for 62 in 2002. Yet another interpretation could be that the quality

of learning goes down as an effect of the increase in enrollment immediately after the introduction of free meals, which might crowd out some students over the following years. Finally, we must acknowledge the strong general increasing trend in enrollment, clearly visible in Figure 1, which seems to be present even in control schools. It might well be possible that the presence of the school meal program only has an effect in anticipating this growth in enrollment in the treated schools, but the control schools follow suit anyway.

The analysis by grades and gender, not reported, shows that the bulk of the effect comes from grades 4-6, and from girls. The enrollment increases are particularly large for girls, in the absolute sense and as compared to boys, in 2001 and 2002, which we interpret as a potential effect of the take-home rations.[25] However, there are positive effects also for boys in these years, which might suggest that the rule of exclusively targeting poor girls with take-home rations was not strictly followed.

[25]Starting as a pilot in 2001 and expanding in 2002, families of girls in grades 4 to 6 were provided with take-home rations, as girls in these grades are most vulnerable to dropout.

Table 4: Simple difference-in-differences after 1 year of treatment, school level data

	Non-treated	Treated	Difference
	Treatment group 1999		
Before	5.930	6.140	0.210
	(0.059)	(0.072)	(0.093)
After	5.982	6.24	0.258
	(0.048)	(0.065)	(0.079)
Diff-in-diff			**0.041***
			(0.019)
n			345
	Treatment group 2000		
Before	5.692	5.937	0.244
	(0.028)	(0.061)	(0.068)
After	5.833	6.121	0.288
	(0.028)	(0.051)	(0.058)
Diff-in-diff			**0.043***
			(0.024)
n			1239
	Treatment group 2001		
Before	5.732	5.780	0.048
	(0.022)	(0.056)	(0.060)
After	6.024	6.111	0.094
	(0.019)	(0.047)	(0.051)
Diff-in-diff			**0.046***
			(0.025)
n			1815
	Treatment group 2002		
Before	5.624	5.281	-0.343
	(0.022)	(0.080)	(0.083)
After	5.939	5.764	-0-.175
	(0.019)	(0.066)	(0.068)
Diff-in-diff			**0.167****
			(0.036)
n			2014

Note: The dependent variable is the natural logaritm of enrollment. The control groups include all non-treated schools within the same provinces for each treatment group. Robust standard errors clustered at the school level in parentheses. Statistic significance is displayed only for the difference-in-difference term: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effect on enrollment, by type and length of treatment

	Diff-in-diff	Fixed effects	Fixed effects
	All treated schools observed in 2003/04		
TreatXAfter	0.146***	0.036*	
	(0.024)	(0.019)	
	Treated schools observed in 2000/01		
TreatXAfter	0.0311	0.0511***	0.0582**
	(0.0201)	(0.0182)	(0.0236)
Treat00XAfterXTrGroup99			-0.0223
			(0.0265)
R^2	0.023	0.164	0.164
Schools	1302	1302	1302
Observations	2555	2555	2555
	Treated schools observed in 2001/02		
TreatXAfter	-0.000808	0.00728	0.0522**
	(0.0179)	(0.0172)	(0.0249)
TreatXAfterXTrGroup00			-0.0834**
			(0.0333)
TreatXAfterXTrGroup99			-0.0212
			(0.0317)
R^2	0.039	0.425	0.427
Schools	2010	2010	2010
Observations	3957	3957	3957
	Treated schools observed in 2002/03		
TreatXAfter	0.0303	0.0544***	0.188***
	(0.0191)	(0.0182)	(0.0359)
TreatXAfterXTrGroup01			-0.131***
			(0.0432)
TreatXAfterXTrGroup00			-0.113***
			(0.0351)
TreatXAfterXTrGroup99			-0.00339
			(0.0348)
R^2	0.038	0.397	0.406
Schools	2402	2402	2402
Observations	4715	4715	4715

Note: The dependent variable is the natural logaritm of enrollment. The control groups include all non-treated schools within the same provinces for each treatment group. Columns (2) and (3) include school fixed effects. Column (3) allows for a separate intercept and slope for the schools depending on which treatment group they belong to. Robust standard errors clustered at the school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Effect on highest grade and probability of being in school

We follow a similar approach for the household data and start by reporting, in table 6, the simple difference-in-difference for the highest grade achieved and the probability of being in school for all children who, based on their birth year, were supposed to be in school during at least one treatment year. Children interviewed in 2004 in one of the treated communes are compared to children in non-treated communes, and then with the corresponding cohorts of children interviewed in 1999, before the treatment started. Since there are no data prior to 1999, communes treated in 1999 are excluded from the sample. The first treatment year in this part of the analysis is hence 2000.

Table 6: Simple difference-in-difference, individual level data

	Non-treated	Treated	Difference
	Highest grade completed in year 2004/2005		
Before	2.1	1.6	-0.44
	(0.057)	(0.118)	(0.13)
After	2.6	2.65	0.051
	(0.045)	(0.099)	(0.108)
Diff-in-diff			**0.491*** **
			(0.152)
	Probability of being in school in year 2004/2005		
Before	0.76	0.69	-0.069
	(0.011)	(0.032)	(0.033)
After	0.85	0.89	0.036
	(0.006)	(0.012)	(0.013)
Diff-in-diff			**0.106****
			(0.034)

Note: The dependent variable is the highest grade completed in the first panel and the probability of being in school in year 2004/2005 in the second panel. The control group includes all children in the same age cohorts interviewed in non-treated communes. Robust standard errors clustered at the commune level in parentheses. Statistic significance is displayed only for the difference-in-difference term: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 7: Average treatment effect, individual level data

	(1)	(2)	(3)	(4)
	Highest grade		Probability	
	OLS	FE	OLS	FE
Treat	-0.487***	-0.130	-0.0693**	-0.0686*
	(0.121)	(0.154)	(0.0341)	(0.0373)
After	0.516***	0.319***	0.0973***	0.182***
	(0.0652)	(0.0494)	(0.0142)	(0.0190)
TreatXAfter	0.510***	0.271*	0.106***	0.0995***
	(0.144)	(0.141)	(0.0339)	(0.0334)
R^2	0.383	0.493	0.056	0.148
Districts	168	168	168	168
Communes	852	852	852	852
Observations	22499	22499	22497	22497

Note: The dependent variable is the highest grade completed in columns 1-2 and the probability of being in school in year 2004/2005 in columns 3-4. The control group includes all children in the same age cohorts interviewed in non-treated districts. All the regressions include age and birth year fixed effects. Column (2) and (4) include districts fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7 shows the OLS estimations including both age and birth year dummies (columns (1) and (3)). In order to reduce the noise in the data, district fixed effects are added (columns (2) and (4)) which makes the treatment estimates smaller in size. The district fixed effect estimates imply an almost two-month longer education (0.27 years more) relative to the before-treatment mean of 1.8.[26] The same specification is used for the probability of being in school.

The fixed effect estimates show that this probability increases by about 10 percentage points more for the children in treated communes as compared to children in non-treated communes which, relative to the mean in 2004 (69%),

[26]This figure is so low because it is an average for all children aged 7-15, including those with zero education. The mean excludes the zeroes, i.e. the mean education achieved for those that have been to school at some point is 2.8.

implies a 14% increase due to the program. These effects are averages of all treated communes in a given year and do not take into account the length of treatment.

In table 8, we want to investigate whether the program effect differs with the length of treatment. Starting with the highest grade achieved, only children treated in 2000 have significantly higher achievements as compared to the control children when we observe them in the 2004 survey. No effect is found for the treatment groups 2001 and 2002 (communes receiving treatment for two years and one year, respectively, when observed in the 2004 survey). Columns (4) to (6) reveal, though, that these children are significantly more likely to be in school in 2004 than the control children. One intuitive explanation is that the duration of the program is important: children that receive food in school for three years (started in 2000/01), stay in school longer than they otherwise would (however, not until 2004, since their probability in column (4) is not significantly higher). For those that receive food for shorter durations, the treatment does not make any difference. In the same spirit, we do not observe any increase in the number of those who complete the full primary school, because three years are probably not sufficient to make a difference for this decision.[27] An alternative interpretation is that many of these children who are more likely to be in school in 2004 are repeaters: in their case, the potential additional years of school attendance would not show up in the completed grade. We also look at the effect by birth year but do not find any particular pattern in this respect.

Tables 9 and 10 present the heterogeneous effects between groups, regardless of which treatment group they belong to. Girls and children of fathers with low education, groups that we would expect to be disadvantaged in terms of schooling, have completed a higher grade in 2004, while children of parents with lower education are more likely to be in school in 2004.

[27]These results are not shown. Notice that the FFE went on until 2008. Hence, it is possible that such longer term effects will be visible in later data.

Table 8: Effect by treatment group 2000, 2001 and 2002

	(1)	(2)	(3)	(4)	(5)	(6)
	Highest grade			Probability of being in school		
Treatment group	2000	2001	2002	2000	2001	2002
Treat	-0.380	0.260	-0.231	-0.0149	-0.0296	-0.293**
	(0.252)	(0.218)	(0.400)	(0.0448)	(0.0478)	(0.145)
After	0.663***	0.880***	0.292***	0.116***	0.119***	0.168***
	(0.0605)	(0.0547)	(0.0452)	(0.0158)	(0.0430)	(0.0194)
TreatXAfter	0.450*	0.162	0.230	0.0445	0.0739*	0.342**
	(0.263)	(0.189)	(0.391)	(0.0444)	(0.0430)	(0.136)
R^2	0.396	0.427	0.445	0.129	0.134	0.173
Districts	165	167	167	165	167	167
Communes	801	806	789	801	806	789
Observations	16350	16188	15537	16348	16187	15536

Note: The dependent variable is the highest grade completed in columns 1-3 and the probability for being in school in year 2004/2005 in columns 4-6. The control group includes all the children in the same age cohorts interviewed in non-treated districts. All the regressions include district, age and birth year fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Table 11 we look at the last year of treatment to further investigate the effect of take-home rations. If we separate between girls and boys in that year, we find that the effect in terms of a higher probability of being in school is indeed stronger and larger for girls. Once more, this does not add to the total duration of their education.[28] This might once more suggest that the rule of targeting girls was not strictly followed.

Although parents'educational level can be considered to be a proxy for income, Table 12 looks in more detail at the effects of the program along the income distribution.[29] The probability of being in school in 2004 is highest for the poorest quintile [30] and is decreasing with higher income, which indicates that the program is indeed a pro-poor intervention. The effect on highest grade in column (1) instead has an inverted-U shape, displaying larger effects for the middle quintiles. The very poorest and the richest households in the sample increase their total education less or not at all. One interpretation is that in these households, the cost-benefit balance of completing the full year is not affected by school meals. Rich children, generally having better educational prospects, complete their education anyway with or without free food. For the poorest households, it could be that they i) are only in school during the free breakfast but not attending the classes (maybe need to help with family business), ii) have incomplete school records, thus making it impossible to enroll and officially complete the full year, or iii) they are more sensitive to countervailing effects from crowded classrooms.

To investigate whether the program attracted older cohorts of children, we look at birth cohorts that, according to official age limits, should have been too old to be in school during the treatment years. Table 13 presents the effect of the treatment in terms of both highest grade and the probability

[28] Although not significant, the estimates are larger for girls than for boys even in this case.

[29] The per capita income is here proxied by per capita consumption.

[30] The quintiles are computed with respect to the general population. The analysis using quintiles computed within the sample is not reported but shows almost identical estimates.

