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Abstract

In the first chapter of the dissertation, two administrative datasets from the Targeted Social Assistance Program (unconditional cash transfer) and National Assessment and Examination Center in Georgia are merged in order to investigate the impact of an unconditional cash transfer on the university enrollment rate in Georgia. Given that the program recipients were selected by virtue of being below a certain quantitative poverty threshold, this feature of the program is exploited to implement a global regression discontinuity. The study finds a positive impact of cash transfers on enrollment in tertiary education. Specifically, being a recipient of the social assistance program significantly increases a student's likelihood of enrollment, by 6.3%. More importantly, the findings suggest that the observed effect is gender specific: the impact is stronger for males. Male children of a beneficiary family have a 13.4% greater chance of being admitted to university. This marks the first attempt to study such a program in the context of education. The paper contributes to the growing literature on the long-run effects of cash transfers.

In the second chapter, the impact on a broad range of outcomes of the same social assistance program in Georgia is examined. An original household survey was developed and conducted in 2014, and a total of 340 households living in Tbilisi participated. A local regression discontinuity approach was employed to evaluate the unconditional cash transfer program in Georgia. In this study we found that receiving the transfer leads to a worsening in (self-reported) basic economic conditions, such as the ability to afford food. The recipients' worsening of economic conditions relative to the control is genuine, which begs the natural question: why? One possibility is that the program crowds out other sources of income or, alternatively, receiving the transfer could reduce incentives to work. Another possibility is that the recipients invest both the transfers and additional resources in investments in durable goods or human capital. This could then lead to a temporary lower ability to afford food (and similar patterns) in the time window of the survey.

The third chapter investigates how a unique education policy positively affected university enrollment rates of public school students in Georgia. In 2007, the Georgian government enacted legislation mandating the replacement of all public school principals under

the assumption that the replacement of the principals with random assignation of qualified candidates to public schools would decentralize and improve school governance across Georgia in a fair manner. About half the public school principals were actually replaced with new candidates and a majority of them were assigned through a random allocation mechanism. Therefore, the standard difference-in-differences methodology is used to compare treated public schools with private schools that are not affected by the policy, in order to identify how this reform impacted education outcomes. Using the National Assessment and Examination Center university admissions data, the public schools with replaced principals increased university enrollment more than the control schools, by an average of 4%. The largest part of this increase comes from schools with randomly assigned principals. The positive findings herein could impact education policy in developing (and perhaps developed) countries. The statistically significant and strong effects of this type of reform could cause a positive domino effect in the developing world, especially in countries with similar characteristics and predicaments in their education system.

Abstrakt

V první kapitole této dizertační práce slučujeme data z cíleného programu sociálních dávek (nepodmíněný transfer hotovosti) s daty z gruzínského národního a zkušebního centra, abychom analyzovali vliv nepodmíněného transferu hotovosti na míru zápisů na vysoké školy v Gruzii. Vzhledem k tomu, že výše zmíněný program poskytoval hotovostní transfer pouze jedincům, kteří byli pod určitou kvantitativně měřitelnou hladinou chudoby, používáme v této studii metodu globálního regression discontinuity designu. Nacházíme pozitivní vliv peněžních transferů na míru zápisů na vysoké školy. Být příjemcem transferu v rámci programu sociální podpory konkrétně zvyšuje pravděpodobnost zápisu na vysokou školu o 6,3%. Důležitým zjištěním také je, že výsledky ukazují, že vliv výše zmíněného programu je vyšší pro muže než pro ženy. Chlapci z chudých rodin, které dostávají peníze skrze program sociální podpory, mají o 13,4% vyšší šanci, že se dostanou na vysokou školu než chlapci pocházející z rodin, které transfer neobdržely. Tato analýza je prvním pokusem o zkoumání efektu podobného programu v rámci vzdělávacího systému a zároveň přispívá k rostoucí literatuře zabývající se dlouhodobým vlivem hotovostních tranferů.

Ve druhé kapitole zkoumáme vliv stejného gruzínského programu sociální podpory na širší spektrum proměnných. V roce 2014 bylo vyvinuto a zorganizováno originální dotazníkové šetření, kterého se zúčastnilo 340 domácností pocházejících z Tbilisi. Ve své analýze používáme metodu lokálního regression discontinuity designu, abychom zhodnotili výsledky nepodmíněného transferu peněžních prostředků v Gruzii. Výsledky studie ukazují, že obdržení peněžního transferu vede ke zhoršení (dle subjektivních výpovědí dotázaných o sobě samých) základních ekonomických podmínek, jako je například nedostatek prostředků ke koupi základních potravin. Horší ekonomické podmínky jedinců, kteří obdrželi transfer oproti kontrolní skupině, logicky vede k otázce: „Proč tomu tak je?“ Jedna z možností je to, že státní transfer vytlačuje alternativní zdroje obživy, jinými slovy lidé, kteří dostávají peníze od státu, mají menší motivaci pracovat. Další možností může být to, že příjemci transferů investují jak své původní, tak i nové prostředky do investičních statků dlouhodobé spotřeby nebo do lidského kapitálu. Toto může vést k dočasnému zhoršení jejich finanční situace a k následnému snížení jejich

schopnosti pořídit si základní potraviny, což je skutečnost objevující se v časovém rámci námi zkoumaného dotazníkového šetření.

Ve třetí kapitole se zabýváme tím, jak politická intervence v gruzínském vzdělávacím systému ovlivnila míru zápisů středoškolských studentů z veřejných škol na vysoké školy. Za účelem decentralizace a zlepšení spravování veřejných škol, vyhlásila gruzínská vláda v roce 2007 v celé zemi výběrová řízení na pozici ředitelů veřejných středních škol, přičemž nově zvolení ředitelé měli být více kvalifikovaní pro vedení těchto vzdělávacích institucí. Tento vládní zákrok vedl k odchodu asi poloviny původních ředitelů veřejných středních škol a na jejich místa nastoupili nově zvolení jedinci. Noví ředitelé byli rozděleni mezi střední školy většinou namátkově. Ke zjištění efektu této intervence používáme standardní metodu difference –in-differences, v níž porovnáváme ovlivněné veřejné střední školy se soukromými středními školami, na něž se intervence nevztahovala. Za použití dat z národního zkušebního a posuzovacího centra o počtu přijatých na vysoké školy zjišťujeme, že veřejné střední školy s novými řediteli mají v průměru o 4% vyšší míru zápisů žáků na vysoké školy než kontrolní skupina soukromých středních škol. Nejvyšší nárůst míry zápisů zaznamenaly školy s náhodně přidělenými řediteli. Námi nalezený pozitivní vliv této intervence by tedy mohl také ovlivnit politiky jiných rozvojových zemí (a snad i politiky rozvinutých zemí). Tak silný a statisticky významný efekt tohoto typu politické reformy by mohl vést k výraznému rozšíření podobných intervencí i v dalších rozvojových zemích, především pak v zemích s podobnými charakteristikami a s podobně žalostnou situací vzdělávacího systému.

Introduction

This dissertation is made up of three chapters within the economics of education branch of literature. The first chapter looks at how unconditional cash transfers from the Georgian government to the poor of the country affected university enrollment rates. Cash transfer programs are widely used as a tool to fight poverty. Most developing countries spend between 1% and 2% of GDP on cash transfers and international donors also invest substantially in cash transfer programs (Arnold, Conway, and Greenslade. 2011). Aid is crucial in terms of moving people out of this vicious cycle. A more skeptical view, though, is that cash transfers reduce people's incentives to solve their own problems and that cash transfer recipients may be tempted to engage in conspicuous consumption (alcohol, drugs, ceremonial activities, entertainment, etc.) instead of investing in education, health, and other areas with long-term benefits.

Ultimately, the effectiveness of cash transfer programs is an empirical question. In this paper, we study the impact of a cash transfer program in Georgia on enrollment in tertiary education. The program was introduced in 2005, and involved unconditional cash transfers to people living in extreme poverty. Program recipients were selected by virtue of being below a certain quantitative poverty threshold. We use this feature of the program to implement a regression discontinuity design (RDD) approach.

Studies of the impact of family income on teenage/child development and scholastic achievements generally face an income endogeneity problem. The previous literature mostly employs randomized experiments that offer very strong internal validity. However, they typically do not consider long-term outcomes and are based on relatively small sample sizes. Observational studies thus have a useful role to play in complementing evidence from field experiments. Using regression discontinuity design in program evaluation is still very rare (Duflo & Kremer, 2005; Ravallion, 2007). In order to separately identify the effects attributed to additional income from those of other unobserved characteristics, it is important to study the impact of exogenous variation in family income with a credible methodology such as the regression discontinuity design (Lee and Lemieux 2010).

The second chapter studies the same unconditional cash transfer program, but employs an ex-post, original survey that created new data for the analysis of the program's effects on the general wellbeing of the recipient households versus non-recipient households in Tbilisi. Cash transfers to the poor have become a cornerstone of social policy in developing countries. Following the success of Progresa/Oportunidadesa in Mexico and Bolsa Familia in Brazil, many countries have adopted similar schemes (Barrientos and Hulme 2009). Correspondingly, a large literature has developed to evaluate the impact of cash transfers on recipient outcomes (for a review see Arnold, Conway, and Greenslade. 2011).

Elementary microeconomics suggests that the extra income from the cash transfer program could affect their overall income in two directions. First, the extra money necessarily increases total income from the start and can be spent on direct consumption or into productive investments, such as household or small business production. Second, the extra money can reduce earned income through the income effect. Labor supply of the household could thus be reduced, as the household would not need to work as much to earn as much as they had before. Alternatively, the added income could cause a preference shift such that the household would want to work even more to have even greater consumption ability to accompany their increased social mobility. The distribution and level of spending would certainly be an outcome of the total income of the household, but it would likely remain the same if the household reduces their labor supply, or would increase in terms of consumption and/or investment if the household does not reduce their labor supply as much or even increases it. Increased consumption could take many forms according to utility preferences, but investment would most likely occur along the dimensions of durable goods, production capital, or human capital.

In this paper, an original household survey and a regression discontinuity approach are employed to evaluate a Targeted Social Assistance program (unconditional cash transfer) in Georgia. It is found that receiving the transfers leads to a worsening in (self-reported) basic economic conditions, such as the ability to afford food. A number of possible mechanisms that explain this counterintuitive result are discussed in the text, including crowding out from other sources of income as well as dynamic changes in behaviors and preference due to relaxed budget constraints. However, the results which may be driven by respondent misreporting, clearly remain puzzling.

The third chapter utilizes a unique education policy to analyze the impact of the mass replacement of school principals on student scholastic outcomes, in the form of university enrollment rates. The main objective of any school system is to improve student learning outcomes, cognitive skills, and socialization in society. In order to reach this objective and make schools more efficient, specific efforts are made by teachers, staff, and the principal school-wide. It is widely believed (Branch et al, 2012; Bloom et al., 2015 and Oduro et al, 2007) that the quality of the principal plays an important role in a school's organizational success, as well as significantly affecting student scholastic achievements.

Under the Georgian political initiative to decentralize school governance, the Ministry of Education and Science issued an order (N543) in July 2007, officially dismissing all public school principals and subsequently "randomly" assigning qualified candidates to public schools across the country, under the assumption that the replacement of the principals with randomly assigning qualified candidates to public schools would fairly decentralize and improve school governance across Georgia. About half of the public school principals were actually replaced with new candidates, a majority of whom were assigned through a random allocation mechanism.

Accordingly, this paper uses a standard difference-in-differences methodology to compare treated public schools with private schools that are not affected by the policy, in order to identify how this reform impacted education outcomes. Using the National Assessment and Examination Center university admissions data, it can be seen that the public schools with replaced principals increased university enrollment more than the control schools by an average of 4%. The largest part of this increase comes from schools with randomly assigned principals.

As the majority of schools are financed by the government in most countries (including Georgia), public finance efficacy makes it necessary to create and implement policies that ensure that the highest quality principals are selected (or assigned) to public schools. The positive findings herein could impact education policy in developing (and perhaps developed) countries and invites further research where applicable. The statistically significant and strong effects of this type of reform could cause a positive domino effect in the developing world, especially in countries with similar characteristics and predicaments in their education system.

Chapter 1: Education for the Poor

Co-authored by Lasha Lanchava

1.1 Introduction

Cash transfer programs are widely used as a tool to fight poverty. Most developing countries spend between 1% and 2% of GDP on cash transfers and international donors also invest substantially in cash transfer programs (Arnold, Conway, and Greenslade. 2011). The rationale for cash transfers is neatly summarized in Banerjee and Duflo (2011), who write that people become trapped in poverty due to geography and adversity. For those living barely above subsistence level, productivity is difficult without securing health and food provision, because they must focus most of their energy on subsistence items like food and shelter simply to survive.

Therefore, aid is crucial in terms of moving people out of this vicious cycle. A more skeptical view, though, is that cash transfers reduce people's incentives to solve their own problems and that cash transfer recipients may be tempted to engage in conspicuous consumption (alcohol, drugs, ceremonial activities, entertainment, etc.) instead of investing in education, health, and other areas with long-term benefits.

Ultimately, the effectiveness of cash transfer programs is an empirical question. In this paper, we study the impact of a cash transfer program in Georgia on enrollment in tertiary education. The program was introduced in 2005, and involved unconditional cash transfers to people living in extreme poverty. Program recipients were selected by virtue of being below a certain quantitative poverty threshold. We use this feature of the program to implement a regression discontinuity design (RDD) approach. RDD allows for the neutralization of greater economic effects and idiosyncratic characteristics that obscure causal inference through standard means analyses by concentrating on very similar households just around a given threshold.

We find a positive impact of cash transfers on admittance into tertiary education and we also find that being a recipient of the program significantly increases a student's likelihood of admittance, by 6.3%. Specifically, the proportion of admitted individuals out of the total number of applicants increased from 12.7% to 13.5%. Furthermore, we find that the observed effect is gender specific—the impact is stronger for males. On average, male children of beneficiary families are more likely to be admitted to university, by 13.3%, than male children of non-beneficiary families.

Our study contributes to the growing literature on the long-run effects of cash transfers, which has emerged as an important topic in development economics. Researchers have examined the effects of cash transfers on recipients' consumption patterns (Jensen & Miller, 2008; Attanasio & Mesnard, 2006), savings and investments (Gertler, Martinez, & Rubio-Codina, 2012), labor supply (Bertrand, Mullainathan, & Miller; 2003; Dabalen, Kilic, & Wane, 2008) and the effectiveness of the poverty alleviation programs (Coady, Grosh, and Hoddinott 2004).

Significant research has been devoted to the impact of cash transfers on education. De Brauw and Hoddinott (2011) study the effect of conditional cash transfers at school enrollment in Mexico. Using nearest neighborhood matching and fixed effects regressions, the study found that for those households who misperceived transfers as unconditional, school enrollment was significantly lower. Barrera-Osorio, Bertrand, Linden, and Perez-Calle (2008) use a randomized experiment and show that cash incentives increase school attendance and graduation rates. Alternatively, Dahl and Lochner (2008) identify a positive and significant effect (6% improvement in math and reading exams) of family income on scholastic achievements by exploiting exogenous variation of income for American families through the earned income tax credit program, which can be considered a form of unconditional cash transfer in relation to education outcomes.

Baird, McIntosh, and Özler (2011) performed a randomized control trial to evaluate the role of conditionality of cash transfers. They conclude that conditional cash transfers are more effective at reducing dropout rates and increasing scores in English reading tests. Oosterbeek, Ponce, and Schady (2008) evaluate the impact of cash transfer programs (aimed at increasing school attendance) on school enrollment in Ecuador, and find that for the poorest households the impact is positive, but the effect disappears for the households in the second quintile of the

income distribution. Fack and Grenet (2015) show that provision of need-based scholarships in France led to a 5% to 7% increase in university admittance.

With the exception of the last study, previous research focused on educational achievements during primary and secondary education, whereas our research evaluates the impact of cash transfers on enrollment in post-secondary education. Moreover, while most of the above studies have reported positive effects of conditional cash transfers on scholarly achievements, the Georgian transfers were entirely unconditional on anything other than household poverty. Furthermore, unlike previous cash transfer programs which targeted specific groups such as micro-entrepreneurs (Blattman, Fiala, & Martinez, 2014), orphans (The Kenya CT-OVC Evaluation Team 2012), pensioners (Duflo, 2003), and students (Barrera-Osorio, Bertrand, Linden, & Perez-Calle, 2008; Fack & Grenet, 2015), the Georgian cash transfer program was not directed towards any particular social or age group.

Finally, studies of the impact of family income on teenage/child development and scholastic achievements generally face an income endogeneity problem. The previous literature mostly employs randomized experiments that offer very strong internal validity. However, they typically do not consider long-term outcomes and are based on relatively small sample sizes. Observational studies thus have a useful role to play in complementing evidence from field experiments. Using regression discontinuity design in program evaluation is still very rare (Duflo & Kremer, 2005; Ravallion, 2007). In order to separately identify the effects attributed to additional income from those of other unobserved characteristics, it is important to study the impact of exogenous variation in family income with a credible methodology such as the regression discontinuity design (Lee and Lemieux 2010).

1.2 The Targeted Social Assistance Program in Georgia

The dataset on Georgia's poor households was obtained from the Social Service Agency (SSA) affiliated with the Ministry of Labor, Health and Social Assistance. The agency collects

national and regional data as a part of the system of means testing of households that apply for the targeted social assistance program (SAP). After a household applies, a trained interviewer employed by the SSA visits a household, inspects its living standards, interviews its members, and completes a special questionnaire. Then the agency processes the information obtained and assigns a corresponding poverty score to the household (based on a logarithmic sums methodology). The formula of the family score assessment combines all kinds of indices with different weights according to priority; see Formula 1.A.1 in appendix 1.A. Families with a poverty score below a 52,000 cut-off point were eligible for assistance from April 2005 until March 2008, when the cut-off point exogenously changed to 57,000.[1] By February 2010, more than half a million households (over 40% of Georgia's population) had applied and been assessed by the SSA. The amount of cash transferred monthly to the average household (composed of four members), is set proportionally to the average household's subsistence level. Table 1.A1 in appendix 1.A displays the average amounts of monthly transfers and subsistence levels over the 2005–2010 period in USD, adjusted by PPP. The average monthly transfer amount is 46 USD and the average subsistence level is 118 USD. Thus, financial aid comprises at least 39 percent of the subsistence level income and it comes with no tax obligations attached.

1.3 The Georgian University Admission System

University admittance data was acquired from the National Assessment and Examination Center (NAEC), affiliated with Georgia's Ministry of Education and Science, in order to link the SAP to an education outcome. The NAEC collects data on student admissions, annual entry examinations and scholarship allocations related to accredited universities in Georgia. Since the 2005 reform, recent secondary school graduates who wish to enter university take mandatory exams (standardized tests) in general skills, Georgian language, a foreign language, and in a

[1] The cut-off point thereafter remained at 57,000 points throughout the remainder of the program.

fourth subject corresponding to the student's specialization. According to UNESCO data[2] (see table 1.A2 in appendix 1.A), Georgia, relative to other countries in transition, enjoyed high enrollment rates in the late 90s. Admittance peaked in 2005 as the gross enrollment ratio reached about 47%. However, university attendance fell rapidly in subsequent years and in 2010 only 28% of the university-aged population was enrolled in higher education. In the households designated as 'poor' in our data, the enrollment rates average only 12.7%, far below the national rates. It is generally accepted that the major cause of the sudden and significant decrease in the university enrollment ratio in Georgia was a direct effect of the university accreditation process, which started in 2005, and imposed a lowered number of available seats at Georgian universities.

In 2005, Georgia introduced a centralized university admissions model that uses the NAEC applicant test scores as the sole criterion by which students may gain admission to the strictly limited number of allocated University seats (both public and private), the number of which is set by the government through institutional accreditation regulations. Moreover, the NAEC entrance exams are also the sole criterion for tuition grant allocation. For both admissions and scholarships, this model removes all personal and school-level achievements as well as demographic considerations from the selection criteria. Furthermore, only a small amount of public finances were allocated towards tuition grants throughout the period of this study. For example, in 2005 – 2009, only 40% of newly admitted students received any public funding whatsoever and only 9% received a scholarship that covered their full tuition costs (NAEC, 2009a). Notably, private or NGO scholarships in Georgia are and were nearly non-existent.