Table 9: Heterogeneous effects on highest grade achieved

	(1)	(2)	(3)	(4)	(5)	(6)
	Girls	Boys	Mother's education		Father's education	
			Low	High	Low	High
Treat	-0.0943	-0.136	-0.0224	-0.485*	-0.124	0.140
	(0.169)	(0.168)	(0.169)	(0.249)	(0.154)	(0.238)
After	0.369***	0.881***	0.287***	0.378***	0.765***	0.833***
	(0.0609)	(0.0748)	(0.0523)	(0.0891)	(0.0601)	(0.105)
TreatXAfter	0.277*	0.240	0.260	0.333	0.355**	-0.0206
	(0.160)	(0.151)	(0.159)	(0.246)	(0.139)	(0.241)
R^2	0.504	0.492	0.489	0.517	0.465	0.584
Districts	168	168	168	164	168	154
Communes	850	849	850	799	846	743
Observations	10919	11580	16017	6482	15790	6709

Note: The dependent variable is the years of education in year 2004/2005. The treatment group consists of all children interviewed in any of the treated communes regardless of when they were treated or what type of treatment. The control group includes all children in the same age cohorts interviewed in non-treated districts. All regressions include district, age and birth year fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Heterogeneous effects on the probability of being in school

	(1)	(2)	(3)	(4)	(5)	(6)
	Girls	Boys	Mother's education		Father's education	
			Low	High	Low	High
Treat	-0.0707*	-0.0688	-0.0628	-0.0739	-0.0423	-0.0647
	(0.0415)	(0.0458)	(0.0414)	(0.0594)	(0.0413)	(0.0463)
After	0.182***	0.107***	0.190***	0.173***	0.148***	0.0406***
	(0.0256)	(0.0146)	(0.0221)	(0.0291)	(0.0161)	(0.0144)
TreatXAfter	0.101**	0.0992**	0.110***	0.0522	0.0976***	0.0578
	(0.0394)	(0.0386)	(0.0358)	(0.0602)	(0.0372)	(0.0445)
R^2	0.161	0.154	0.157	0.162	0.157	0.126
Districts	168	168	168	164	168	154
Communes	850	849	850	799	846	743
Observations	10919	11578	16015	6482	15788	6709

Note: The dependent variable is the probability of being in school in year 2004/2005. The treatment group consists of all children interviewed in any of the treated communes regardless of when they were treated or what type of treatment. The control group includes all children in the same age cohorts interviewed in non-treated districts. All regressions include district, age and birth year fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Effect by birth year and gender for treatment group 2002

	(1)	(2)	(3)	(4)
	Highest grade		Probability	
	Girls	Boys	Girls	Boys
TreatXAfterXByr1991	0.436	0.0279	0.452***	0.293**
	(0.492)	(0.513)	(0.171)	(0.149)
TreatXAfterXByr1992	-0.0633	-0.127	0.371**	0.314*
	(0.581)	(0.457)	(0.160)	(0.172)
TreatXAfterXByr1993	-0.155	-0.140	0.369*	0.215
	(0.694)	(0.522)	(0.189)	(0.170)
TreatXAfterXByr1994	0.515	0.104	0.413***	0.307*
	(0.551)	(0.535)	(0.148)	(0.185)
TreatXAfterXByr1995	0.629	0.0638	0.327*	0.187
	(0.597)	(0.407)	(0.191)	(0.174)
TreatXAfterXByr1996	0.753	0.0971	0.491***	0.140
	(0.500)	(0.459)	(0.176)	(0.228)
R^2	0.459	0.439	0.181	0.181
Districts	168	168	168	168
Communes	847	846	847	846
Observations	8099	8696	8099	8695

Note: The dependent variable is the highest grade completed in columns (1) and (2) and the probability of being in school in 2004 in columns (3) and (4). All regressions include district, age and birth year fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Effects by income quintiles

	(1) Highest grade	(2) Probability of being in school
Treat	-0.153	-0.0744**
	(0.158)	(0.0375)
After	0.217***	0.169***
	(0.0495)	(0.0188)
TreatXAfterXQ1	0.104	0.134***
	(0.163)	(0.0366)
TreatXAfterXQ2	0.359**	0.112***
	(0.169)	(0.0373)
TreatXAfterXQ3	0.440***	0.0876**
	(0.165)	(0.0396)
TreatXAfterXQ4	0.240	0.0687*
	(0.210)	(0.0370)
TreatXAfterXQ5	0.0722	0.0734*
	(0.213)	(0.0377)
R^2	0.519	0.161
Districts	168	168
Communes	852	852
Observations	22499	22497

Note: The dependent variable is the highest grade completed in column 1 and the probability of being in school in year 2004/2005 in column 2. The control group includes all children in the same age cohorts interviewed in non-treated districts. All regressions include district, age and birth year fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

of being in school for three cohorts of older children, aged between 12-14, 12-17 and 18-20. None of these cohorts are more likely to be in school in 2004 as a consequence of the treatment; on the other hand, for the younger ones, a strong positive effect can be observed in terms of highest grade. Although not (differentially more) enrolled in 2004, when they are 16 and older, these children went to school longer than children of the same age in the non-treated communes, which implies that they have been enrolled in primary school during the program although they were then already aged above 12.

Table 13: Effect on older cohorts

	(1)	(2)	(3)	(4)	(5)	(6)
	Highest grade			Probability of being in school		
	Aged 12-14	Aged 12-17	Aged 18-20	Aged 12-14	Aged 12-17	Aged 18-20
Treat	-0.824**	-0.967**	0.0480	-0.00778	0.00932	-0.00464
	(0.325)	(0.387)	(0.508)	(0.0539)	(0.0448)	(0.0191)
After	0.767***	0.444***	0.599***	0.103***	0.00984	0.00842
	(0.113)	(0.160)	(0.201)	(0.0188)	(0.0167)	(0.0124)
TreatXAfter	0.894***	0.535	0.274	0.00335	0.00610	0.0168
	(0.303)	(0.378)	(0.467)	(0.0523)	(0.0449)	(0.0215)
R^2	0.245	0.251	0.252	0.145	0.117	0.074
Districts	168	168	166	168	168	166
Communes	843	842	813	843	842	813
Observations	7812	6609	5606	7811	6603	5604

Note: The dependent variable is the highest grade completed in columns 1-3 and the probability of being in school in year 2004/2005 in columns 4-6. The control group includes all children in the same age cohorts interviewed in non-treated districts. All regressions include district, age and birth year fixed effects. Robust standard errors clustered at the commune level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6 Cost-benefit analysis

A central policy question is whether FFE programs yield a higher impact per dollar spent than alternative programs. There are, however, very few studies on the cost per outcome for school feeding programs. The average cost of running FFE programs at WFP in 2001 was 19 cents per child per day, including everything from the values of all food commodities provided by WFP to the cost of transportation and monitoring, to internationally- and locally-recruited staff.[31] In 2005, the average cost was 7.9 cents per child per day, or 15.79 USD per child per year.[32] In general, the take-home rations are more expensive, around 30 USD per child per year, due to transport costs and differences in food bundles.[33] The per child cost of deworming in 2005 was around 22 cent per treatment against both Soil-Transmitted Helminth (STH, one tablet of Mebendazole 500 mg costs approximately 2 cent) and Schistosomiasis (one tablet of Praziquatel 600 mg costs 20 cents).[34]

Using the food allocation from the 2001/2002 school year multiplied by the 2004 average cost per metric tons for each food item, we are able to calculate a rough measure of the total value of the Cambodia FFE program for that year.[35] Table 14 shows that the average cost for on-site breakfast is around 8 USD per child per year; take-home rations cost 37 USD per girl, so the average cost for both is 10 USD per child.[36] Assuming that it is sufficient with one tablet of Praziquatel 600 mg and one tablet of Mebendazole 500 mg to treat a child per year, the total cost for a full package intervention

[31] Source: WFP Brief "WFP Global School Feeding Campaign - Into School, Out of Hunger", 2001.

[32] We are using the world average of 200 school days per year for cross-country comparisons.

[33] See Adelman et al. (2008).

[34] See "Global School Feeding Report 2006", WFP.

[35] See WFP, 2004, "Protracted Relief and recovery operation Cambodia 10305.0", Jan 2004, for food costs and FASONLINE WFP for detailed resource allocation in 2001/2002.

[36] Bear in mind that these costs are only food costs and do not include indirect costs such as transport costs, staff costs, etc.

that includes on-site meals, take-home rations for poor girls and deworming is around 10.36 USD per child per year.

Table 14: Program costs, 2001-2002

Resource allocation 2000/2001		Quantity (mt)	2004 value (USD)	No of Pupils	Cost per pupil (USD)
Rice	on site	3,470	697,478		
	take home	2,038	409,630		
	both	5,508	1,107,108		
Vegetable oil	on site	255	201,958		
	take home	236	186,423		
	both	491	388,381		
Canned fish	on site	663	1,453,296		
	take home	-	-		
	both	663	1,453,296		
Salt	on site	99	7,920		
	take home	-	-		
	both	99	7,920		
Total	on site	4,487	2,360,652	291,593	8.10
	take home	2,274	596,053	16,000	37.25
	both	6,762	2,956,705	291,593	10.14
Deworming (2002)	Mebendazole (against STH)		5,831	291,593	
	Praziquatel (against Schistosomiasis)		58,318		
	both		64,150		0.22
Full package incl. deworming					10.36

Note: The average cost per metric tons of rice is 201 USD, vegetable oil is 791 USD, canned fish is 2192 USD, iodized salt is 80 USD, source: "Protracted Relief and recovery operation - Cambodia 10305.0", January 2004. The quantity of resources is based on the allocation plan for the school year 2001/2002, source: FASONLINE WFP.

Table 15: Cost effectiveness of the different interventions

Treatment	(1) On site	(2) On site + take-home	(3) On site + take-home + deworming	(4) Scholarships to poor girls in 6th grade	(5) Meals to pre-school in Kenya
Year	2000-01	2001-02	2002-03	2004-06	2000-01
Budgeted n. of children	125,000	291,593	317,053	2,765	2,750
Total cost (USD)	1,012,000	2,956,753	3,284,669	124,425	127,875
Actual n. of children in the treatment group before treatment	167,230	227,550	291,930	2,765	2,750
Program impact	5.82%	5.22%	18.8%	30%	8.5%
Additional children in school due to the treatment	9,733	11,878	54,883	830	234
Cost per additional child in school (USD)	104	249	60	150	546

Note: The total cost is computed multiplying the average cost per pupil from Table 14 by the number of children in the remaining years. The budgeted number of children is from the program documentation, and is WFP's own estimate. The actual number of enrolled children is what we observe in the data, and it can differ from the ex-ante estimated number. The program impact is our own estimate, from Table 5 column (4). The figures in columns (4) and (5) are our elaborations from Filmer and Schady (2008) and Vermeersch et al. (2005) respectively.

To assess the cost-effectiveness, in Table 15, we use the program cost with the 2004 food values divided by the number of additional children enrolled due to treatment. The latter is computed using our fixed effect estimates from Table 5. We find that on-site feeding is quite cost-effective, while distributing take-home rations is relatively expensive, as expected. However, adding the deworming intervention is a way of making the full package much more cost-effective, due to the fact that this complete package attracts many more pupils, while the deworming medications are extremely cheap.

In comparison, we also report in the table the cost of a conditional cash transfer program, the Japan Fund for Poverty Reduction (JFPR) scholarship program in Cambodia. This scholarship program, started in 2004, awarded poor girls who were completing sixth grade a scholarship of 45 USD.[37] The program increased the enrollment and attendance of recipients at program schools by about 30 percentage points; hence, the cost per additional child in school was 150 USD. With the exception of the year 2001/2002, the FFE intervention was hence more cost-effective.

The Cambodia FFE was also more cost effective than the Kenya pre-school FFE program studied in Vermeersch et al. (2005), discussed in section 2. The authors do not explicitly report the costs of the program, so the figures in the last column of Table 15 are our own elaborations based on data reported in their paper.

At 60 USD per child in school, the FFE still looks quite expensive as compared to the programs overviewed in Miguel and Kremer (2004), for example. Their estimate of the cost of a deworming intervention is hard to beat, at 3.5 USD per child in school, i.e. if the objective is purely that of attracting more children in school. However, if FFEs also contribute to the nutrition and general health status of children, especially the poor and malnutrient ones, this comparison is not really fair. We did not look at these outcomes in the present paper, but it is certainly a very important area of

[37] See Filmer and Schady (2008).

inquiry for future studies.

7 Conclusions

This study provides an insight into the impact of three types of school feeding programs on enrollment, educational achievements and the probability of being in school. We show that the FFE program boosted school enrollment in the short run for all three types of treatment: on-site feeding, take-home rations and full package including deworming. Enrollment continued to increase at a slower pace after the first year, hinting at potential resource constraints. If the program attracts more children, but the school resources remain fixed, this might lead to a deterioration in the student-teacher ratios and class size which, in turn, might impair learning. Moreover, children who were already attending school may suffer negative peer effects from lower ability children joining school. Beyond enrollment, the intervention also increased the probability of being in school after one or two years. But in most cases, it did not lead to higher educational achievements which might once more suggest a negative countervailing class size effect.

This calls to mind the critique frequently raised against treatment evaluations, namely that partial equilibrium estimates that ignore responses from general equilibrium and political economy sources are to be taken with caution. The argument is clearly spelled out in a recent contribution by Acemoglu (2010) for a case very similar to the instance we are looking at. The authors show that a simple model of the relation between cost of schooling and investments in education, and the relative reduced-form estimations, will not be informative about counterfactuals involving large-scale interventions in the presence of constraints on individual choice. One such constraint can be given for example by school size, which we suspect to be present in this case. In fact, we see the large first-year impacts fade out when the intervention is expanded over time. This consideration, together with the fact that

the intervention is a natural experiment and as such lacks the full strength of randomization, should lead to interpret our estimates with caution.

An alternative explanation, however, is that the time horizon after the implementation of the program is too short to find any effect on the total duration of education. The FFE program also seems to have attracted many overaged children, who boosted the school enrollment figures especially in the fourth to sixth grade (extensive margin), but for some reasons (perhaps due to their incomplete school records) did not remain in school to increase the highest grade they complete (intensive margin). It will be possible to learn more about this outcome by looking at later data, which have recently become available.

Keeping in mind that the impact estimates must be taken with caution, and also that we used approximative figures on costs, we tried anyway to say something about the cost-effectiveness of this program, and make it comparable to other types of interventions. A rough measure of the cost-effectiveness reveals that school feeding alone is a very cost-effective intervention, in a set of comparable programs, but adding deworming medicines, very cheap and extremely effective, makes the full-package scheme even better. Take-home rations instead proved to be a very expensive intervention when put in relation to the average benefit. However, it seems to have reached the intended aim of increasing schooling outcomes for girls.

The impact on nutrition and general health of this program remains to be investigated. Moreover, given the (weak) impact observed on educational outcomes, it is possible that the program also had long-term effects on wages and employment. These are open questions for future research.

Bibliography

Acemoglu, D. (2010). Theory, General Equilibrium, and Political Economy in Development Economics. *The Journal of Economic Perspectives 24*(3),

17–32.