Since this model of admissions stringently covers all publically available financial assistance for university students, then the only major difference, between public and private university tuitions are rates. For example, in 2009, whereas public universities were not allowed to charge more than $1350 per year, one of the most prestigious private universities charged $8900. Ministry of Education (2009a) data shows that in 2007 – 2008, 72% of tertiary education students were enrolled at public Universities. The World Bank (2010) reports that the overall mean tuition for Georgian universities in 2006 – 2009 was about $1180. Therefore, the SSA targeted social assistance program, with an average unconditional monthly payout of $46 ($552

[2] The statistics we report are a gross enrollment ratio, which is a share of enrolled students of the total number of people at the university age (18-23).

annually; which constitutes nearly half of the mean tuition), could considerably relax credit constraints for tertiary education in Georgia.

1.4 Combined Dataset

In order to identify whether or not and how much the unconditional cash transfers may have affected university enrollment amongst the poor who received the transfers, two main datasets were combined. The cash-transfer theoretical framework implies a couple main sub-channels of how cash transfers may affect recipient enrollment in higher education. First, the higher budget could result in increased subsistence, spending on education, and increased (time and capital) investment into human capital accumulation, which could benefit children of all ages and could lead them to enrollment in tertiary education. Second, the direct relaxation of credit constraints may increase the ability of recent graduates of secondary school to be able to afford the costs associated with higher education. The latter is the case being investigated herein.

Based on the identifying characteristics (Surname, Name, and Birth Date) of common observations, the following two datasets were merged to obtain a conjoined cross-sectional sample of candidates for university applications from 2007 to 2013. The datasets were merged such that the treatment and control groups are well defined around the change from the initial poverty cut-off point (52000) to the increased poverty cut-off point (57000).

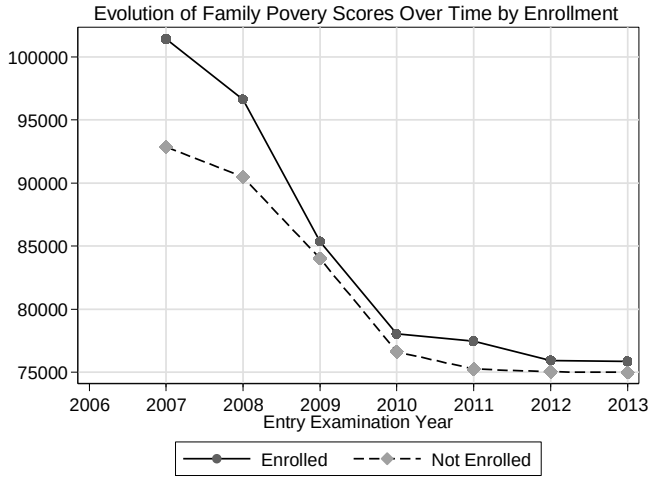
The first dataset (hereinafter referred to as "pre-modification") is made up of observations about university applicants that come from families assessed by the SSA before the threshold modification in March, 2008. In contrast, the second dataset (hereinafter referred to as "post-modification") includes applicants from those families that were assessed after the threshold modification. To evaluate the program's effect, the treatment group consists of university students that began their studies after at least one year after their families began receiving the cash transfers. Table 1.A3 in appendix 1.A demonstrates the quantitative distribution of applicants from SSA families. Numbers in bold refer to those candidates whose families were assessed by the SSA at least one year before the university entry examination. The remaining numbers refer to placebo candidates who took the university entry examination before the SSA assessed their families and assigned scores.

1.5 Methodology and results

Several interesting facts emerge from an initial inspection of the data. First, we see that overall, enrolled students come from wealthier families and the difference is strongly statistically significant (Average family poverty scores of enrolled and no-enrolled students are around 80000 and 78000 respectively).[3]

However, when poverty scores of enrolled and non-enrolled students are plotted over time, there is a significant drop in average poverty from 2007 to 2009 followed by a consistently small gap throughout 2013 (see Figure 1.1), meaning that relatively more applicants from poorer families were able to enroll in universities.

Figure 1.1 Average family poverty score by student enrollment status over time



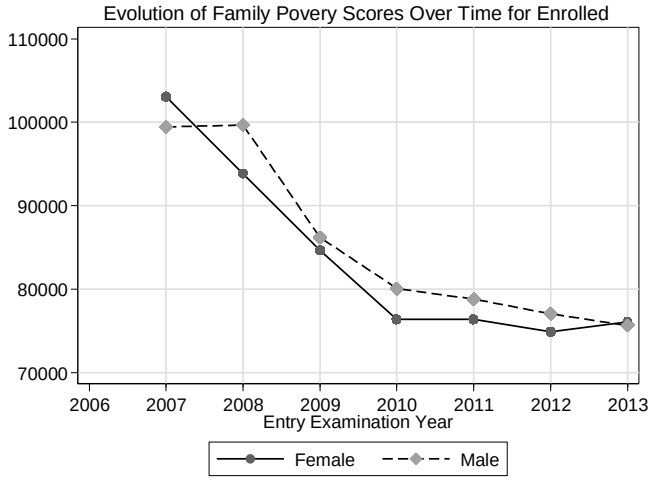
This evidence suggests that two years after its introduction, the SSA made higher education relatively more affordable for students from poor households. Moreover, throughout the timeframe of this study, there were no other significant changes to tertiary education conditions for students, their families, or the associated communities in Georgia from 2005, when the national tertiary education admission system underwent a policy change.

We also disaggregate poverty score time series by gender and enrollment status. Figure 1.2 shows that there was initially a large decline in average household poverty scores for enrolled

[3] The higher the poverty score, the wealthier the household.

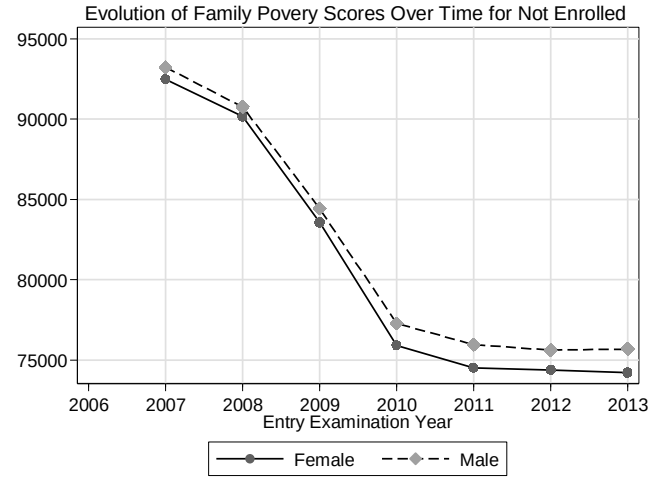
females, which leveled off in 2009, while the declining trend in household poverty scores for enrolled males occurred more steadily.

Figure 1.2 Average family poverty score by enrolled student gender over time



Interestingly, according to Figure 1.3, the decline in average household poverty scores for non-enrolled females was equally dramatic, while the decrease in household poverty scores for non-enrolled males was less significant—an indication of a gender specific effect of cash transfers on university enrollment.

Figure 1.3 Average family poverty score by not enrolled student gender over time



These findings call for further investigation of a causal impact of SAP on students' chances of university enrollment using a regression discontinuity methodology.

An assessment of the causal inference (average treatment effect) of the social assistance program on university enrollment can be achieved using a parametric regression discontinuity design (polynomial regression, so-called global strategy estimation) because the density of the assignment variable—university enrollment—is discontinuous, while the covariates are not statistically different close to the threshold. To implement RDD analysis on the conjoined dataset, we first go through an inspection of covariates around the cut-off point. Covariates such as gender, age, and number of siblings do not seem to be statistically different in the 5000- and 1000-bin widths around the thresholds for neither the pre-modification dataset nor the post-modification dataset.

Sample of families with an SSA visit before March, 2008 (pre-modification dataset) with a 5000-bin bandwidth around the threshold.

	5000 poverty score around the cut-off, sample size = 8709=(4893+13816)			
Covariates	Treatment group	Control group	Difference	t statistics
Family size	4.69	4.99	-0.31	-10.71***
Number of siblings	1.61	1.62	-0.01	-0.26
Age	15.32	15.28	0.04	1.05
Male	0.51	0.50	0.01	0.20

Sample of families with an SSA visit before March, 2008 (pre-modification dataset) with a 1000-bin bandwidth around the threshold.

	1000 poverty score around the cut-off, sample size = 3560=(922+2638)			
Covariates	Treatment group	Control group	Difference	t statistics
Family size	4.65	5.06	-0.41	-6.25***
Number of siblings	1.58	1.61	-0.03	-1.09
Age	15.25	15.22	0.03	0.38
Male	0.47	0.50	-0.03	-1.45

Sample of families with an SSA visit after March, 2008 (post-modification dataset) with a 5000-bin bandwidth around the threshold.

	5000 poverty score around the cut-off, sample size = 5783=(3262+ 2521)			
Covariates	Treatment group	Control group	Difference	t statistics
Family size	4.64	4.49	0.15	3.60***
Number of siblings	1.73	1.68	0.06	3.00***
Age	13.15	13.19	-0.09	-1.57
Male	0.53	0.53	0.53	0.01

Sample of families with an SSA visit after March, 2008 (post-modification dataset) with a 1000-bin bandwidth around the threshold.

	1000 poverty score around the cut-off, sample size = 1158=(647+511)			
Covariates	Treatment group	Control group	Difference	t statistics

Family size	4.43	4.48	-0.05	-0.60
Number of siblings	1.66	1.59	0.07	1.80*
Age	13.17	13.08	0.09	0.70
Male	0.53	0.56	-0.03	-0.88

As the basic means comparison test (t-test) suggested, only household size (either total family size or number of siblings) amongst the covariates is systematically different over the treatment status between the pre- and post-modification samples. Moreover, when the RDD regression with all covariates as dependent variables is run, the results show that the household size is significantly greater for the control group, in the amount of 0.1 and 0.04 larger families for the pre- and post-modification data sets. F statistics are reported in appendix B. In addition, we have constructed outcome, rating, and covariate variable graphs for both samples. There is a clear sign of discontinuity in the case of average admissions and continuity amongst the covariates; see appendix 1.A, Figures 1.A.1–1.A6.

According to Figure 1.A3, we observe the discontinuity in the density of the rating variable at the threshold (57,000) for the pre-modification dataset. We do not have an explanation why this might be the case. Therefore, it could be argued that the allocation of cash transfers may not have been random and this may be a limitation of the study. However, this particular feature of the data does not drive our results. This is because according to Figure 1.A.6, the density of the rating variable is continuous at the cutoff point (52000) for the post-modification dataset and has a positive and significant effect (larger in size relative to the effect from the pre-modification dataset) of cash transfers on the enrollment. Moreover, there is a discontinuity in the density of the family score variable around 57000 in the pre-modification dataset. That is, the discontinuity around 57000 was present before 2008 when the cutoff point was 52000, and therefore this might be a particular characteristic of the data unrelated to the allocation rule of the cash transfers to the recipients. Furthermore, a difference-in-differences RDD did not show any significant result for either the full sample or just for males, which means that there is no statistically significant difference between the effects of the program for pre- and post-modification data. Therefore, the standard RDD approach used below is neither conflating nor concealing any natural effect phenomenon occurring at the 57000 point threshold and it provides more efficient point estimates (similar effects with lower standard errors) than the DiD RDD methodology.

The polynomial model, where the framework includes the entire dataset in the analysis, is as follows:

$$e_i = \alpha + \beta p_i + \gamma P^{(n)}(s_i - T) + \delta P^{(n)}(s_i - T)p_i + \theta X_i + \epsilon_i, \qquad (1)$$

where the binary outcome variable e_i stands for university enrollment, while the dummy variable p_i is one if the family is a program recipient and zero otherwise. Other explanatory parts are nth order polynomials of the distance between the poverty score (s_i) and the threshold (T) as well as for the interaction term, and X_i are the relevant individual covariates.

In order to specify the model or degree of the polynomial terms correctly using a parametric model, we go through a three-step procedure separately for both datasets. The first step is a visual inspection of the average outcome values over the poverty score variable and a formal test of the selection of an appropriate bin width. After choosing the optimal bin sizes, the second step is to identify the polynomial degrees. We use the methodology of Lee and Lemieux (2010) for the model selection criterion. Finally, we conduct a sensitivity analysis (robustness checks) that shows that the treatment estimates do not vary much after the outermost observations are dropped, when we iteratively censor both tails of the distribution of the data at 1%, 5%, and 10%.

By dividing intervals into equal sub-intervals up to the point when the next step brings no explanatory power to the outcome variable, the resulting most appropriate bin size is a poverty score of 500. Specifically, the corresponding F-statistic is no longer statistically significant at the 95% confidence level; see table 1.B1 in appendix 1.B. Table 1.B2 in appendix 1.B illustrates the model selection criterion. Comparing a linear model to higher order polynomial specifications for the pre-modification dataset, F-test values suggest that a first order (linear) model with interaction terms and covariates fits best from amongst all options; see table 1.B2 in appendix 1.B. Finally, we perform a sensitivity analysis where we show that our model is not sensitive to the dropping of the outermost points from the data and the results are stable across all possible sub-samples; see table 1.B3 in appendix 1.B.

Based on the three-step procedure above, it was decided that the pre-modification model's specification would be a parametric model with first-degree polynomial terms with interactions and covariates. Our main findings, shown in Table 1.1 below, clearly suggest that being a member of a beneficiary family significantly increases chances of enrollment in university, by an average of 0.8 percentage points. Since the sample mean of enrollment in our

sample is 12.7%, the effect size of cash transfers on university enrollment is 6.3% (0.8%/12.7%). Furthermore, the gender-based interaction term (column 2b, labeled "Gender gap") indicates that males have a 13.4% (1.7%/12.7%) higher probability of university admission compared to females.

Table 1.1

The Impact of the Social Assistance Program on University Enrollment First–degree, Polynomial Regressions: Pre-Modification Dataset

	Sample 1				Sample 2		Sample 3	
Enrollment to university	(1)	(2)	(2a)	(2b)	(3)	(4)	(5)	(6)
	Full sample	Males Only	Females only	Gender gap	Oldest	Oldest males	City	City, males
Program recipient	.008**	.017***	-.005	-.003	.0073	.015*	.011	.024
	(.004)	(.006)	(.007)	(.005)	(.006)	(.008)	(.016)	(.021)
Interaction term * Male dummy	-	-	-	.015*	-	-	-	-
	-	-	-	(.005)	-	-	-	-
Mean	.127	.115	.141	.127	.126	.115	.125	.129
# observations	61150	31183	29967	61150	38217	19393	6924	3574
R^2	0.0021	0.0008	0.0014	0.0034	0.0017	0.0010	0.0052	0.0025

Notes: Coefficients in all columns are OLS regression estimates, robust standard errors are in parentheses; ***, **, and * indicates significance at 5%, 10% and 1% level, respectively. Samples 1, 2 and 3 are households (candidate applicants) with the entry examination at least one year later than the family assessment period. The second sample focuses on large families (more than 3 members) and the third subsample considers only households located in the capital city of Georgia. Furthermore, cohort and entry-year fixed effects and covariates (household size and gender) are considered in the regressions. Interaction term is a multiplication of male and beneficiary indicator variables.

A similar analysis was performed for the post-modification dataset. Based on the three–step procedure (see tables 1.B4, 1.B5 and 1.B6 in appendix 1.B), it was decided that the model's specification will be a parametric model with second–degree[4] polynomial (quadratic) terms with interactions and covariates. According to Table 1.2, the effect size in this case is 11% (1.4%/12.7%) and 18.1% (2.3%/12.7%) for males.

[4] The model specification for the post-modification dataset is different from the model specification for the pre-modification dataset. This is because model specifications in each case are optimized through the three-step procedure Lee and Lemieux (2010). Moreover, the pre-modification and post-modification datasets are not identical as they are distant in time and Lee and Lemieux (2010). Moreover, pre-modification and post-modification datasets are not identical as they are distinct in time and have different cutoff points for poverty score and these may, in addition, be factors that lead to different model specifications for each dataset.

Table 1.2

The Impact of the Social Assistance Program on University Enrollment Second–Degree Polynomial Regressions: Post-Modification Dataset

	Sample 1				Sample 2		Sample 3	
Enrollment to university	(1)	(2)	(2a)	(2b)	(3)	(4)	(5)	(6)
	Full sample	Males Only	Females only	Gender gap	Oldest	Oldest males	City	City, males
Program recipient	.014*	.023*	.016	.008	.021**	.022*	-.017	-.012
	(.007)	(.011)	(.013)	(.012)	(.009)	(.012)	(.021)	(.029)
Interaction term	-	-	-	.004	-	-	-	-
	-	-	-	(.006)	-	-	-	-
Mean	.117	.127	.128	.117	.115	.106	.141	.136
# observations	71132	34378	36754	71132	50129	25960	11286	5802
R^2	0.0025	0.0027	0.0036	0.004	0.0027	0.0057	0.0049	0.0077

Notes: Coefficients in all columns are OLS regression estimates, robust standard errors are in parentheses; **, and * indicates significance at 5%, and 10% level, respectively. Sample definitions are the same as in the previous table. Samples 1, 2 and 3 are households (candidate applicants) with the entry examination at least one year later than the family assessment period. The second sample focuses on large families (more than 3 members) and the third subsample considers only households located in the capital city of Georgia. Furthermore, cohort and entry-year fixed effects and covariates (household size and gender) are considered in the regressions. Interaction term is a multiplication of male and beneficiary indicator variables.

Thus far, it has been shown that there is a statistically significant effect from the unconditional cash transfer program in Georgia on university enrollment. A placebo test, which examines the effects at the cut-off in the year before the social assistance program started, was run in order to test the robustness of the validity of the regression outcomes. For both data sets there is no effect of the program on university enrollment. In particular, we obtain negative coefficient estimates (-0.01% and -2.1%) and they are not statistically significant. Therefore, the results are, in this way, confirmed as robust.

In light of the recently published study by Fack & Grenet, (2015) that reports up to a 7% increase in university enrollment as a result of 1500 Euro need-based scholarships allocated to potential students in France, the effects of the Georgian cash assistance program are particularly notable. First of all, unlike in France, cash transfers in Georgia were unconditional. Second, the amount of cash transfers to Georgian households, which averaged 46 USD and never exceeded 100 USD for the average beneficiary family, is minuscule relative to the need-based scholarships granted to students in France. Even when multiplying the figure by the average differences in

PPP (2.45) and median income (9.22 times higher in France) between the two countries for the 2007–2013 period, the average Georgian cash transfer amounts to 1039 USD for an entire household. Arguably, the Georgian cash transfer was thus considerably more effective compared to the 1500 EUR (or about 1875 USD during that period) per French student.[5]

1.6 Heterogeneity Analysis

In this section, we extend our analysis and explore whether and how a family's composition moderates the observed effect. First of all, we are interested in whether the observed effect is gender specific. Gender preferences have been significantly and positively linked with education spending on the children of the preferred gender in Korea (Choi & Hwang, 2015) and the US (Behrman, Rosenzweig, & Taubman, 1996). Moreover, even the number of years, focus, and attainment of education has been associated with gender preference (Choi and Hwang, 2015 and Behrman, Rosenzweig, & Taubman, 1996). In line with these results, our findings show that cash transfers significantly increase the odds of university enrollment for males. According to Table 1.1 above, in the column males only, the program's effect for males against the average is an increase of 13.4% (18.1% in the post-modification dataset; from Table 1.2). This may echo reported gender specific preferences (biased towards males) of parents in South Caucasian countries (King, Guo, McKee, Richardson, and Stuckler, 2013). In consequence, while cash transfers increase overall university enrollment rates in Georgia, they may also widen the gender gap in education.