Adelman, S., D. Gilligan, and K. Lehrer (2008). *How effective are food for education programs?: A critical assessment of the evidence from developing countries*. International Food Policy Research Insitute.

Afridi, F. (2010). Child welfare programs and child nutrition: Evidence from a mandated school meal program in India. *Journal of Development Economics 92*(2), 152–165.

Ahmed, A. (2004). Impact of feeding children in school: evidence from Bangladesh. Mimeo, International Food Policy Research Institute, Washington, DC.

Ahmed, A. and C. Del Ninno (2002). The Food For Education Program in Bangladesh: An Evaluation of Its Impact on Educational Attainment and Food Security. FCND. Discussion Paper 138.

Attanasio, O., E. Fitzsimons, A. Gomez, M. I. Gutiérrez, C. Meghir, and A. Mesnard (2006, July). Child education and work choices in the presence of a conditional cash transfer programme in rural colombia. IFS Working Papers W06/13, Institute for Fiscal Studies.

Barrera-Osorio, F., M. Bertrand, L. Linden, and F. Perez-Calle (2008). Conditional Cash Transfers in Education Design Features, Peer and Sibling Effects Evidence from a Randomized Experiment in Colombia. NBER Working Paper No. 13890.

Filmer, D. and N. Schady (2008). Getting girls into school: Evidence from a scholarship program in Cambodia. *Economic Development and Cultural Change 56*(3), 581–617.

Frankenberg, E., K. Beegle, B. Sikoki, and D. Thomas (1999). Health, family planning and well-being in indonesia during an economic crisis: Early re-

sults from the indonesian family life survey. Papers 99-06, RAND - Labor and Population Program.

Gelli, A., U. Meir, and F. Espejo (2007). Does Provision of Food in School Increase Girls' Enrollment? Evidence from Schools in Sub-Saharan Africa. *Food and Nutrition Bulletin 28*(2), 149–55.

Glewwe, P. and P. Olinto (2004). Evaluating the Impact of Conditional Cash Transfers on Schooling: An Experimental Analysis of Honduras' PRAF Program. Technical report, USAID. Washington, DC: International Food Policy.

Jacoby, E., S. Cueto, and E. Pollitt (1996). Benefits of a school breakfast programme among Andean children in Huaraz, Peru. *Food and Nutrition Bullettin, United Nations University 17*, 54–64.

Kazianga, H., D. De Walque, and H. Alderman (2009). Educational and Health Impacts of Two School Feeding Schemes: Evidence from a Randomized Trial in Rural Burkinafaso. Technical report, The World Bank. Policy Research Working Paper Series No. 4976.

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

Pollitt, E. (1995). Does breakfast make a difference in school? *Journal of the American Dietetic Association 95*(10), 1134–1139.

Schady, N. (2008). The Impact of an Economic Contraction on Human Capital: Evidence from LAC and the World. Presentation at the World Bank, Washington, DC.

Schultz, P. et al. (2004). School subsidies for the poor: evaluating the Mexican Progresa poverty program. *Journal of development Economics 74*(1), 199–250.

Sen, A. (2002). Introduction. In *Pratichi education report: an introduction.*

Todd, P. and K. Wolpin (2006). Assessing the impact of a school subsidy program in Mexico: Using a social experiment to validate a dynamic behavioral model of child schooling and fertility. *The American economic review 96*(5), 1384–1417.

Vermeersch, C., M. Kremer, L. Center, and T. Floor (2005). School meals, educational achievement, and school competition: evidence from a randomized evaluation. World Bank Policy Research Working Paper No. 3523.

A Appendix

Chapter 5

Constitutions and the growth-elasticity of poverty*

1 Introduction

1.4 billion people, or one quarter of the developing world, currently live at the margins of survival on less than $1.25 a day.[1] The developing world outside China is not on track to reach the Millennium Development Goal on poverty.[2]

Increasing per capita incomes are generally associated with decreasing poverty rates. Although this does not necessarily make growth the best, or only, tool for poverty reduction, a big research effort has focused on quantifying the responsiveness of poverty to growth, using the concept of growth-elasticity of poverty: the percentage change in poverty associated with a 1 percent growth in per capita income. Many factors can affect how growth translates into poverty reduction. Understanding more about this can help make the effort for poverty reduction more effective.

*I am indebted to Jakob Svensson for inspiration and very valuable comments.

[1]Chen and Ravallion (2008).

[2]China has already achieved the goal of halving the 1990 poverty headcount, several years in advance.

Fundamentally, the way growth translates into poverty reduction has to do with what share of the income produced in the country accrues to different groups in the population, in other words, the country's distributional features. But more generally, since income distribution and inequality are not given, the list of factors that have the potential to affect the growth-elasticity of poverty either directly or indirectly is much longer. The initial conditions that enable different individuals to benefit from growth episodes (education, health); the sectoral composition of growth generation (agriculture, manufacturing or service sector); the protection accorded to different stakeholders in society are but a few examples.

This paper focuses mainly on how the relation between growth and poverty is influenced by the institutional setting of a country, and specifically by the constitutional arrangement. The constitution is the fundamental rule that aggregates voters preferences into political outcomes, such as the relative power of various decision-makers, which in turn affect policy choices. A burgeoning literature on the economic effects of constitutions is building an expanding map from constitutional arrangements into empirically frequent and theoretically justified policy sets. This paper is an attempt to use this map to establish a link between a country's constitution and its growth-elasticity of poverty.

The main result of the empirical analysis conducted here concerns two aspects of the constitution: the electoral rule and the form of government. Poverty appears to be less responsive to growth in countries where a majority of the legislators are elected under plurality rule. On the other hand, poverty appears to be more responsive to growth in countries with a constitutional arrangement that tends to result in a strong executive. This should raise awareness that, in some circumstances more than in others, it is necessary to invoke interventions in support of the poor to complement pro-growth policies.

The remainder of the paper is organized as follows: section 2 introduces

the relationship between growth and poverty. Section 3 presents one possible approach to studying it empirically, and gives a quick overview of the data. Section 4 starts with exploring the variation by region and income group, together with other covariates that have been identified by previous studies. Section 5 constitutes the main novelty of the paper. It starts with a brief overview of the theoretical predictions in terms of the links between constitutions and policies or other political outcomes which are relevant in the present context; discusses how the fight against poverty relates to them; finally reports the empirical results. Section 6 concludes the paper.

2 Growth and poverty reduction

In September 2000, the international community chose to set the "Millennium" goal for poverty reduction in terms of the poverty headcount: the share of people living below (the PPP equivalent of) $1 a day was to be halved by 2015, as compared to the 1990 level.[3] Perhaps the most intuitive approach for assessing different countries' progress and projecting their future prospects in this respect is to relate this measure to growth in GDP per capita. Many empirical exercises have been performed with the goal of evaluating, for example, if continuing with the recently observed rate of growth, a given country would succeed halving the poverty rate, or estimating how fast growth would be necessary given the observed elasticity of poverty to growth.[4]

Abstracting from inequality, rising average incomes should be mechanically associated with a decreasing number of poor people, when the definition of poor is in absolute terms, related to a fixed monetary threshold. It has indeed been observed that economic growth is distributional neutral on av-

[3] 30% of the developing world population fell under this definition of poverty at the time.

[4] See for example Besley and Burgess (2003), Dalgaard and Erickson (2009) and Bigsten and Shimeles (2007).

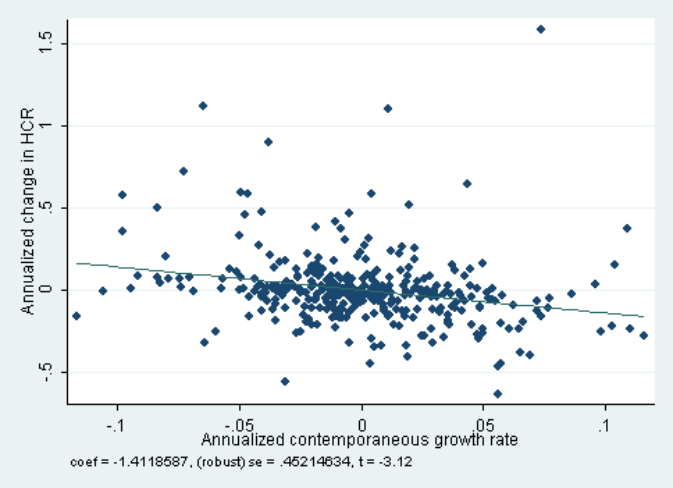


Figure 1: Annualized changes in poverty and growth, whole sample

erage in developing countries, in the sense that, among growing developing economies, inequality rises as often as it falls.[5] We should hence observe a strong negative relation on average between mean income increases and poverty rate reductions in developing countries. Figure 1 plots this relationship for 96 countries in the "Low income", "Lower middle income" and "Upper middle income" groupings of the World Bank in the last 3 decades. A regression line that fits the cloud is also shown. The relation is indeed negative, but not tight: the estimated slope is at -1.41, but with a White standard error of 0.45 around it, the 95% confidence interval implies that the poverty reduction corresponding to a 1% increase in the growth rate could be anywhere between 2.3% and 0.5%.

The heterogeneity around the average relation can be exploited to learn more about the circumstances in which growth is more or less beneficial for poverty reduction.The fact that the effect of growth on poverty is so much larger in some economies than in others calls for a deeper investigation. Data availability and quality has increased since the most recent overview, by

[5]Ferreira and Ravallion (2008).

Ferreira and Ravallion (2008). New household surveys have become available, and moreover the poverty lines for developing countries have been recompiled, taking into account both the new surveys and most importantly the updated purchasing power parity (PPP) estimates following the 2005 round of the International Comparison Program (ICP) price surveys. [6] Finally, we now have data for several more countries, importantly including China and India. Chen and Ravallion (2008) report extensively on the global trends in poverty that emerge from these new data.

3 Empirical specification and data

A general way to investigate the variability in the growth-elasticity of poverty is through regressions of the form

$$\log p_{it} = \eta \log y_{it} + \sum_{j=1}^{J} \beta_j X_j + \sum_{j=1}^{J} \gamma_j X_j * \log y_{it} + \delta_t + \alpha_i + \varepsilon_{it} \qquad (1)$$

where p_{it} is the poverty headcount ratio and y_{it} is income per capita for country i in year t, so that (the absolute value of) η is the average growth-elasticity of poverty. δ_t is a year effect, to take into account the fact that the data are irregularly spaced over time, and the error term includes country-specific effects, α_i. In the set of controls X_j it is possible to allow for several factors to have both a direct effect on poverty and an effect on the elasticity, interacting them with the log income. The variation in these factors is not of a nature that can justify causal inference. By measuring these covariates at the beginning of the period, and by controlling for country and year

[6] Before 2008, the global poverty measures had been anchored to the 1993 round of ICP, that was known to have a number of problems. An independent evaluation (Ryten (1998)) identified a number of methodological concerns with it, including problems with quality standards for international comparability of consumption goods, which have been addressed in the last round.

fixed effects, I can control for the most obvious omitted variable problems. However, the results should be viewed as primarily suggestive. For the constitutional features, I supplement the analysis with an instrumental variables (IV) approach, following the approach by Persson and Tabellini (2003). This set of results is more robust and, if we are willing to believe the exclusion restrictions, can be interpreted as the causal effects of the constitutions.

For the empirical analysis, I use a sample of 96 countries, all for which two or more records on the poverty headcount is available over the period 1980-2008. Table 8 in the appendix reports all the countries and years along with the main characteristics that are relevant for the analysis. There are on average 4.5 observations per country, irregularly spaced over time. The average distance between two consecutive observations is 3.3 years.

Poverty and income measures, along with the geographic classification of countries, are from the World Development Indicators. As a definition of democracy I use the PolityIV index, ranging between -10 (strongly autocratic) and 10 (strongly democratic). I take a threshold of 5 for defining democracy as a binary indicator. This is relatively common in the literature and splits the sample evenly: 51% of the country-year observations in the sample are democratic according to this definition. 29% of the countries are above the threshold for the whole length of the period, while 40.5% never are; the others switch.

The classification of the electoral rules is from DATAVINE/Harvard CID and the World Bank (Beck et al. (2004)). I restrict the definition to countries that are also classified as democracies, i. e. have a polity score above 5. A country is defined to have a majoritarian electoral rule if the plurality rule is used to assign the majority of seats in the lower house.[7] 64% of country-year observations in my sample have a majoritarian electoral rule in this definition, 13 countries changed it during the period.

[7]Some countries have a mixed system, where different shares of the seats are assigned following the plurality, respectively the proportional rule. Different rules may be used for the senate and lower house, when the legislature is composed of two chambers.

Systems with presidents who are elected directly or by an electoral college (not by the legislature), are defined as presidential. 83% of observations have a presidential form of government, 21 changed during the period. 36% of the countries have both these features. From the same source are also variables that identify the presence of term limits in office, the district size, the fractionalization of the government and the legislature and closed list systems. The index of executive constraints is from the Polity database.

4 Beyond averages

4.1 Geographic variation

I start my investigation of the heterogeneity in the growth-elasticity of poverty looking at the geographic variation. Figure 2 shows the total changes observed in the headcount ratio over the period, from the earliest to the latest observation available for each country, and the contemporaneous changes in GDP per capita, organized by regions of the world.[8] There are relatively few arrows pointing up outside of Sub-Saharan Africa (SSA), which means that absolute poverty decreased almost everywhere during the last 3 decades. The picture for SSA looks more mixed. In some regions the arrows look steeper (big gains in poverty with little contribution from growth) and in others flatter (fast growth periods with limited progress in poverty reduction).