Further, there is strong evidence in other areas of economic research that shows how birth order affects child outcomes. For example, Devereux, Salvanes, and Black (2005) find a strong and significant negative effect of birth order on children's education, employment, wages, and even teenage childbearing. Dohmen, Falk, Huffman and Sunde (2011) investigate the intergenerational transmission of risk and trust attitudes as a result of parental socialization

[5] Sourced from the OECD website (oecd.org) in 2016.

efforts. They find that first–born children are usually more similar to their parents in terms of risk and trust preferences. To explain the findings, the authors maintain that socialization is a result of parental effort, which seems to be stronger for oldest children. In line with the literature noted, we observe that the impact of cash transfers on university enrollment is stronger for the oldest children in a family; see column 3 in the Tables above. This finding is also a direct implication of the quantity-quality tradeoff paradigm formulated by Becker (Becker & Lewis, 1973). Alternatively, it is consistent with a family participation model in which parental investments in older children not only pays back to the parents later in life, but already includes contributions towards the households in young adulthood, including towards the education costs of younger siblings. Column 4 in the Table above shows that this birth order effect is even stronger when the oldest child is male, consistent with the above-mentioned observations.

Finally, we check whether the effect differs across the geographical locations of the program's recipients. One might argue that the program is more likely to increase the chance of enrollment for those students who live in the capital city of Georgia (Tbilisi) and has less impact on university enrollees in the regions. Surprisingly, the Tbilisi coefficient has a negative sign, although it is not statistically significant. Still, this combined with the following reported findings may suggest that university education is costlier for students from the more rural regions.

Evidently, one factor distorting educational choices is distance (Griffith & Rothstein, 2009). The applicants most likely to be deterred from applying to high-ranking universities by the distance factor are low income (Turley, 2009) and those from rural areas (Chanqseliani, 2013). The distance factor in the Georgian context is reinforced by the fact that universities do not offer student accommodation or support for living expenses, and financing student life per academic year in Tbilisi would cost an average rural adult three years of income (Chanqseliani, 2013). As a result, according to Chanqseliani (2013), rural applicants are 12 times less likely to apply to prestigious universities, most of which are located in Tbilisi.[6] Therefore, allocation of regional talent is biased towards less prestigious and peripheral universities.

[6]According to Chanqseliani (2013), the ranking of the universities is based on the average United National Examination scores of the student cohort. According to this measure of university quality, 100% of the highest, second highest and medium quality universities are located in Tbilisi. 65% of the lowest quality universities are located outside of Tbilisi.

1.7 Discussion and Conclusion

This paper investigates the impact of unconditional cash transfers in Georgia on university enrollment. The program selects recipients based upon a quantitative poverty threshold, which gives us the ability to implement a regression discontinuity approach. We use the data on program recipients from the SSA and on university admissions from the NAEC and combine these into a single dataset. First of all, we observe that the enrollment rate in the sample of poorest Georgian households is very low relative to the national average. We find that being a recipient in the program significantly increases a student's likelihood of university enrollment, by 6.3%. In comparison, Fack and Grenet (2015) report up to a 7% increase in university enrollment as a result of 1500 Euro need-based scholarships allocated to potential university students in France. The large effects of cash transfers on enrollment rates in Georgia are particularly notable. First of all, unlike in France, cash transfers in Georgia were unconditional. Second, the amount of cash transfers to Georgian households, which averaged 46 US dollars for an average family, was notably smaller relative to the 1500 Euro scholarships in France, even when adjusting for PPP and median income differences.

If unconditional transfers have such a strong impact on university enrollment by poor students, then the Georgian government may want to consider further complementary approaches to nudge the poor to invest in skills and education; which may help break the poverty cycle. Furthermore, politicians might also opt for *conditional* transfer programs, such as need-based university scholarships that could encourage students from poor family backgrounds to continue their education. Such measures could reduce the pressure to leave the educational system and start working early with low education levels and correspondingly low productivity and income levels. In fact, such conditional programs could have an even greater positive effect upon education outcomes than the unconditional transfers.

We also find a gender specific effect. While cash transfers increase overall university enrollment rates in Georgia, the effect for males is much stronger than the average effect (13.4% vs. 6.3% in the pre-modification findings and 18.1% vs. 11% in the post-modification findings).

Our findings echo previously reported gender specific effects of education (Choi and Hwang, 2015 and Behrman, Rosenzweig, & Taubman, 1996). We also observe that the impact of cash transfers on university enrollment is stronger for the oldest children in a family. This finding is in line with the quantity-quality tradeoff paradigm formulated by Becker (Becker & Lewis, 1973).

Finally, as noted, the negative coefficient on Tbilisi may be an indication that cash transfers most effectively help students from rural regions, as the costs of higher education are greater for these applicants. Previous literature is rich with examples of when the sorting of students and universities according to prestige considerations has a very significant effect on educational outcomes, occupations, earnings, and, consequently, social mobility (Behrman, Rosenzweig, & Taubman, 1996; Brewer, Eide, & Ehrenberg, 1999; Carnevale& Rose, 2003; Li, Meng, Shi, & Wu, 2012). The misallocation of regional talent may in turn adversely impact the overall quality of education and heighten the current skills mismatch in the labor sector[7] in Georgia. Effectively, this would likely lessen the productivity of workers and ultimately generate some degree of welfare loss. Therefore, this study should encourage policy aimed at increasing education outcomes, taking into account gender and location heterogeneity.

[7]World Economic Forum's Executive Opinion Survey, 2012.

1.8 References

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1.A Appendix

Formula 1.A1: The Family Score Assessment methodology is based on the logarithmic sums principle that considers different weights according to the priority. $K_{i,j}$refers to the weights and $Y_{i,j}$ to survey responses.

Poverty score *(made up of variables below)*	
Welfare index	$\mathrm{I} = \frac{\mathrm{C}}{\mathrm{N}}$
Household consumption index	$C = \exp(K_0 + \sum_{i=1}^{10} C_i) - pC_0$
Agricultural index (land)	$C_1 = \sum_{i=1}^{11} K_{1,i} \ln(1 + Y_{1,i})$
Agricultural index (livestock)	$C_2 = \sum_{i=1}^{4} K_{2,i} \ln(1 + Y_{2,i})$
Non-agricultural index	$C_3 = \sum_{i=1}^{8} K_{3,i} \ln(1 + Y_{3,i}) + \sum_{i=9}^{12} K_{3,i} Y_{3,i}$
Income index	$C_4 = K_{4,1} \ln(1 + Y_{4,1})$
Demographic index	$C_5 = K_{5,1} \ln(1 + Y_{5,1})$
Education and skills index	$C_7 = \sum_{i=1}^{2} K_{7,i} Y_{7,i}$
Territorial index	$C_8 = \sum_{i=1}^{10} K_{8,i} Y_{8,i}$
Interviewer index	$C_9 = \sum_{i=1}^{10} K_{9,1,i} Y_{9,1,i} + \sum_{i=1}^{4} K_{9,2,i} Y_{9,2,i} + \sum_{i=1}^{4} K_{9,3,i} Y_{9,}$
Other possessions index	$C_{10} = \sum_{i=1}^{2} K_{10,i} Y_{10,i}$
Family adult members index	$E = \sum_{i=1}^{n} e_i$
Household necessity index	$N = \frac{E}{n^{\beta}} \cdot B$

Table 1.A1: Amount of cash transferred monthly to the average household in PPP adjusted USD. All other values are calculated per month.

Year	Fixed payment	Marginal payment	4-member family cash transfer	4-member family GDP	Average family's subsistence level
2005	16.5	6.6	36.4	501.8	88.7
2006	16.9	6.8	37.1	774.6	100.6
2007	18	7.2	39.5	960.6	119.1

2008	20.1	8.1	44.3	973.7	144
2009	18	14.4	61.1	818.4	129.3
2010	16.8	13.5	57.2	874.3	126.4
2011	17.8	14.2	60.5	1076.9	156.9
2012	18.2	14.5	61.8	1174.5	153.5

Table 1.A2: Gross enrollment rates for countries in transition.

GROSS ENROLLMENT RATIO, TERTIARY, BOTH SEXES (%): 1999-2010								
		1999-2005			2005-2010			1999-2010
	Country	% enrollment In 1999	% enrollment In 2005	**% change**	Country	% enrollment In 2010	**% change**	**% Change**
Low enrollment level in 1999(below30%)	Romania	21.63	44.90	107.59	Romania	56.77	26.44	162.48
	Kazakhstan	24.93	52.92	112.24	Kazakhstan	39.49	-25.37	58.38
	Czech Rep.	25.56	48.90	91.32	Czech Rep.	63.21	29.24	147.27
	Macedonia	21.77	29.63	36.07	Macedonia	37.07	25.12	70.26
	Mongolia	26.91	44.65	65.93	Mongolia	53.81	20.49	99.95
	Slovakia	25.94	40.39	55.66	Slovakia	55.99	38.61	115.78
	Kyrgyzstan	29.16	42.53	45.82	Kyrgyzstan	42.13	-0.94	44.44
	Tajikistan	17.44	20.96	20.14	Tajikistan	22.69	8.27	30.08
	Armenia	34.62	38.36	10.82	Armenia	50.62	31.94	46.22
	Uzbekistan	12.50	9.85	-20.88	Uzbekistan	9.94*	0.91	-20.48
	Azerbaijan	15.72	14.45	-8.07	Azerbaijan	19.26	33	22
High enrollment level in 1999(above30%)	Hungary	32.49	65.10	100.33	Hungary	60.37	-7.26	85.78
	Lithuania	44.01	77.50	76.10	Lithuania	80.75	4.18	83.47
	Slovenia	52.35	79.70	34.31	Slovenia	88.46	10.99	68.97
	Latvia	50.90	78.85	54.90	Latvia	70.55	-10.53	38.59
	Croatia	30.55	44.53	45.74	Croatia	55.83	25.37	82.73
	Ukraine	47.10	68.66	45.78	Ukraine	76.65	11.63	62.74
	Poland	45.43	63.60	39.97	Poland	73.52	15.59	61.80
	Russia	51.44	72.59	41.09	Russia	75.89	4.54	47.53
	Estonia	51.12	68.44	33.89	Estonia	71.65	4.68	40.16
	Georgia	35.70	46.60	30.51	Georgia	28.26	-39.34	-20.84
	Belarus	52.11	66.16	26.96	Belarus	78.99	19.38	51.56
	Moldova	32.69	36.09	10.40	Moldova	38.14	5.67	16.67
	Bulgaria	45.20	44.27	-2.05	Bulgaria	57.99	30.99	28.29

Table 1.A3: Quantitative distribution of candidate applicants (ready for higher education) from SSA families, where numbers in bold refer to those candidates whose families were assessed before the entry examination year

Threshold	Family assessment year (by SSA)	University entry Examination Year							Total
		2007	2008	2009	2010	2011	2012	2013	
T=52k	2005	**842**	**943**	**746**	**658**	**584**	**527**	**531**	4831
	2006	**5582**	**6051**	**5,599**	**4904**	**4564**	**4579**	**4476**	35755
	2007	4036	**4418**	**4425**	**4105**	**3865**	**3924**	**3875**	28648
T=57k	2008	6803	6460	**5863**	**5735**	**5727**	**6024**	**5771**	42383
	2009	10497	11081	9668	**8778**	**9259**	**9346**	**9530**	68159
	2010	718	753	703	644	**602**	**608**	**565**	4593
	Total	28478	29706	27004	24824	24601	25008	24748	184369
	Enrollment								
	no	25165	26913	23687	22109	21564	21708	21318	162464
	yes	3313	2793	3317	2715	3037	3300	3430	21905
	% enrollment	12%	9%	12%	11%	12%	13%	14%	12%
	NAEC	15599	14159	25153	19749	23204	24495	27097	149456
	% share in NAEC	21%	20%	13%	14%	13%	13%	13%	15%

Figure 1.A1: (Pre-Modification Dataset) – Distribution of the covariates (Family size, age, gender).