Previous literature has found support for systematic differences in the response of poverty to income growth at the geographic level. Table 1 reports the elasticities estimated in Besley and Burgess (2003) (BB). BB estimate separate regressions for each region, and thus cannot include country fixed effects, due to the limited number of observations. East Asia and Pacific region has the largest growth-elasticity of poverty[9], followed by Latin Ameri-

[8]South Africa and Botswana are excluded from SSA for the purpose of this graph because of a difference in scale for income.

[9]Remember that the elasticity is the absolute value of the coefficient, so a more negative

can and the Caribbean, South Asia and finally Sub-Saharan Africa, while the estimates for the remaining two regions are not significantly different from zero. These estimates do not show, however, whether the regional elasticities are significantly different from each other.

Table 1: The growth-elasticity of poverty across regions in a previous study

	Poverty Headcount
Whole sample	-0.73**
	(0.25)
East Asia and Pacific	-1.00**
	(0.14)
Europe and Central Asia	-1.14
	(1.04
Latin America and Caribbean	-0.73**
	(0.29)
Middle East and North Africa	-0.72
	(0.64)
South Asia	-0.59**
	(0.36)
Sub-Saharan Africa	-0.49**
	(0.23)
Countries	88

Source: Besley and Burgess (2003). Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

coefficient indicates a bigger elasticity

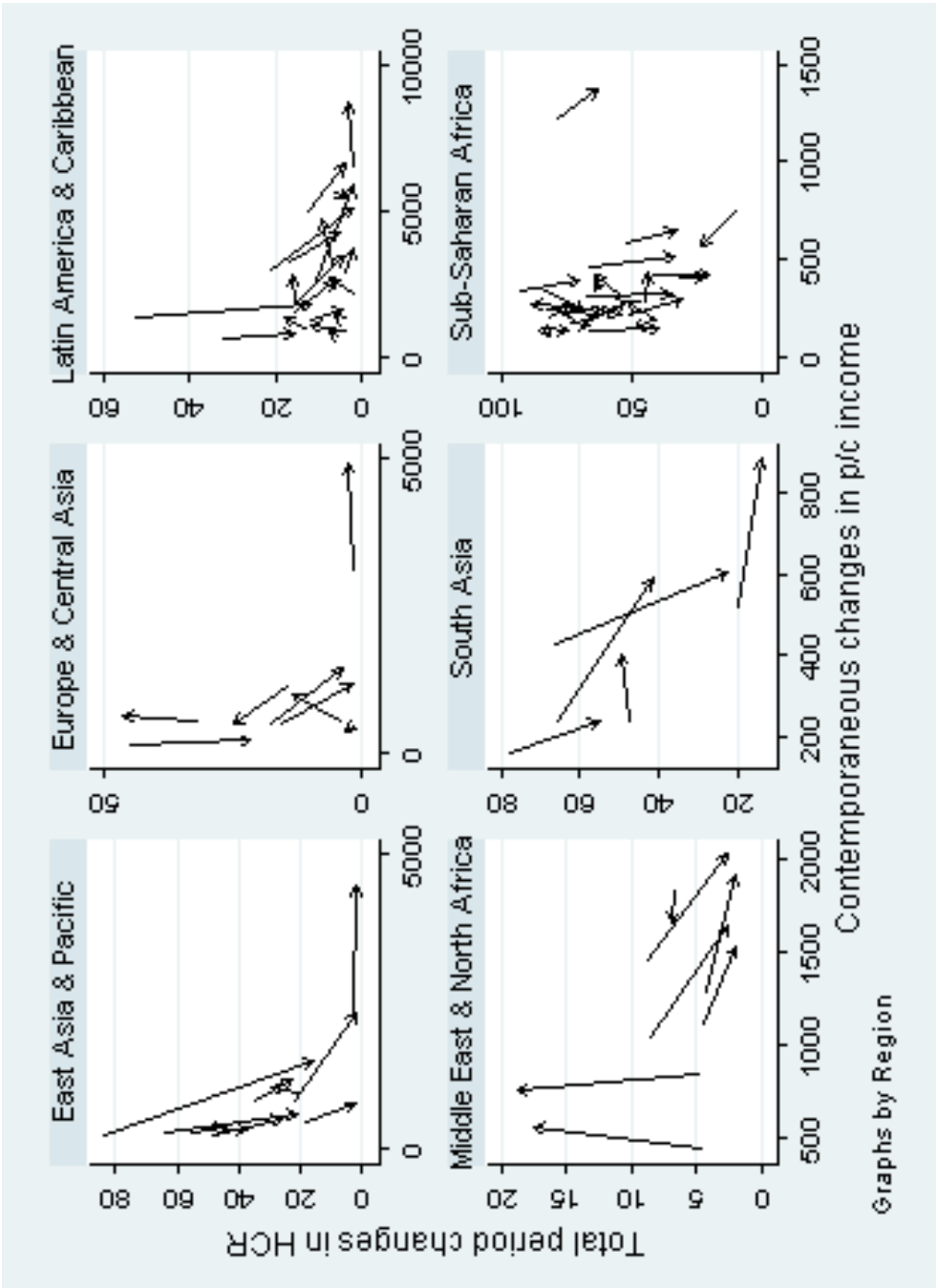


Figure 2: Total changes in poverty and growth, by region

Table 2 reports my regression estimates. As mentioned above, my sample goes up to 2008, and includes many new waves of household surveys. Moreover my data are based on the new $1.25-a-day international poverty line. The specification with interaction terms allows me to use all the observations and estimate the *difference* in the elasticity across regions, controlling for country fixed effects. The average elasticity is around 1, similar to previous estimates, implying that, on average, a 1 percentage point increase in the growth rate is associated with a 1 percent reduction in the poverty headcount. Columns (3) and (4) allow for region-specific intercept and slope, using interaction terms. Without fixed effects, the excluded region, East Asia and Pacific, together with the Middle East and North Africa, would have the highest growth-elasticity of poverty. The worst performer would be, even in this case, Africa south of Sahara, while the remaining three regions would be somewhere in the middle. It is immediately clear though that, when allowing for country-specific heterogeneity, the difference across regions becomes insignificant.

4.2 Income and inequality

Although figure 1 does not seem to suggest non-linearities in the relationship between changes in poverty and changes in income, nevertheless this relationship might be different with respect to income *levels*. In other words, similar changes in income might have a different impact on poverty reduction if income changes from a low level or from a relatively high level. Table 3 estimates separately equation 1 for data intervals with an initial income above and below the sample median of $1355. The elasticity is slightly different, and is larger in poorer countries than in relatively richer countries. However, when all the countries are pooled together and country-specific heterogeneity is taken into account, the difference based on initial income is not significant.

Similarly, we might also wonder if, even when inequality is not rising, a high initial level of inequality can stifle prospects for pro-poor growth.

Table 2: The elasticity of poverty to growth across regions

	(1) OLS	(2) FE	(3) OLS	(4) FE
LogGDP	-0.881***	-0.859***	-1.360***	-1.080***
	(0.0706)	(0.191)	(0.157)	(0.369)
LogGDPXECA			0.506***	0.568
			(0.187)	(0.423)
LogGDPXLAC			0.515**	-0.666
			(0.211)	(0.501)
LogGDPXMENA			0.274	-0.828
			(0.242)	(0.658)
LogGDPXSA			0.519*	0.461
			(0.297)	(0.480)
LogGDPXSSA			0.778***	0.193
			(0.206)	(0.456)
R^2	0.588	0.292	0.795	0.316
Countries	96	96	96	96
Observations	558	558	525	525

Notes: The dependent variable is the log of the poverty headcount. All regressions include time effects. Columns(3) and (4) allow for region-specific intercept and slope. ECA stands for Europe and Central Asia, LAC stands for Latin America and the Caribbeans, MENA stands for the Middle East and North Africa, SA stands for South Asia and SSA stands for Sub-Saharan Africa. Starred coefficients indicate a slope significantly different from the excluded group, East Asia and Pacific. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Ravallion (2001) argues that what counts is not the rate of growth but the distribution-corrected growth rate, defined as $(1 - G_{i,t-\tau}) * \log y_{it}$, where τ indicates the beginning of period. The last column of table 3 shows that the elasticity of poverty with respect to this measure is larger. This implies that the elasticity of poverty to growth declines as the extent of initial inequality rises. Using the estimates in table 3, a country with high inequality (Gini index=60%) is expected to reduce poverty by .56 percent for each percentage point increase in the growth rate. For a country with low inequality (Gini index=20%), this estimate is 1.12 percent.

Table 3: Effect of initial income and inequality

	(1) High income	(2) Low income	(3) Joint	(4) Inequality
Log GDP	-0.986**	-0.907***	-0.990***	
	(0.386)	(0.233)	(0.376)	
LogGDPXLow income			0.0910	
			(0.435)	
Distribution-corrected income growth				-1.686***
				(0.389)
R^2	0.155	0.199	0.218	0.334
Countries	36	68	96	90
Observations	247	278	525	173

Notes: The dependent variable is the log of the poverty headcount. All regressions include time effects. Low or high income is defined with respect to the sample median of $1355. Column (1) and (2) estimate separate regressions, column (3) pools together all the observations with an interaction term. The definition of distribution-corrected growth is from Ravallion (2001). Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.3 Other contextual effects

Previous studies have identified a number of other factors that may help to understand the relationship between changes in growth and poverty, because

they affect the incomes of the poor. Ideally, this should be investigated with help of microdata, in order to directly observe incentives and constraints, behaviors and choices that lead to income generation at the individual level. Bourguignon et al. (2004) and a series of case studies[10] identify, through micro-simulation exercises on survey data, a role for factors like returns to schooling, labor force participation by women, the flexibility of the labor market.

To keep to a global perspective, though, we are confined to macroeconomic factors, due to both limited availability and problems with comparability of microdata across countries. Among such macroeconomic factors, existing studies have focused on the sectoral composition of growth, literacy and health conditions, social spending, and the inflation rate.

In all the previous studies, the poverty impact of sector-specific growth rates was found to vary substantially and significantly across sectors, but the relative sector ranking was not the same in different countries. For example, agricultural growth was the most effective in China (Ravallion and Chen (2007) and Montalvo and Ravallion (2010)) while it was the service sector to play the most important role in India and Brazil (Datt and Ravallion (1998), Ravallion and Datt (2002) and Ferreira et al. (2010)). Using cross-country data and the same method of analysis as above, with country fixed effects and interaction terms, I find that the growth-elasticity of poverty has been smaller in countries where the growth of value added in the manufacturing sector was faster than average. Allowing for region specific slopes, this effect seems to be strongest for the East Asia and Pacific region. Results are shown in table 4.

Moreover, countries with a higher than average inflation rate also experienced a smaller impact of growth on poverty during the period considered. This is a repeated finding both in the cross-country (Easterly and Fischer (2001) and Dollar and Kraay (2002)) and in the case study literature, for the

[10]Robilliard et al. (2002), Chen and Ravallion (2004), Ferreira et al. (2003)

cases of Brazil, China and India (respectively Ferreira et al. (2010), Ravallion and Chen (2007) and Datt and Ravallion (1998)). The interpretation put forward by Easterly and Fischer (2001) is that the poor are less able to hedge against inflation, as they are likely to hold relatively more cash, while Datt and Ravallion (1998) argue that a continuing higher rate of inflation erodes real wages over time.

No other factor has a significant effect on the growth-elasticity of poverty in my analysis.

5 The effect of constitutions

In this section, I focus on the effect of constitutions, which is the main innovation of this paper.

An important component of a country's institutional arrangement is the design of the constitution. There is by now a large literature on the economic effects of constitutions. While the intermediate link from constitutional design to political and institutional outcomes is generally left to the political scientists, the economic literature on the topic focuses mostly on the reduced form relation from constitutions to economic policies and long-term economic outcomes. In particular, areas that have been explored are the size and composition of public expenditure, fiscal policy, trade policy, regulation, corruption, growth. A comprehensive overview is given in the works of Persson and Tabellini (2000, 2003 and 2005).

The question asked in this paper is similarly of the reduced form type. Assuming that the constitution aggregates a society's preferences into policies by affecting policymakers' constraints and incentives, will the sets of policies resulting from different constitutional designs have different impacts on a long-term outcome such as the extent to which economic growth benefits the poor?

First of all, the question arises whether democratic institutions per se

Table 4: Other contextual effects

	(1)	(2)	(3)
	Value added in manufacturing		High inflation
LogGDP	-0.924***	-1.070***	-1.032***
	(0.239)	(0.203)	(0.229)
LogGDPXManuf.	0.0958**	0.134*	
	(0.0460)	(0.0818)	
LogGDPXInflation			0.0958**
			(0.0431)
LogGDPXECA		-0.0166	
		(0.0280)	
LogGDPXLAC		-0.0367	
		(0.0265)	
LogGDPXMENA		-0.0226	
		(0.0280)	
LogGDPXSA		-0.0296	
		(0.0244)	
LogGDPXSSA		-0.0142	
		(0.0235)	
R^2	0.327	0.220	0.336
Countries	96	96	96
Observations	525	525	525

Notes: The dependent variable is the log of the poverty headcount. All regressions include sample-specific year effects. Column(2) allows for region-specific intercept and slope. ECA stands for Europe and Central Asia, LAC stands for Latin America and the Caribbeans, MENA stands for the Middle East and North Africa, SA stands for South Asia and SSA stands for Sub-Saharan Africa. The excluded group is the East Asia and Pacific region. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

play a role: are more democratic countries better at channeling growth towards poverty reduction goals? The literature on the effects of democracy and democratization is large and rather inconclusive. In particular, while a great deal of literature is available on the relationship between democracy and economic growth, surprisingly little is known about the relationship between democracy and poverty (a summary of the debate is given in Varshney (2000)). Although previously untested in this setting, we can formulate the hypothesis that, by and large, democratic institutions give more voice to the people, and more marginalized groups, such as the poor, get representation and protection more easily in a democratic country.

Moving beyond the effect of democracy per se, in Persson (2005) we find the claim that "the form of democracy, rather than democracy vs. non-democracy per se, may be one of the missing links between history, current policy and economic development".[11] Can we observe, in this particular setting, any effect on poverty reduction related to the form of democracy? I refer here to the two most widely studied features of constitutional design: the form of government and the electoral rule. What should we expect in terms of poverty reduction outcomes from these constitutional features?

One first area of analysis regards the mapping from constitutions to the composition of public spending. Persson and Tabellini (2003), henceforth PT, discuss the theoretical effect of constitutions, which are in essence different ways to aggregate conflicting interests into policies, relating to three different classes of interests that are at stake in different policy choices: the interests of the many (so-called broad-based programs or general interest policies); the interests of a specific group, variously defined (special interest policies); finally, the interests of the political elite (corruption, rent seeking, agency issues in general). Most of the literature surveyed in PT predicts - and their own empirical investigation supports this claim - that less spending for broad-based programs will be associated with presidential constitutions. Two

[11] See also Persson and Tabellini (2006).

features of this form of government encourage the political leaders to resort more often to special interest policies: a more effective separation of powers and the absence of a confidence requirement. These two features generate several institutional veto players, whom the leader has incentive to target with pork barrel in exchange for support (see, e.g., Persson et al. (2000)).

A second area is related to the effectiveness of policies and political leadership. In this respect, different features of the presidential constitution might pull in opposite directions. The separation of power is argued to generate a status quo bias, for the difficulty to have reforms approved, and hence less effective policymaking (see Linz and Valenzuela (1994) and Tsebelis (1995)). A similar effect can be associated with the possibility of a divided government, case in which the president and the congressional majority do not belong to the same party. This case is only possible in presidential regimes while it is ruled out in parliamentary regimes (see, e.g., Alt and Lowry (1994)). On the other hand, the fixed term in office typical of presidential regimes should reduce the policy myopia and allow more room for long term interventions (a mechanism similar to Svensson (1998)).

Summing up, the theoretical predictions about the presidential constitution are unambiguous about the preference for special interest policies at the expense of broad-based programs, but less clear in terms of policy effectiveness.

Coming to the electoral rule, the winner-take-all feature of the plurality rule has, in first approximation, the effect to focus the electoral competition on narrower constituencies. This can lead to a preference for special interest at the expense of general interest policies. The point is made theoretically in Persson and Tabellini (2000), and empirical evidence on the composition of spending under alternative electoral rules is provided in PT. More recent quasi-experimental and experimental micro-evidence is provided in Gagliarducci et al. (2008) and Fréchette et al. (2009).

There are more details about the electoral rule. The first feature consid-

ered more closely in the literature is the size of the electoral district, i.e. the number of representatives elected in each district. On the one hand, larger districts mean that the candidates seeking election must appeal to a larger constituency: this pulls in the direction of general interest policies. Moreover, a smaller district size might result in the selection of a lower "quality" candidate. In other words, the voters might support a representative for ideological reasons, notwithstanding her low quality, only because the competition is stiffer in smaller districts. One example of this mechanism is modeled in Myerson (1993). A second feature whose effects have been considered in the literature is the ballot structure, and in particular the use of closed lists. In principle, the fact that the voters can express preferences on individuals rather than a list decided by the party should affect the personal accountability of the candidate. While this has no clear implications in terms of the composition of spending, the implications are clear with respect to outcomes like corruption or electoral spending cycles (Persson and Tabellini (2000), Kunicova and Rose-Ackerman (2005)). Stretching slightly this argument, these predictions can be extended to a broader idea of "quality" of the candidate.

Summing up, the theoretical predictions about the electoral rule are that the assignment of seats according to the plurality rule and smaller electoral districts should be associated with less preference for broad-based policies. Moreover, the use of closed lists and a smaller district size make more likely the selection of lower quality candidates, which in turns can affect the quality and effectiveness of policies.

Finally, the combination of the two constitutional features can deliver different effects. In particular, parliamentary regimes with majoritarian electoral rules are more likely to produce single-party majority governments (Taagepera and Shugart (1989), Persson and Tabellini (2003)), which in turn can have an ambiguous effect: a good leadership can be, in this situation, more effective, since it is unrestrained, while the converse holds for an in-

competent or corrupt leadership. On the other hand, this system might lead to larger swings in the ideological preferences of the government at election times compared to systems where coalition governments are more common (Alesina et al. (1997), Persson and Tabellini (2003)).

Related to this last point, one further interesting concept is what Aghion et al. (2004) call "insulation" of leaders, or the degree to which, once elected, the executive power can or can not be restrained. The expected effect of this variable is subject to the same sort of ambiguity: unrestrained power is good only if in good hands.

How does poverty reduction fit in this framework? A first set of predictions can be derived from the previous results on the composition of spending. According to the classification of policies given above, a policy strategy for poverty reduction can be considered a broad-based program, akin to redistribution.[12] As such, it is expected to receive less support if the executive power has the characteristics associated with a presidential regime, or the legislators are elected under a majoritarian rule. The assumption needed for the empirical analysis performed below is that a weaker support for these policies will result and be observable in terms of slower rates of poverty reduction corresponding to the same growth in income, controlling for other factors, i.e. in a smaller (in absolute value) η in equation (1).

With respect to quality of political leaders and policies, under the plausible assumption that poverty reduction requires deep reforms, effective policies and a consistent effort over time, the selection of lower quality politicians and a reduced effectiveness of policies can be expected to be associated with a smaller elasticity of poverty to growth. The predictions in terms of quality and effectiveness from the above discussion are clear for closed lists and dis-

[12]There is no clear consensus on the most effective policy menu to address poverty reduction. In particular, redistribution systems of the kind existing in most western countries are not fully functional in many developing countries at the present. Complementary interventions in very different areas are needed in most cases, but we can broadly think of them as social policies.

trict size, but ambiguous both for the presidential regime, the single-party rule, and also for the "insulation" of the executive power. However, they can be tested empirically.

5.1 Empirical results

Table 5: Effect of democratic institutions

	(1) Democracies	(2) Autocracies	(3) Joint
Log GDP	-0.978*	-0.678***	-0.739***
	(0.510)	(0.202)	(0.215)
LogGDPXDemocracy			-0.126*
			(0.0709)
R^2	0.226	0.435	0.336
Countries	56	67	92
Observations	294	211	505

Notes: The dependent variable is the log of the poverty headcount. Democracies are defined by having a value 5 or greater in the Polity index. Column (1) and (2) estimate separate regressions, column (3) pools together all the observations with an interaction term. All regressions include regime-specific year effects. Robust standard errors clustered at the country level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The sample includes both democratic and non democratic countries, so I can separate the effect of the form of democracy from the democratic rule per se. As depicted in table 5, the elasticity of poverty to growth is larger[13] in democracies compared to autocracies. This is consistent with the claim that poor people get more voice and more weight in the political process in a democratic country. The difference between the two subsamples is significant, as evidenced by the interaction term in the third column of the table. The effect is robust to controlling for regional effects, both in the intercept

[13]Remember that the elasticity has to be read in absolute values.

and the slope. The size of the coefficient can be interpreted as follows. Consider a non-democratic fast-growing country like Tajikistan, where GDP per capita has been growing at an average rate of almost 7% in the last 10 years. Projecting the same average growth rate in the future up to 2015, and applying to this the average elasticity of autocracies, Tajikistan can be expected to reduce its poverty rate to 12.7% from the 2004 value of 21.5%. If we apply to the same growth rate the average elasticity of a democratic country, instead, Tajikistan's poverty rate would be predicted to fall to 9.9%. This is a difference of 180 thousand people.

Table 6: Effect of constitutional features

	(1) Electoral rule	(2) Form of government	(3) Combination
Log GDP	-2.015*	0.663**	-0.999*
	(1.013)	(0.261)	(0.524)
LogGDPXMaj	1.818*		
	(1.047)		
LogGDPXPres		-2.343***	
		(0.793)	
LogGDPXParMaj			2.213***
			(0.653)
R^2	0.473	0.238	0.254
Countries	60	49	56
Observations	258	227	296

Notes: The dependent variable is the log of the poverty headcount. Only observations from democratic countries are included. All regressions include regime-specific year effects. Robust standard errors clustered at the counry level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Even larger effects are observable in tables 6 and 7, which present the estimation of equation (1) with the constitutional rules as contextual factors.[14]

[14]The drop in the number of observations is due to the fact that I only consider here

As argued above, once country and year fixed effects are included, the constitutions are close to randomly assigned. However, a plausible instrumental variable strategy has been proposed, in PT, to isolate some truly exogenous variation in constitutional rules and estimate their effects in terms of size and composition of public spending and other economic outcomes. This strategy offers a valuable complement to the fixed effect analysis even in this context. In table 7, the selection of the constitution is predicted, in the first stage, using seven IV: three indicators plus a continuous variable for the age of the constitution, the fraction of the population whose mother tongue is English or another European language, and the distance from the equator. The exclusion restrictions are somewhat more sensible for the first four, implying that the timing of adoption of the constitution has no direct effect on the rate of poverty reduction between 1980 and 2008. Assuming the exogeneity of these four, the validity of the other instruments can be tested. The Sargan-Hansen test fails to reject the overidentifying restrictions for the full set of instruments (p-values reported in in table 7). The results of this analysis confirm and strengthen the FE analysis, and deliver somewhat more plausible point estimates.

As reported in column (1) of both tables, a majoritarian electoral rule is associated with smaller growth-elasticity of poverty, which is in accordance to predictions from the theory about the composition of spending. This is not due to geographic concentration of this constitutional feature, as the effect is robust to controlling for regional effects both in the intercept and the slope. In countries with a majoritarian electoral rule, growth has a significantly smaller effect on poverty reduction: the point estimates are at -2.5 for proportional and -1.2 for majoritarian countries. This is a big difference. A majoritarian country like Brazil has been growing at an average of 1.03% per year between 1990 and 2004. Projecting forward the same growth rate up to 2015, and keeping everything else equal, Brazil would

---

the subsample of democracies, i. e. observations for which the polity index is above 5.

reduce its poverty rate, from the 2004 level of 11.68% to 10.15% in 2015 with the average elasticity of majoritarian countries. The poverty rate would fall instead to 8.7% with the average elasticity of proportional countries. This makes a difference for several millions of poor people.

Both a presidential constitution and weaker checks and balances on the executive power are associated with a larger growth-elasticity of poverty: one story consistent with these results is related to the effectiveness of a strong leader. An increase in the constraints on the executive to the top value of the Polity measure raises the elasticity only marginally.[15] The presidential constitution has instead a very big and significant impact on the elasticity in relative terms, with a difference of a factor of three between the two subsamples (table 7, column (2)), although confidence intervals are consistent with much closer values. Continuing with the example, in Brazil, which currently has a presidential constitution, the poverty rate could be predicted to fall to 9.5% (respectively, 10.78%) by 2015 with the average elasticity of presidential (parliamentary) countries and a constant growth rate.

When both presidential form of government and majoritarian electoral rule are present together, it is the electoral rule to have the stronger influence: the elasticity is smaller for these countries, compared to the three other possible combinations. These differences, however, are washed away by country specific heterogeneity.[16] In countries that have both a parliamentary constitution and a majoritarian electoral rule, instead, poverty is significantly less responsive to growth, as shown in tables 6 and 7, column (3). One theory consistent with this result would be the one in Alesina et al. (1997), referred above, according to which these countries are typically subject to large swings in the ideological preferences of the government at election times, which somehow would introduce more instability in policies.

The results in terms of the other details of the constitution are more mixed

[15]Results not reported.
[16]Results not shown.

Table 7: Effect of constitutional features, IV estimation

	(1) Electoral rule	(2) Form of government	(3) Combination
Log GDP	-2.528***	-0.701*	-1.784***
	(0.658)	(0.415)	(0.295)
LogGDPXMaj	1.304*		
	(0.727)		
LogGDPXPres		-1.110**	
		(0.497)	
LogGDPXParMaj			1.059**
			(0.500)
R^2			
Countries	39	37	37
Sargan-Hansen (p-val)	0.265	0.626	0.586
Observations	262	260	260

Notes: The dependent variable is the log of the poverty headcount. Only observations from democratic countries are included. All regressions include regime-specific year effects. Robust standard errors clustered at the counry level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

and difficult to reconcile with a consistent interpretation.[17] The elasticity is larger in countries where the executive is subject to term limits. Larger districts and closed list are also associated with a larger elasticity of poverty to growth. Finally, I also looked at the fractionalization of the government and the legislature, a measure for the probability that two randomly picked members of the cabinet, respectively the legislature, belong to two different parties. The elasticity is smaller with bigger fractionalization, and this supports once more the interpretation about the greater effectiveness of a strong undivided leadership facing less veto players. There are relatively few observations for these measures, though, so these results are less robust.