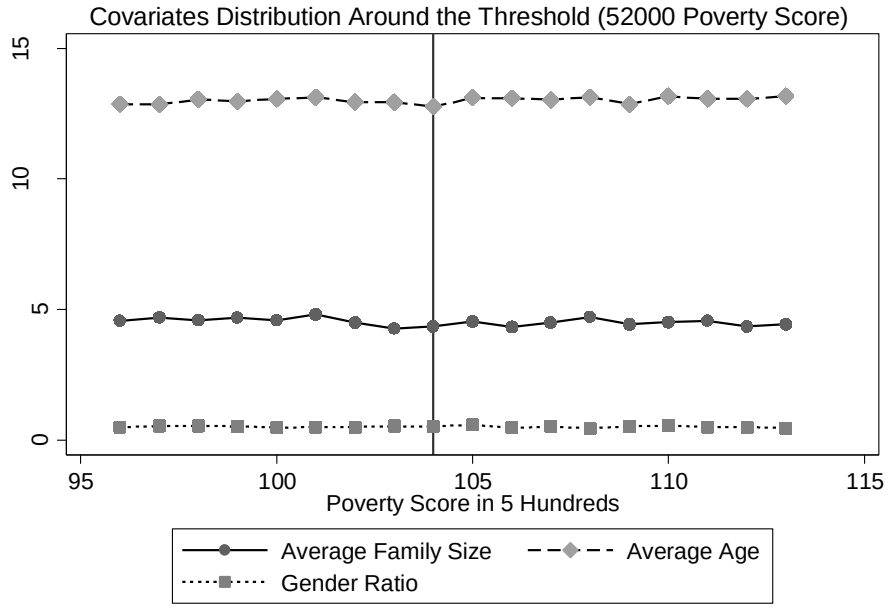


Figure 1.A2: (Pre-Modification Dataset) – Average enrollment rate across bins.

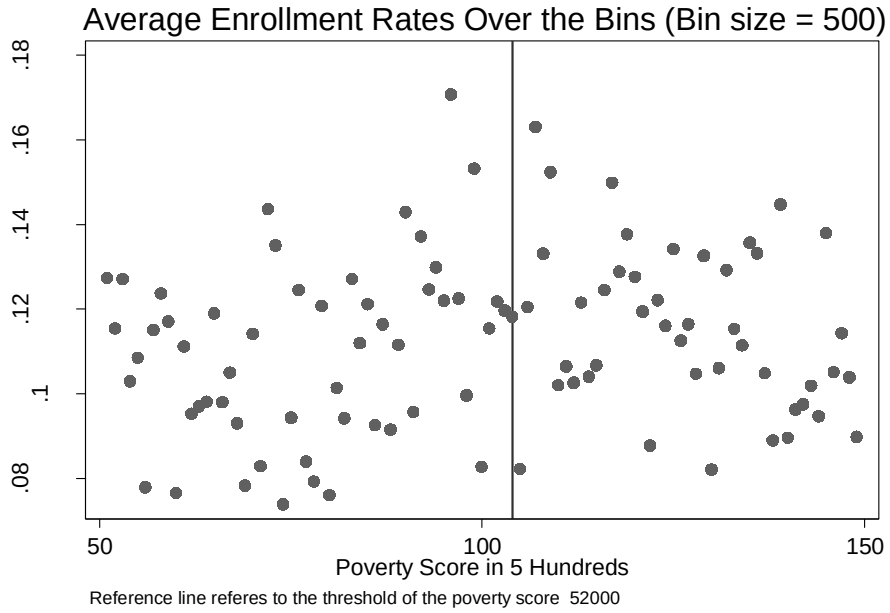


Figure 1.A3: (Pre-Modification Dataset) – Density of rating variable.

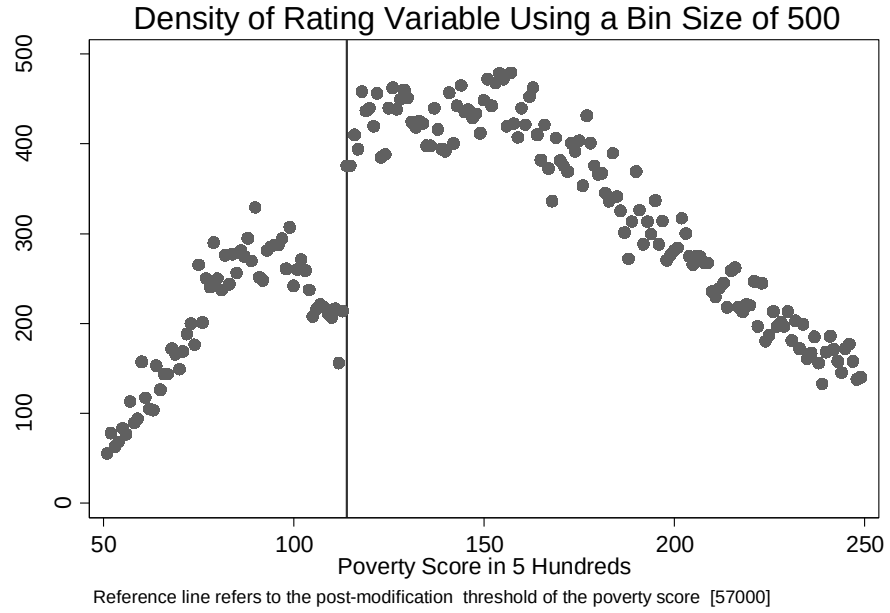


Figure 1.A4: (Post-Modification Dataset) – Distribution of the covariates (Family size, age, gender).

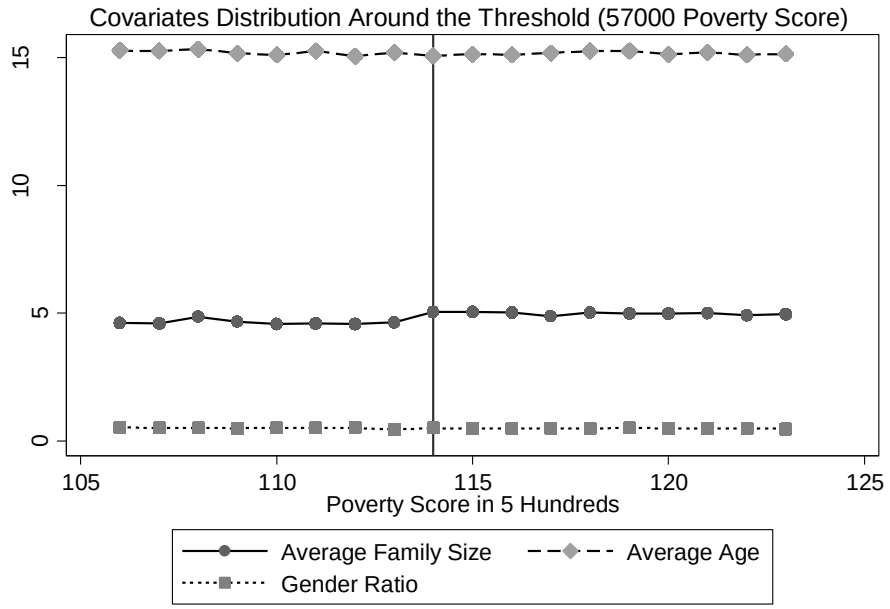


Figure 1.A5: (Post-Modification Dataset) – Average enrollment rate across bins.