The findings of this analysis can be summarized as follows: economic growth contributes to poverty reduction to a significantly larger extent in democratic countries, and within democracies, in countries that have a proportional as opposed to a majoritarian electoral rule or a presidential as opposed to a parliamentary form of government. How can we interpret these results? Maybe there is no need to stress that the interpretation cannot be normative, in the sense of recommending constitutional reforms. A regime change is a complicated process with many interactions and partly unforeseeable consequences. Nevertheless, we observed that, even within democratic systems, the way representatives are elected and the balance of power between political institutions have an impact on how well policies are able to address broad social issues like poverty reduction. Under some constitutional arrangements, economic growth seems to "trickle down" to a lesser extent, and hence pro-growth policies should be complemented by other more direct interventions in support of the poor.[18]

[17] Results are not reported.

[18] In a forthcoming book, Kenworthy (2011) shows that, in 20 industrial countries, when a positive relation is observed between average income growth and growth in the incomes of the bottom-decile household, this is overwhelmingly due to the transfer component of the latter. In other words, even in rich, democratic western countries, no "trickle down" would have happened without redistribution!

6 Conclusions

The importance of understanding the fine details around the average positive relationship between economic growth and poverty reduction is recognized since long. This paper offers a new take on it. In particular, I explore for the first time if and how this relationship is affected by the institutional framework of the country, meaning by this the constitution.

I use new data since the most recent overview on the topic, although the lag in data availability implies that the impact of the recent food and fuel price crises and the global financial crisis are still not reflected in these estimates. Many scholars believe that some of the gains in terms of poverty reduction that are visible in these new data have already been undone during the very last couple of years. Newer and better data, in particular more abundant and more comparable micro-data, will in the future allow to answer more questions and provide better guidance in the design of effective policies for poverty reduction.

The main results provided in this study can be summarized as follows: during the last three decades, poverty has been on average less responsive to growth in countries with a majoritarian electoral system and high inflation rates, and more responsive to growth in democracies and in particular presidential democracies. Although these results are primarily descriptive, they can nevertheless be accompanied by a normative recommendation: economic growth in itself is not enough to achieve goals of poverty reduction; this is true in some cases more than others, and I stressed here the case of countries with particular constitutional features. Although this results as an endogenous outcome of incentives provided to the political leaders by the constitutional arrangement, it is necessary in these cases to invoke interventions in support of the poor to complement pro-growth policies.

Bibliography

Aghion, P., A. Alesina, and F. Trebbi (2004). Endogenous Political Institutions. *Quarterly Journal of Economics 119*(2), 565–611.

Alesina, A., N. Roubini, and G. Cohen (1997). *Political Cycles and the Macroeconomy*. MIT Press.

Alt, J. and R. Lowry (1994). Divided Government, Fiscal Institutions, and Budget Deficits: Evidence from the States. *American Political Science Review 88*(4), 811–828.

Beck, T., G. Clarke, A. Groff, and P. Keefer (2004). New Tools and New Tests in Comparative Political Economy: The Database of Political Institutions. *World Bank*.

Besley, T. and R. Burgess (2003). Halving Global Poverty. *Journal of Economic Perspectives 17*(3), 3–22.

Bigsten, A. and A. Shimeles (2007). Can Africa reduce poverty by half by 2015? *Development policy review 25*(2), 147–166.

Bourguignon, F., F. Ferreira, and N. Lustig (Eds.) (2004). *The microeconomics of income distribution dynamics in East Asia and Latin America*. World Bank and Oxford University Press.

Chen, S. and M. Ravallion (2004). Welfare impacts of China's accession to the world trade organization. *The world bank economic review 18*(1), 29.

Chen, S. and M. Ravallion (2008). The developing world is poorer than we thought, but no less successful in the fight against poverty. World Bank Policy Research Working Paper 4703.

Dalgaard, C. and L. Erickson (2009). Reasonable expectations and the first millennium development goal: how much can aid achieve? *World development 37*(7), 1170–1181.

Datt, G. and M. Ravallion (1998). Why have some Indian states done better than others at reducing rural poverty? *Economica 65*(257), 17–38.

Dollar, D. and A. Kraay (2002). Growth is Good for the Poor. *Journal of economic growth 7*(3), 195–225.

Easterly, W. and S. Fischer (2001). Inflation and the Poor. *Journal of Money, Credit and Banking 33*(2), 160–178.

Ferreira, F., P. Leite, L. da Silva, and P. Picchetti (2003). Aggregate Shocks and Income Distribution: Can Macro-Micro Models Help Identify Winners and Losers from a Devaluation? World Bank, Washington, DC. Mimeo.

Ferreira, F., P. Leite, and M. Ravallion (2010). Poverty reduction without economic growth?:: Explaining Brazil's poverty dynamics, 1985-2004. *Journal of Development Economics 93*(1), 20–36.

Ferreira, F. H. and M. Ravallion (2008, May). Global poverty and inequality : a review of the evidence. Policy Research Working Paper Series 4623, The World Bank.

Fréchette, G., J. Kagel, and M. Morelli (2009). Pork Versus Public Goods: An Experimental Study of Public Good Provision Within a Legislative Bargaining Framework. Working Paper.

Gagliarducci, S., T. Nannicini, and P. Naticchioni (2008, February). Electoral rules and politicians' behavior: A micro test. IZA Discussion Papers 3348, Institute for the Study of Labor (IZA).

Kenworthy, L. (forth. 2011). *Progress for the Poor*. Oxford Universtity Press.

Kunicova, J. and S. Rose-Ackerman (2005). Electoral Rules and Constitutional Structures as Constraints on Corruption. *British Journal of Political Science 35*(04), 573–606.

Linz, J. and A. Valenzuela (1994). *The failure of presidential democracy: Comparative perspectives*. Johns Hopkins Univ Pr.

Montalvo, J. and M. Ravallion (2010). The pattern of growth and poverty reduction in China. *Journal of Comparative Economics 38*(1), 2–16.

Myerson, R. (1993). Effectiveness of Electoral Systems for Reducing Government Corruption: A Game-Theoretic Analysis. *Games and Economic Behavior 5*(1), 118–32.

Persson, T. (2005). Forms of Democracy, Policy and Economic Development. NBER Working Paper 11171.

Persson, T., G. Roland, and G. Tabellini (2000). Comparative Politics and Public Finance. *Journal of Political Economy 108*(6), 1121–1161.

Persson, T. and G. Tabellini (2000). *Political economics*. MIT Press Cambridge, Mass.

Persson, T. and G. Tabellini (2003). *The Economic Effects of Constitutions*. MIT Press.

Persson, T. and G. Tabellini (2006). Democracy and development: The devil in the details. *The American Economic Review 96*(2), 319–324.

Ravallion, M. (2001). Growth, inequality and poverty: looking beyond averages. *World Development 29*(11), 1803–1815.

Ravallion, M. and S. Chen (2007). China's (uneven) progress against poverty. *Journal of Development Economics 82*(1), 1–42.

Ravallion, M. and G. Datt (2002). Why has economic growth been more pro-poor in some states of India than others? *Journal of development economics 68*(2), 381–400.

Robilliard, A., F. Bourguignon, and S. Robinson (2002). Crisis and Income Distribution: A Micro-Macro Model for Indonesia. Mimeo. IFPRI, Washington, DC.

Ryten, J. (1998). The Evaluation of the International Comparison Project (ICP). Technical report, Consultant Report for the International Monetary Fund, United Nations and World Bank.

Svensson, J. (1998). Investment, property rights and political instability: Theory and evidence. *European Economic Review 42*(7), 1317–1341.

Taagepera, R. and M. Shugart (1989). *Seats and Votes: The Effects and Determinants of Electoral Systems*. Yale University Press.

Tsebelis, G. (1995). Decision Making in Political Systems: Veto Players in Presidentialism, Parliamentarism, Multicameralism and Multipartyism. *British Journal of Political Science 25*(3), 289–325.

Varshney, A. (2000). Democracy and Poverty. Paper for the Conference on World Development Report 2000, U.K. Department for International Development and the Institute of Development Studies, Sussex.

A Appendix

A.1 Sample

Table 8: Sample

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
Albania	2002	< 2	1319.49	ECA	7	1	0
	2004	< 2	1466.31	ECA	7	1	0
	2005	< 2	1541.04	ECA	9		

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
Argentina	1992	< 2	6877.86	LAC	7	0	1
	1996	< 2	7497.73	LAC	7	0	1
	1998	< 2	8210.8	LAC	7	0	1
	2002	9.92	6425.13	LAC	8	0	1
	2004	8.4	7486.15	LAC	8	0	1
	2005	4.5	8097.42	LAC	8		
	2006	3.39	8699.01	LAC	8		
Armenia	1999	18.03	584.082	ECA	5	1	1
	2001	10.99	683.453	ECA	5	1	1
	2002	14.97	774.797	ECA	5	1	1
	2003	10.63	883.449	ECA	5	1	1
	2007	3.65	1425.3	ECA	5		
Azerbaijan	2001	6.32	714.401	ECA	-7	1	1
	2005	< 2	1182.92	ECA	-7		
Bangladesh	1986	43.03	242.41	SA	-5		1
	1992	66.77	265.239	SA	6	1	0
	1996	59.35	292.664	SA	6	1	0
	2000	57.82	334.573	SA	6	1	0
	2005	49.64	400.703	SA	6		
Belarus	1995	< 2	920.034	ECA	0	1	1
	1997	2.66	1061.63	ECA	-7	1	1
	1998	< 2	1156.3	ECA	-7		1
	2000	< 2	1273.05	ECA	-7		1
	2001	< 2	1337.85	ECA	-7		1
	2002	< 2	1411.76	ECA	-7		1
	2005	< 2	1871.39	ECA	-7		
	2007	< 2	2252.48	ECA	-7		
Bolivia	1997	18.94	993.813	LAC	9	1	1

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
	1999	24.7	1005.43	LAC	9	1	1
	2002	22.81	1010.38	LAC	9	1	1
	2005	19.62	1115.98	LAC	8		
	2007	11.86	1124.96	LAC	8		
Bosnia-Herz.	2004	< 2	1766.11	ECA			1
	2007	< 2	2105.93	ECA			
Brazil	1982	17.52	3248.03	LAC	-3	1	0
	1983	20.86	3066.46	LAC	-3	1	0
	1984	20.56	3157.31	LAC	-3	1	0
	1985	17.51	3336.38	LAC	7	1	0
	1986	12.29	3530.12	LAC	7	1	1
	1987	16.68	3586.27	LAC	7	1	1
	1988	17.66	3515.96	LAC	8	1	1
	1989	14.59	3566.52	LAC	8	1	1
	1990	15.49	3354.75	LAC	8	1	1
	1992	13.29	3281.75	LAC	8	1	1
	1993	12.97	3382.55	LAC	8	1	1
	1995	10.51	3609.49	LAC	8	1	1
	1996	11.43	3631.74	LAC	8	1	1
	1997	11.98	3698.14	LAC	8	1	1
	1998	11.03	3644.6	LAC	8	1	1
	1999	11.15	3600.25	LAC	8	1	1
	2001	10.96	3697.23	LAC	8	1	1
	2002	9.81	3743.3	LAC	8	1	1
	2003	10.43	3735.97	LAC	8	1	1
	2004	11.68	3899.41	LAC	8	1	1
	2005	7.76	3974.82	LAC	8		
	2006	7.36	4086.34	LAC	8		

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
	2007	5.21	4290.53	LAC	8		
Bulgaria	1992	< 2	1490.89	ECA	8	0	0
	1994	< 2	1514.14	ECA	8	0	0
	1995	2.02	1563.94	ECA	8	0	0
	1997	< 2	1351.73	ECA	8	0	0
	2001	2.64	1658.15	ECA	9	0	0
	2003	< 2	1839.76	ECA	9	0	0
Burkina Faso	1998	70.03	216.84	SSA	-4	0	1
	2003	56.54	245.064	SSA	0	0	1
Burundi	1998	86.43	114.462	SSA	-1	0	1
	2006	81.32	109.183	SSA	6		
Cambodia	2004	40.19	369.508	EAP	2	0	0
	2007	25.84	486.699	EAP	2		
Cameroon	2001	32.81	648.291	SSA	-4	1	1
Chile	1990	4.37	3067.02	LAC	8	1	1
	1994	2.6	3912.57	LAC	8	1	1
	1996	< 2	4504.62	LAC	8	1	1
	1998	< 2	4822.81	LAC	8	1	1
	2000	< 2	4877.88	LAC	9	1	1
	2003	< 2	5174.68	LAC	9	1	1
	2006	< 2	5869.65	LAC	10		
China	1984	69.4327	258.721	EAP	-7		0
	1987	54.0292	341.023	EAP	-7		0
	1990	60.1817	391.655	EAP	-7		0
	1993	53.6871	536.36	EAP	-7		0
	1996	36.3714	716.248	EAP	-7		0
	1999	35.6283	882.556	EAP	-7		0
	2002	28.3631	1105.96	EAP	-7		0