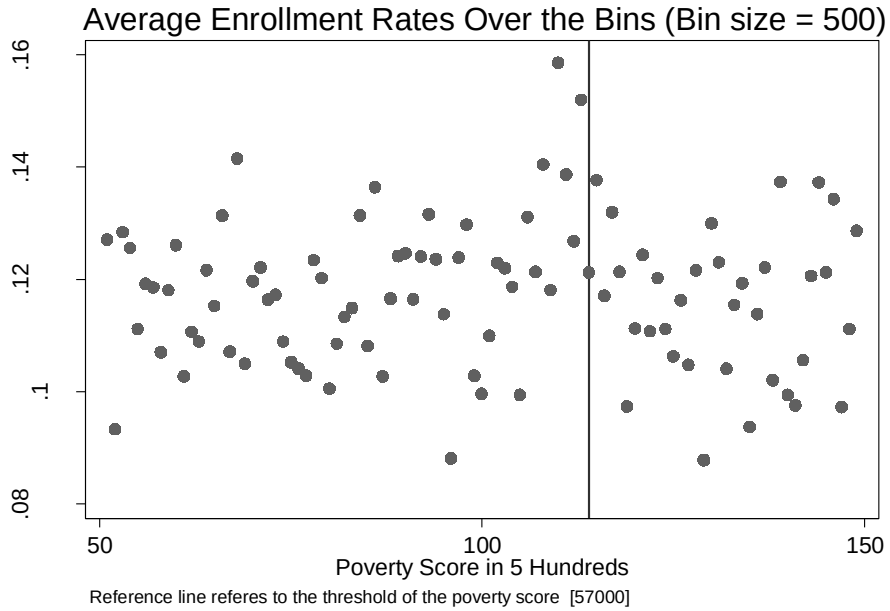
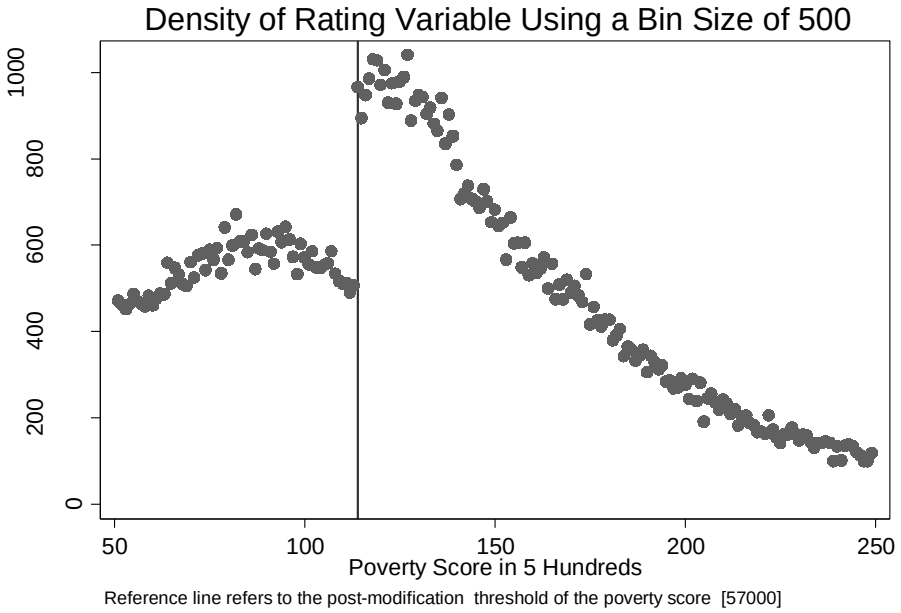


Figure 1.A6: (Post-Modification Dataset) – Density of rating variable.



1.B Appendix

Table 1.B1
Step 1 – Bin size selection criteria, F–test

Bin size	Restricted R^2	Unrestricted R^2	# of bins	Observations	F value
10000	0.0007	0.0009	19	105377	1.11
5000	0.0009	0.0013	39	105377	1.08
2000	0.0013	0.0023	99	105377	1.07
1000	0.0023	0.0043	199	105377	1.06
500*	**0.0043**	**0.0081**	**399**	**105377**	**1.01***
200	0.0115	0.0213	999	105377	1
100	0.0213	0.0411	1999	105377	1
50	0.0249	---	3999	105377	---

Table 1.B2

Step 2 – Model specification, F–test

Model specification	no covariates	Estimate	St. Error	t value	F - Value
Linear	model 1	0.00368	0.0041	0.89	1.012

Linear interaction*	**model 2**	**0.00917**	**0.0048**	**1.92**	**0.904***
Quadratic	model 3	0.00710	0.0050	1.41	0.904
Quadratic interaction	model 4	0.00942	0.0068	1.38	0.904
Cubic	model 5	0.00629	0.0050	1.24	0.905
Cubic interaction	model 6	0.01033	0.0089	1.16	0.904
4th degree	model 7	0.00535	0.0059	0.91	0.903
4th degree interaction	model 8	0.01823	0.0109	1.66	0.888
5th degree	model 9	0.00751	0.0063	1.19	0.903
5th degree interaction	model 10	0.01615	0.0114	1.41	0.904
	with covariates				
Linear	model 1	0.00308	0.0041	0.75	1.06
Linear interaction*	**model 2**	**0.00775**	**0.0046**	**1.69**	**0.89***
Quadratic	model 3	0.00554	0.0050	1.10	0.90
Quadratic interaction	model 4	0.00841	0.0068	1.24	0.89
Cubic	model 5	0.00486	0.0051	0.96	0.90
Cubic interaction	model 6	0.00915	0.0089	1.03	0.89
4th degree	model 7	0.00408	0.0059	0.69	0.90
4th degree interaction	model 8	0.01663	0.0109	1.52	0.90
5th degree	model 9	0.00637	0.0063	1.01	0.90
5th degree interaction	model 10	0.01460	0.0114	1.28	0.90

Table 1.B3

Step 3 – Robustness checks, comparisons of estimates under three levels of outermost point dropouts

Dropping outliers	Treatment estimates	Standard Errors	t value
Dropping outermost 1%	0.009	0.005	1.80
with covariates	0.008	0.005	1.52
Dropping outermost 5%	0.006	0.005	1.08
with covariates	0.005	0.005	0.89
Dropping outermost 10%	0.008	0.006	1.35
with covariates	0.007	0.006	1.21

Table 1.B4

Step 1 – Bin size selection criteria, F–test

Bin size	Restricted R^2	Unrestricted R^2	# of bins	Observations	F value
10000	0.0008	0.0012	19	75532	1.59

5000	0.0012	0.0018	39	75532	1.16
2000	0.0019	0.0034	99	75532	1.14
1000	0.0034	0.0061	199	75532	1.03
500*	**0.0061**	**0.0113**	**399**	**75532**	**0.99***
200	0.0142	0.0265	999	75532	0.94
100	0.0265	0.0502	1999	75532	0.92
50	0.0502	---	3999	75532	---

Table 1.B5

Step 2 – Model specification, F–test

Model specification	no covariates	Estimate	St. Error	t value	F value
Linear	model 1	0.0037	0.0040	0.92	1.06
Linear interaction	model 2	0.0121	0.0055	2.19	1.04
Quadratic	model 3	0.0021	0.0054	0.39	1.00
Quadratic interaction*	**model 4**	**0.0135**	**0.0071**	**1.87**	**1.00***
Cubic	model 5	0.0027	0.0063	0.44	1.00
Cubic interaction	model 6	0.0010	0.0101	0.1	1.00
4th degree	model 7	0.0017	0.0063	0.27	1.00
4th degree interaction	model 8	-0.0044	0.0124	-0.35	1.00
5th degree	model 9	0.0048	0.0071	0.68	1.00
5th degree interaction	model 10	-0.0301	0.0147	-2.05	0.98
	with covariates				
Linear	model 1	0.0037	0.0040	0.93	1.06
Linear interaction	model 2	0.0118	0.0055	2.14	1.04
Quadratic	model 3	-0.0021	0.0054	-0.38	1.01
Quadratic interaction*	**model 4**	**0.0092**	**0.0078**	**1.17**	**1.00**
Cubic	model 5	0.0027	0.0063	0.44	1.01
Cubic interaction	model 6	0.0010	0.0101	0.1	1.01
4th degree	model 7	0.0017	0.0063	0.27	1.02
4th degree interaction	model 8	-0.0041	0.0124	-0.33	1.01
5th degree	model 9	0.0049	0.0071	0.68	1.01
5th degree interaction	model 10	-0.0172	0.0130	-1.32	1.00

Table 1.B6

Step 3 – Robustness checks, comparisons of estimates for three levels of outermost point dropouts

	Treatment estimates	Standard Errors	t value
Dropping outermost 1%	0.014	0.006	2.30
with covariates	0.013	0.006	2.23
Dropping outermost 5%	0.012	0.007	1.67
with covariates	0.012	0.007	1.64
Dropping outermost 10%	0.016	0.010	1.60
with covariates	0.016	0.010	1.60

Chapter 2: Can Unconditional Cash Transfers Make Recipients Worse Off? Evaluating a social assistance program in Georgia

Co-authored with Patrick Gaule and Lasha Lanchava

2.1 Introduction

Cash transfers to the poor have become a cornerstone of social policy in developing countries. Following the success of Progresa/Oportunidadesa in Mexico and Bolsa Familia in Brazil, many countries have adopted similar schemes (Barrientos and Hulme 2009). Correspondingly, a large literature has developed to evaluate the impact of cash transfers on recipient outcomes (for a review see Arnold, Conway, and Greenslade. 2011).

Elementary microeconomics suggests that cash transfers should lead to an increase of some combination of consumption and investment. The extra income from the cash transfer program could affect their overall income in two directions. First, the extra money necessarily increases total income from the start. The additional ability to spend can be spent on direct consumption or into productive investments, such as household or small business production. Second, the extra money can reduce earned income through the income effect. Labor supply of the household could thus be reduced, as the household would not need to work as much to earn as much as they had before. Alternatively, the added income could cause a preference shift such

that the household would want to work even more to have even greater consumption ability to accompany their increased social mobility. Throughout all the standard cases described above, it is almost axiomatic that a cash transfer program would increase the consumption of recipients. Certainly, this has been consistently supported by all studies that have examined effects of cash transfers on consumption (Attanasio & Mesnard 2006, Gertler et al. 2012).

More interestingly, recent literature has begun focusing more upon what type of consumption is increased (see e.g. Attanasio et al. 2012, Evans & Popova 2014). The distribution and level of spending would certainly be an outcome of the total income of the household, but it would likely remain the same if the household reduces their labor supply, or would increase in terms of consumption and/or investment if the household does not reduce their labor supply as much or even increases it. Increased consumption could take many forms according to utility preferences, but investment would most likely occur along the dimensions of durable goods, production capital, or human capital.

In this paper, an original household survey and a regression discontinuity approach are employed to evaluate a Targeted Social Assistance program (unconditional cash transfer) in Georgia. It is found that receiving the transfers leads to a worsening in (self-reported) basic economic conditions, such as the ability to afford food. A number of possible mechanisms that explain this counterintuitive result are discussed in the text, including crowding out from other sources of income as well as dynamic changes in behaviors and preference due to relaxed budget constraints. However, the results which may be driven by respondent misreporting, clearly remain puzzling.

To the best of our knowledge, there are just two other studies that evaluate the effect of the same program. Both focus on a relatively narrow set of outcomes. Abramishvili and

Lanchava (2015) find a positive effect of the program on university enrollment, while Kits et al. (2013) find that the program decreases the labor supply of women. Therefore, a major contribution of this research is the evaluation of this unconditional cash transfer program in Georgia on a wider set of outcomes that provide a much more comprehensive assessment of the multiple effects of the program. Another novel contribution to this program evaluation is the new data generated by the survey we created specifically for this purpose. In particular, the unique addition of subjective welfare indices–an uncommon feature of similar studies—that illuminates several new interesting effects.