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
	2005	15.9205	1464.11	EAP	-7		
Colombia	1988	11.03	2096.97	LAC	8	0	1
	1989	8.36	2125.71	LAC	8	0	1
	1991	8.27	2217.39	LAC	9	0	1
	1995	11.23	2464.61	LAC	7	0	1
	1996	13.54	2470.54	LAC	7	0	1
	1998	16.1	2481.09	LAC	7	0	1
	1999	16.54	2336.43	LAC	7	0	1
	2000	16.77	2364.75	LAC	7	0	1
	2003	15.36	2467.81	LAC	7	0	1
	2006	16.01	2789.08	LAC	7		
Costa Rica	1986	10.38	2851.77	LAC	10	0	1
	1990	9.16	3111.48	LAC	10	0	1
	1992	8.44	3316.31	LAC	10	0	1
	1993	7.9	3477.29	LAC	10	0	1
	1996	7.08	3545.73	LAC	10	0	1
	1997	4.52	3650.28	LAC	10	0	1
	1998	3.96	3859.38	LAC	10	0	1
	2000	4.41	4056.73	LAC	10	0	1
	2001	3.53	4012.52	LAC	10	0	1
	2003	5.61	4221.76	LAC	10	0	1
	2005	2.37	4501.22	LAC	10		
	2007	< 2	5123.72	LAC	10		
Côte d'Ivoire	1986	4.11	742.242	SSA	-9	1	1
	1987	8.68	712.326	SSA	-9	1	1
	1988	13.76	694.531	SSA	-9	1	1
	1993	17.79	589.478	SSA	-7	1	1
	1995	21.09	595.791	SSA	-6	1	1

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
	1998	24.06	649.276	SSA	-6	1	1
	2002	23.34	567.959	SSA	0	1	1
Croatia	1999	< 2	4543.89	ECA	1	1	1
	2000	< 2	4817.07	ECA	8	1	1
	2001	< 2	4986.03	ECA	8	1	1
	2005	< 2	5991.93	ECA	9		
Dominican Rep.	1989	12.16	1949.44	LAC	6	1	1
	1992	4.6	1935.22	LAC	6	1	1
	1996	5.87	2227.17	LAC	8	1	1
	1997	6.71	2364.86	LAC	8	1	1
	2000	4.41	2717.68	LAC	8	1	1
	2003	6.12	2786.35	LAC	8	1	1
	2005	4.98	2993.34	LAC	8		
	2006	3.96	3264.72	LAC	8		
	2007	4.426	3490.89	LAC	8		
Ecuador	1994	15.87	1335.15	LAC	9	0	1
	1998	14.92	1382.14	LAC	9	0	1
	2003	10.49	1419.45	LAC	6	0	1
	2005	9.78	1589.09	LAC	6		
	2007	4.69	1680.5	LAC	5		
Egypt	1996	2.46	1250.35	MENA	-6	1	0
	2000	< 2	1422.73	MENA	-6	1	0
	2005	< 2	1539.21	MENA	-3		
El Salvador	1995	12.68	1972.44	LAC	7	1	1
	1996	14.99	1985.51	LAC	7	1	1
	1997	13.11	2052.18	LAC	7	1	1
	1998	13.48	2114.14	LAC	7	0	1
	2000	12.77	2209.16	LAC	7	0	1

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
	2002	14.16	2280.12	LAC	7	0	1
	2003	14.25	2324.45	LAC	7	0	1
	2005	10.97	2423.67	LAC	7		
	2007	6.43	2621.65	LAC	7		
Estonia	1993	< 2	2744.22	ECA	6	0	0
	1995	< 2	2945.86	ECA	6	0	0
	1998	< 2	3742.38	ECA	6	0	0
	2000	< 2	4144.38	ECA	9	0	0
	2001	< 2	4479.69	ECA	9	0	0
	2002	< 2	4858.22	ECA	9	0	0
	2003	< 2	5229.64	ECA	9	0	0
	2004	< 2	5680.38	ECA	9	0	0
Ethiopia	1995	60.52	114.957	SSA	1		1
	2000	55.58	124.851	SSA	1	1	0
	2005	39.04	149.657	SSA	1		
Georgia	1997	4.58	605.163	ECA	5	1	1
	1998	6.9	631.82	ECA	5	1	1
	1999	8.65	657.88	ECA	5	1	1
	2000	9.59	678.302	ECA	5	1	1
	2001	9.24	719.829	ECA	5	1	1
	2002	15.1	768.589	ECA	5	1	1
	2003	17.27	867.084	ECA	5	1	1
	2005	13.44	998.488	ECA	7		
Ghana	1989	49.37	217.197	SSA	-7		1
	1992	51.07	225.377	SSA	-1		1
	1998	39.12	247.048	SSA	2	1	1
	2006	29.99	302.384	SSA	8		
Guatemala	1989	39.33	1435.54	LAC	3	1	1

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
	1998	15.65	1673.09	LAC	8	1	1
	2000	13.06	1717.66	LAC	8	1	1
	2002	16.92	1738.59	LAC	8	1	1
	2006	11.7	1811.17	LAC	8		
Guinea	1994	36.77	334.294	SSA	-5		1
	2003	70.13	394.911	SSA	-1	1	1
Guinea-Bissau	1993	52.11	182.376	SSA	-6		1
	2002	48.83	146.55	SSA	5		1
Honduras	1989	39.72	1078.26	LAC	6	1	0
	1990	43.5	1049.23	LAC	6	1	1
	1992	33.33	1083.14	LAC	6	1	1
	1994	28.28	1078.22	LAC	6	1	1
	1997	15.6	1138.08	LAC	6	1	1
	1999	14.44	1101.15	LAC	7	1	1
	2003	18.1	1195.69	LAC	7	1	1
	2005	22.19	1294.09	LAC	7		
	2006	18.19	1352.79	LAC	7		
Hungary	1989	< 2	4383.7	ECA	4	1	0
	1993	< 2	3606.44	ECA	10	1	0
	1998	< 2	4212.68	ECA	10	1	0
	1999	< 2	4403.46	ECA	10	1	0
	2000	< 2	4689.61	ECA	10	1	0
	2001	< 2	4893.09	ECA	10	1	0
	2002	< 2	5122.96	ECA	10	1	0
	2004	< 2	5622.86	ECA	10	1	0
India	1983	55.5111	252.609	SA	8	1	0
	1988	53.5928	296.627	SA	8	1	0
	1994	49.4019	351.867	SA	8	1	0

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
	2005	41.6442	588.721	SA	9		
Iran	1990	3.85	1292.16	MENA	-6	1	1
	1994	< 2	1394.96	MENA	-6	1	1
	1998	< 2	1527.98	MENA	3	1	1
	2005	< 2	1924.39	MENA	-6		
Jamaica	1990	< 2	3159.36	LAC	10	1	0
	1993	3.82	3617.77	LAC	9	1	0
	1996	< 2	3646.81	LAC	9	1	0
	1999	< 2	3468.98	LAC	9	1	0
	2002	< 2	3521.83	LAC	9	1	0
	2004	< 2	3721.3	LAC	9	1	0
Jordan	1992	2.77	1660.04	MENA	-2	1	1
	1997	< 2	1709.6	MENA	-2	1	1
	2003	< 2	1901.47	MENA	-2	1	1
	2006	< 2	2246.62	MENA	-2		
Kazakhstan	1996	4.98	1043.73	ECA	-4	1	1
	2001	< 2	1397.29	ECA	-4	1	1
	2002	5.15	1534.17	ECA	-6	1	1
	2003	3.12	1671.21	ECA	-6	1	1
	2007	< 2	2332.29	ECA	-6		
Kenya	1994	28.55	409.47	SSA	-5	1	1
	1997	19.57	410.758	SSA	-2	1	1
	2005	19.72	423.64	SSA	8		
Kyrgyz Rep.	1993	18.61	303.638	ECA	-3		1
	1998	31.84	261.241	ECA	-3	1	1
	2002	34.03	288.874	ECA	-3	1	1
	2004	21.81	324.369	ECA	-3	1	1
	2007	3.42	352.537	ECA	3		

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
Lao PDR	1997	49.32	290.813	EAP	-7	1	0
	2002	43.96	347.152	EAP	-7	1	0
Latvia	1993	< 2	2271.33	ECA	8		0
	1995	< 2	2364.05	ECA	8	0	0
	1996	< 2	2477.31	ECA	8	0	0
	1997	< 2	2727.39	ECA	8	0	0
	1998	< 2	2903.61	ECA	8	0	0
	2002	< 2	3854.11	ECA	8	0	0
	2004	< 2	4538.9	ECA	8	0	0
	2007	< 2	6296.23	ECA	8		
Lesotho	1993	56.43	369.958	SSA	8		1
	1995	47.59	393.335	SSA	8	1	0
	2003	43.41	435.426	SSA	8	1	0
Lithuania	1996	< 2	2684.61	ECA	10	1	1
	1998	< 2	3148.9	ECA	10	1	1
	2000	< 2	3267.36	ECA	10	1	1
	2001	< 2	3505.7	ECA	10	1	1
	2002	< 2	3759.49	ECA	10	1	1
	2004	< 2	4492.78	ECA	10	1	1
Macedonia	2000	2.94	1783.09	ECA	6	1	0
	2002	< 2	1706.49	ECA	9	1	0
	2003	< 2	1750.63	ECA	9	0	0
	2006	< 2	1962.8	ECA	9		
Madagascar	1993	72.49	256.119	SSA	9	1	1
	1997	72.04	243.873	SSA	8	1	1
	1999	82.32	249.635	SSA	7	1	1
	2001	76.34	261.351	SSA	7	1	1
	2005	67.83	246.352	SSA	7		

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
Malaysia	1987	2.39	2174.66	EAP	4	1	0
	1989	< 2	2459.94	EAP	4	1	0
	1992	< 2	2949.97	EAP	4	1	0
	1995	2.08	3604.01	EAP	3	1	0
	1997	< 2	4043.64	EAP	3	1	0
	2004	< 2	4458.56	EAP	3	1	0
Mali	1994	86.08	189.203	SSA	7	1	1
	2001	61.18	252.41	SSA	6	1	1
	2006	51.43	286.236	SSA	7		
Mauritania	1993	42.79	416.462	SSA	-6	1	1
	1996	23.4	432.692	SSA	-6	1	1
	2000	21.16	415.237	SSA	-6	1	1
Mexico	1992	4.48	5168.6	LAC	0	1	1
	1994	3.33	5309.08	LAC	4	1	1
	1996	6.98	5063.81	LAC	4	1	1
	1998	8.01	5512.59	LAC	6	1	1
	2000	4.82	5934.98	LAC	8	1	1
	2002	3.73	5852.99	LAC	8	1	1
	2004	2.8	6048.41	LAC	8	1	1
	2006	< 2	6413.71	LAC	8		
	2008	3.95	6592.09	LAC	8		
Moldova	1997	15.11	382.076	ECA	7	0	1
	1999	44.18	346.016	ECA	7	0	1
	2001	33.02	376.436	ECA	8	0	0
	2002	17.08	406.739	ECA	8	0	0
	2004	8.14	467.98	ECA	8	0	0
	2007	2.38	547.066	ECA	8		
Mongolia	1998	34.15	450.216	EAP	10	1	1

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
	2002	15.47	479.068	EAP	10	1	1
	2008	2.24	735.338	EAP	10		
Morocco	1991	2.45	1229.11	MENA	-8	1	1
	1999	6.76	1266.47	MENA	-6	1	1
	2000	6.25	1270.33	MENA	-6	1	1
	2001	6.25	1349.56	MENA	-6	1	1
	2007	2.5	1647.87	MENA	-6		
Nepal	1996	68.44	206.517	SA	5	1	0
	2004	55.12	235.152	SA	-6	1	0
Nicaragua	1998	21.76	715.506	LAC	8	0	1
	2001	19.42	782.906	LAC	8	0	1
	2005	15.81	842.781	LAC	8		
Niger	1994	78.17	169.378	SSA	8	1	1
	2005	65.88	168.401	SSA	6		
Nigeria	1993	49.19	366.768	SSA	-7		1
	1996	68.51	364.301	SSA	-6		1
	2004	64.41	426.999	SSA	4		1
Pakistan	1991	64.71	476.68	SA	8	1	0
	1997	48.14	519.43	SA	7	1	0
	1999	29.05	526.231	SA	-6	1	0
	2002	35.87	537.26	SA	-5		1
	2005	22.59	605.738	SA	-5		
Panama	1991	16.88	3151.15	LAC	8	1	1
	1995	11.47	3467.02	LAC	9	1	1
	1996	12.44	3493.11	LAC	9	1	1
	1997	7.17	3644.89	LAC	9	1	1
	2000	11.5	3938.08	LAC	9	1	1
	2001	13.81	3886.6	LAC	9	1	1

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
	2002	10.79	3900.3	LAC	9	1	1
	2004	9.2	4215.54	LAC	9	1	1
	2006	9.48	4736.96	LAC	9		
Paraguay	1995	12.69	1486.95	LAC	7	0	1
	1998	19.63	1447.38	LAC	6	0	1
	1999	14.33	1396.11	LAC	7	0	1
	2002	17.23	1294.84	LAC	7	0	1
	2005	9.3	1359.18	LAC	8		
	2007	6.45	1458.83	LAC	8		
Peru	1990	< 2	1657.33	LAC	8	0	1
	1994	5.74	1845.27	LAC	1	0	1
	1996	8.59	1982.89	LAC	1	0	1
	2001	15.13	2023.72	LAC	9	0	1
	2002	12.55	2095.5	LAC	9	0	1
	2005	8.18	2350.66	LAC	9		
	2006	7.94	2502.04	LAC	9		
	2007	7.69	2692.17	LAC	9		
Philippines	1988	30.48	864.482	EAP	8	1	1
	1991	30.68	874.518	EAP	8	1	1
	1994	28.11	873.625	EAP	8	1	1
	1997	21.61	954.087	EAP	8	1	1
	2000	22.45	977.129	EAP	8	1	1
	2003	21.99	1028.12	EAP	8	1	1
	2006	22.62	1143.16	EAP	8		
Poland	1993	4.19	3039.92	ECA	8	0	1
	1996	< 2	3620.38	ECA	9	0	1
	1998	< 2	4065	ECA	9	0	1
	1999	< 2	4249.8	ECA	9	0	1

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
	2000	< 2	4454.08	ECA	9	0	1
	2001	< 2	4532.01	ECA	9	0	1
	2002	< 2	4599.55	ECA	10	0	1
	2005	< 2	5223.67	ECA	10		
Romania	1992	< 2	1532.66	ECA	5	0	0
	1994	4.97	1621.72	ECA	5	0	0
	1998	< 2	1632.29	ECA	8	0	0
	2000	3.73	1650.97	ECA	8	0	0
	2001	2.67	1769.59	ECA	8	0	0
	2002	2.86	1887.9	ECA	8	0	0
	2005	< 2	2260.22	ECA	9		
	2007	< 2	2595.6	ECA	9		
Russian Fed.	1996	3.48	1564.12	ECA			
	1999	2.28	1613.7	ECA			
	2001	< 2	1870.05	ECA			
	2002	< 2	1967.52	ECA			
	2005	< 2	2443.96	ECA			
	2007	< 2	2866.37	ECA			
Senegal	1995	54.11	443.124	SSA	-1	1	1
	2001	44.19	482.724	SSA	8	1	1
	2005	33.5	522.336	SSA	8		
Slovak Rep.	1992	< 2	4133.63	ECA			
	1996	< 2	4773.48	ECA	7	0	0
Slovenia	1998	< 2	9120.16	ECA	10	0	0
	2002	< 2	10665.7	ECA	10	0	0
	2004	< 2	11421.1	ECA	10	0	0
South Africa	1995	21.43	2960.42	SSA	9	0	0
	2000	26.2	3019.95	SSA	9	0	0

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
Sri Lanka	1991	15.01	593.009	SA	5	0	1
	1996	16.32	727.148	SA	5	0	1
	2002	13.95	883.457	SA	6	0	1
Tajikistan	2003	36.25	178.727	ECA	-3	1	1
	2004	21.49	195.406	ECA	-3	1	1
Thailand	1988	17.2	1154.2	EAP	3	1	0
	1992	5.45	1600.31	EAP	9		0
	1996	< 2	2096.44	EAP	9	1	0
	1998	< 2	1826.91	EAP	9	1	0
	1999	< 2	1895.06	EAP	9	1	0
	2000	< 2	1968.43	EAP	9	1	0
	2002	< 2	2071.92	EAP	9	1	0
	2004	< 2	2304.84	EAP	9	1	0
Tunisia	1990	5.87	1500.64	MENA	-5	1	1
	1995	6.48	1651.39	MENA	-3	1	1
	2000	2.55	2033.07	MENA	-3	1	1
Turkey	1994	2.1	3368.44	ECA	8	0	0
	2002	< 2	3901.78	ECA	7	0	0
	2005	2.72	4679.58	ECA	7		
	2006	2.57	4938.4	ECA	7		
Turkmenistan	1993	63.53	777.282	ECA	-9	1	1
	1998	24.82	479.455	ECA	-9	1	1
Uganda	1992	70.01	184.742	SSA	-7	1	1
	1996	64.39	227.645	SSA	-4	1	1
	1999	60.49	247.509	SSA	-4	1	1
	2002	57.37	265.63	SSA	-4	1	1
	2005	51.53	291.392	SSA	-1		
Ukraine	1992	< 2	1141.12	ECA	6		1

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
	1996	< 2	608.816	ECA	7	1	1
	1999	2.03	594.28	ECA	7	1	1
	2002	< 2	745.039	ECA	6	1	1
	2005	< 2	960.226	ECA	6		
	2008	< 2	1155.85	ECA	7		
Uruguay	1989	< 2	5477.49	LAC	10	0	1
	1992	< 2	6015.58	LAC	10	0	1
	1996	< 2	6708.32	LAC	10	0	1
	1998	< 2	7280.73	LAC	10	0	1
	2000	< 2	6914.36	LAC	10	0	1
	2001	< 2	6665.13	LAC	10	0	1
	2003	< 2	6067.65	LAC	10	0	1
	2005	< 2	7229.47	LAC	10		
	2006	< 2	7522.28	LAC	10		
	2007	< 2	8060.65	LAC	10		
Uzbekistan	2002	42.33	590.059	ECA	-9	1	1
	2003	46.28	607.725	ECA	-9	1	1
Venezuela	1987	6.51	5030.86	LAC	9	0	1
	1989	2.91	4637.39	LAC	9	0	1
	1993	2.62	5263.65	LAC	8	0	1
	1995	9.35	5119.6	LAC	8	0	1
	1996	14.71	5005.25	LAC	8	0	1
	1997	9.57	5218.15	LAC	8	0	1
	1998	13.97	5132.02	LAC	8	0	1
	2003	18.41	3966.5	LAC	6	0	1
	2005	9.98	5000.08	LAC	6		
	2006	3.53	5401.42	LAC	5		
Vietnam	1998	49.65	364.104	EAP	-7	1	0

Continued on next page

Table 8 – continued from previous page

Country	Year	Poverty HR	GDP p/cap	Region	Polity	Maj	Pres
	2002	40.05	447.538	EAP	-7	1	0
	2004	24.18	503.268	EAP	-7	1	0
	2006	21.45	575.884	EAP	-7		
Yemen, Rep.	1998	12.88	513.248	MENA	-2	1	0
	2005	17.53	552.438	MENA	-2		
Zambia	1993	65.27	368.857	SSA	6	1	1
	1996	62.07	321.822	SSA	1	1	1
	1998	55.4	308.089	SSA	1	1	1
	2003	64.6	328.557	SSA	5	1	1
	2004	64.29	338.773	SSA	5	1	1

MONOGRAPH SERIES

1. Michaely, Michael *The Theory of Commercial Policy: Trade and Protection*, 1973

2. Söderström, Hans Tson *Studies in the Microdynamics of Production and Productivity Change*, 1974

3. Hamilton, Carl B. *Project Analysis in the Rural Sector with Special Reference to the Evaluation of Labour Cost*, 1974

4. Nyberg, Lars and Staffan Viotti *A Control Systems Approach to Macroeconomic Theory and Policy in the Open Economy*, 1975

5. Myhrman, Johan *Monetary Policy in Open Economies*, 1975

6. Krauss, Melvyn *International Trade and Economic Welfare*, 1975

7. Wihlborg, Clas *Capital Market Integration and Monetary Policy under Different Exchange Rate Regimes*, 1976

8. Svensson, Lars E.O. *On Competitive Markets and Intertemporal Resources Allocation*, 1976

9. Yeats, Alexander J. *Trade Barriers Facing Developing Countries*, 1978

10. Calmfors, Lars *Prices, Wages and Employment in the Open Economy*, 1978

11. Kornai, János *Economics of Shortage*, Vols I and II, 1979

12. Flam, Harry *Growth, Allocation and Trade in Sweden. An Empirical Application of the Heckscher-Ohlin Theory*, 1981

13. Persson, Torsten *Studies of Alternative Exchange Rate Systems. An Intertemporal General Equilibrium Approach*, 1982

14. Erzan, Refik *Turkey's Comparative Advantage, Production and Trade Patterns in Manufactures. An Application of the Factor Proportions Hypothesis with Some Qualifications*, 1983

15. Horn af Rantzien, Henrik *Imperfect Competition in Models of Wage Formation and International Trade*, 1983

16. Nandakumar, Parameswar *Macroeconomic Effects of Supply Side Policies and Disturbances in Open Economies*, 1985

17. Sellin, Peter *Asset Pricing and Portfolio Choice with International Investment Barriers*, 1990

18. Werner, Ingrid *International Capital Markets: Controls, Taxes and Resources Allocation*, 1990

19. Svedberg, Peter *Poverty and Undernutrition in Sub-Saharan Africa: Theory, Evidence, Policy*, 1991

20. Nordström, Håkan *Studies in Trade Policy and Economic Growth*, 1992

21. Hassler, John, Lundvik, Petter, Persson, Torsten and Söderlind, Paul *The Swedish Business Cycle: Stylized facts over 130 years*, 1992

22. Lundvik, Petter *Business Cycles and Growth*, 1992

23. Söderlind, Paul *Essays in Exchange Rates, Business Cycles and Growth*, 1993

24. Hassler, John A.A. *Effects of Variations in Risk on Demand and Measures of Business Cycle Comovements*, 1994

25. Daltung, Sonja *Risk, Efficiency, and Regulation of Banks*, 1994

26. Lindberg, Hans *Exchange Rates: Target Zones, Interventions and Regime Collapses*, 1994

27. Stennek, Johan *Essays on Information-Processing and Competition*, 1994

28. Jonsson, Gunnar *Institutions and Incentives in Monetary and Fiscal Policy*, 1995

29. Dahlquist, Magnus *Essays on the Term Structure of Interest Rates and Monetary Policy*, 1995

30. Svensson, Jakob *Political Economy and Macroeconomics: On Foreign Aid and Development*, 1996

31. Blix, Mårten *Rational Expectations and Regime Shifts in Macroeconometrics*, 1997

32. Lagerlöf, Nils-Petter *Intergenerational Transfers and Altruism*, 1997

33. Klein, Paul *Papers on the Macroeconomics of Fiscal Policy*, 1997
34. Jonsson, Magnus *Studies in Business Cycles*, 1997
35. Persson, Lars *Asset Ownership in Imperfectly Competitive Markets*, 1998
36. Persson, Joakim *Essays on Economic Growth*, 1998
37. Domeij, David *Essays on Optimal Taxation and Indeterminacy*, 1998
38. Flodén, Martin *Essays on Dynamic Macroeconomics*, 1999
39. Tangerås, Thomas *Essays in Economics and Politics: Regulation, Elections and International Conflict*, 2000
40. Lidbom, Per Pettersson *Elections, Party Politics and Economic Policy*, 2000
41. Vestin, David *Essays on Monetary Policy*, 2001
42. Olofsgård, Anders *Essays on Interregional and International Political Economics*, 2001
43. Johansson, Åsa *Essays on Macroeconomic Fluctuations and Nominal Wage Rigidity*, 2002
44. Groth, Charlotta *Topics on Monetary Policy*, 2002
45. Gancia, Gino A. *Essays on Growth, Trade and Inequality*, 2003
46. Harstad, Bård *Organizing Cooperation: Bargaining, Voting and Control*, 2003
47. Kohlscheen, Emanuel *Essays on Debts and Constitutions*, 2004
48. Olovsson, Conny *Essays on Dynamic Macroeconomics*, 2004
49. Stavlöt, Ulrika *Essays on Culture and Trade*, 2005
50. Herzing, Mathias *Essays on Uncertainty and Escape in Trade Agreements*, 2005
51. Bonfiglioli, Alessandra *Essays on Financial Markets and Macroeconomics*, 2005
52. Pienaar, Natalie *Economic Applications of Product Quality Regulation in WTO Trade Agreements*, 2005
53. Song, Zheng *Essays on Dynamic Political Economy*, 2005

54. Eisensee, Thomas *Essays on Public Finance: Retirement Behavior and Disaster Relief*, 2005

55. Favara, Giovanni *Credit and Finance in the Macroeconomy*, 2006

56. Björkman, Martina *Essays on Empirical Development Economics: Education, Health and Gender*, 2006

57. Larsson, Anna *Real Effects of Monetary Regimes*, 2007

58. Prado, Jr., Jose Mauricio *Essays on Public Macroeconomic Policy*, 2007

59. Tonin, Mirco *Essays on Labor Market Structures and Policies*, 2007

60. Queijo von Heideken, Virginia *Essays on Monetary Policy and Asset Markets*, 2007

61. Finocchiaro, Daria *Essays on Macroeconomics*, 2007

62. Waisman, Gisela *Essays on Discrimination and Corruption*, 2008

63. Holte, Martin Bech *Essays on Incentives and Leadership*, 2008

64. Damsgaard, Erika Färnstrand *Essays on Technological Choice and Spillovers*, 2008

65. Fredriksson, Anders *Bureaucracy, Informality and Taxation: Essays in Development Economics and Public Finance*, 2009

66. Folke, Olle *Parties, Power and Patronage: Papers in Political Economics*, 2010

67. Drott, David Yanagizawa *Information, Markets and Conflict: Essays on Development and Political Economics*, 2010

68. Meyersson, Erik *Religion, Politics and Development: Essays in Development and Political Economics*, 2010

69. Klingelhöfer, Jan *Models of Electoral Competition: Three Essays on Political Economics*, 2010

70. Perrotta, Maria Carmela *Aid, Education and Development*, 2010