The paper proceeds as follows. Section 2.2 describes the targeted social assistance program in Georgia. Section 2.3 covers data and section 2.4 methodology. Section 2.5 presents the results and section 2.6 concludes.

2.2 The Social Assistance Program in Georgia

Poverty is a persistent problem in Georgia with 11.6% of the population living below the national poverty line. In 2005, the reformist government that was elected after the Rose Revolution in 2003 instituted a cash transfer program for the poor. Under the scheme, program recipients received an unconditional cash transfer of 30 GEL (around 18 USD) per month plus 12 GEL (around 7 USD; later increased to 24 GEL) per household member beyond the first one. Hence, a household of two adults and two children enrolled in 2009 would receive 102 GEL (around 66 dollars) per month.

Once enrolled in the program, recipients received cash transfers monthly and there were no conditions attached to the receipt of this governmental assistance. If the recipients' economic situation were to improve substantially, they were supposed to report the improvement to the

government agency and could lose the benefits. While only a few households exited the program in this manner, the prospect of losing the support may have influenced the household's behavior.

To determine eligibility, applicants to the program were visited by government agents who asked a range of questions—from income health status to the condition of the applicant's dwelling. The answers to these questions were then aggregated using a complex formula to produce a poverty score. Applicants whose score fell below a certain threshold were deemed eligible for support from the program, while all others were excluded.

Rejected applicants could, in principle, apply in a subsequent year, but this occurred only rarely. In fact, the administrative data shows that less than 0.5% of households applied more than once. The vast majority of survey respondents who were above the poverty threshold (and hence ineligible) reported not receiving support from the program when interviewed five years later. As of 2015, 11.6% of the population received cash transfers.[8] The program redistributed roughly 1% of GDP, which was equivalent to 3.3% of all public expenditure.

2.3 Data

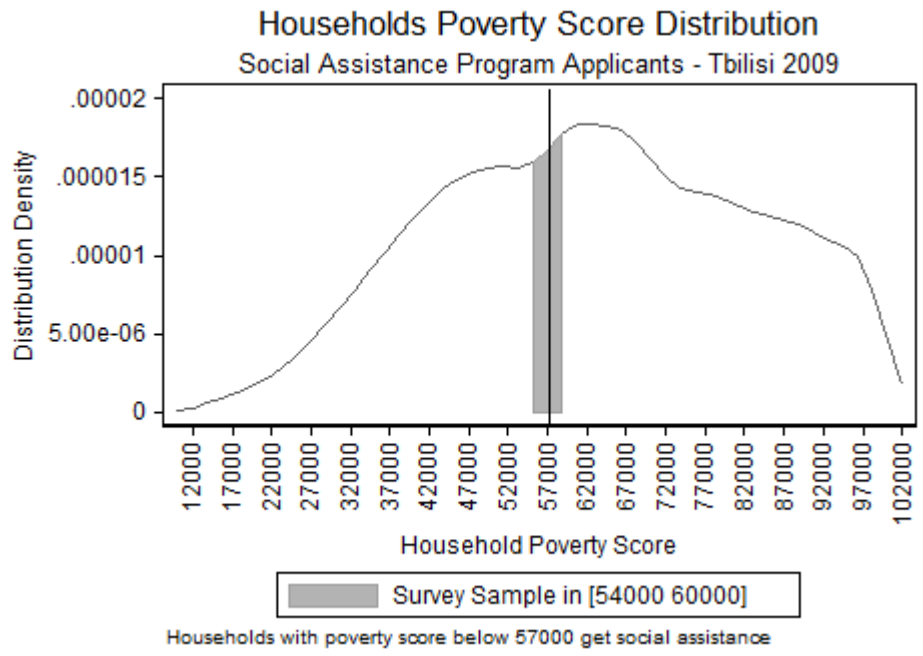
We obtained data from the Social Service Agency (SSA) of Georgia covering the population of households applying to the program from its inception in 2005 to March 2010. The data from the SSA includes the household poverty score, the time of the visit by government agent, and the applicants' answers to a number of questions that enter into the poverty score calculation. To complement the SSA data, we designed a survey instrument to measure the

[8] Our own calculation based upon social assistance data described below.

economic outcomes of the program. To a large extent, we re-used questions used in other surveys in Georgia. The survey questions are presented in appendix 2.A.

An important choice was which households to survey. Since we were planning to analyze the program in a regression discontinuity framework, it was natural to sample households as close to the threshold as possible. While we were initially planning a nationally representative survey, the logistical difficulties of surveying households outside of the capital Tbilisi proved considerable. Given the limited resources at our disposal, we chose to focus on applicants from Tbilisi. We selected 901 households from the Tbilisi region who applied in 2009. Figure 2.1 illustrates our sampling frame.

Figure: 2.1 sampling frame



We conducted our survey between October 2014 and March 2015, with a break in January and February 2015.[9]Research assistants contacted 901 households—including 451 program eligible and 450 program ineligible households—and obtained 334 answers, for a response rate of 37.2%. The response rate was balanced across eligible (36.7%) and non-eligible respondents (37.4%).

It is easily seen that the histogram density just around the threshold is not ideally flat. However, this does not indicate that there is successful participant manipulation, as that would be indicated by a jump in the density just below the threshold in order to obtain the social assistance. Therefore, it can be assumed that the rising density just around the threshold is a natural artifact of the data. Moreover, the regression discontinuity equation employs different distance variable in order to assess if any unobservable effect is occurring in the data.

In the survey, we explicitly asked whether respondents were program recipients in 2009 (see the full survey instrument in Appendix 2.A). This enables us to compare self-reported recipient status with recipient status inferred from the poverty score in the administrative data (see table 2.1).

Table 2.1: Self-reported recipient status in 2014 and 2009 eligibility threshold

		Below eligibility threshold in 2009		
		Yes	**No**	
In 2014, reported receiving program in 2009	**Yes**	121	15	136
	No	38	160	198
		159	175	334

Of the respondents who were eligible for the program according to their 2009 poverty score, 76% reported receiving the program's assistance at the time of survey. The remainder

[9] The bulk of the data was collected in October and November 2014

could be due either to imperfect recall, unwillingness to disclose receiving of the program, recipient change of address, or exit from the program. Conversely, of the respondents who were not eligible for the program according to their 2009 poverty score, 91% reported not receiving the program as of 2014. The remaining 9% could be due to entry in the program at a subsequent date, incorrect answers of the respondent, or recipient change of address. Given that some level of noise can be expected from survey data, there seems to be a reasonably good concordance between eligibility inferred from administrative data and self-reported program recipient status. We use eligibility inferred from administrative data for the purpose of analysis.[10]

2.4 Methodology

To estimate the impact of the program, we implement a regression discontinuity design (RDD) approach. Specifically, we used the form:

$$Y_i = \beta_0 + \beta_1 T_i + f(S) + \varepsilon_i \qquad (1)$$

where Y_i is an outcome variable, T_i is an indicator for being a program recipient (which corresponds to being below the eligibility threshold), and $f(S)$ is a polynomial function of the difference between the poverty score and the eligibility threshold. We use a polynomial of degree one (linearization), but allow for different slopes on both sides of the threshold.

Provided that the assignment to the treatment is as good as random in the neighborhood of the eligibility threshold, this specification will give the local average treatment effect (LATE) of the program. An important provision for the interpretation of the results is that we do not estimate the average effect of the program, only the effect of the program close to the threshold.

[10] This level of noise, which is expected from survey data, could mean that RDD is fuzzy in this case. However, the use of eligibility inferred from administrative data in the analysis should allay concerns about the method's efficacy.

2.5 Results

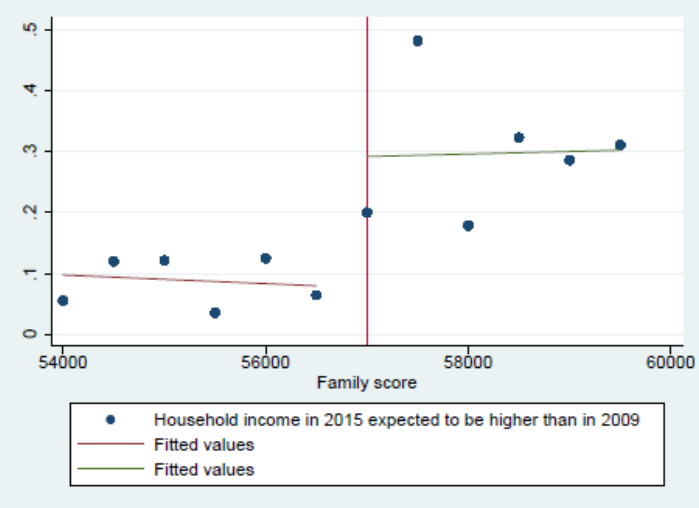
Table 2.2: Effect of the program on economic conditions

	Program receipt		Mean of D.V.
Money is not enough for food	0.297***	(0.106)	0.49
Income in 2015 expected to be higher than in 2009	-0.217**	(0.084)	0.20
Monthly income is below USD 100 per month	0.017	(0.104)	0.33
Monthly income is below USD 250 per month	0.084	(0.105)	0.70
Monthly spending is below USD 100 per month	0.023	(0.104)	0.32
Monthly spending is below USD 250 per month	0.064	(0.104)	0.70
Has savings	-0.021	(0.020)	0.01
Has debts	-0.003	(0.107)	0.61
Had to borrow money to pay for utilities	0.316**	(0.114)	0.38
Had to borrow money to pay for food	0.158	(0.113)	0.53
Rate of happiness	-0.379	(0.621)	5.67
Rate of satisfaction	0.093	(0.545)	4.38
Rate of health	-0.131	(0.225)	2.70
Economic condition of the household	-0.037	(0.111)	0.42
Household economic position in 10 ladders	-0.518	(0.379)	3.44
Share of household members employed	-0.044	(0.037)	0.10
Each line corresponds to a different regression with the dependent variable indicated in the first column. All regressions include linear distance to the threshold as control. N = 334. Standard errors in parentheses. *p < 0.1, ** p < 0.05, ***p < 0.01			

Table 2.2 displays results for a set of economic outcomes. Each line corresponds to a different regression with the relevant dependent variable indicated on the left. Program recipients are significantly more likely to describe their economic condition as “money is not enough for food". Also, program recipients are less likely to expect that their income in 2015 will be higher than in 2009.

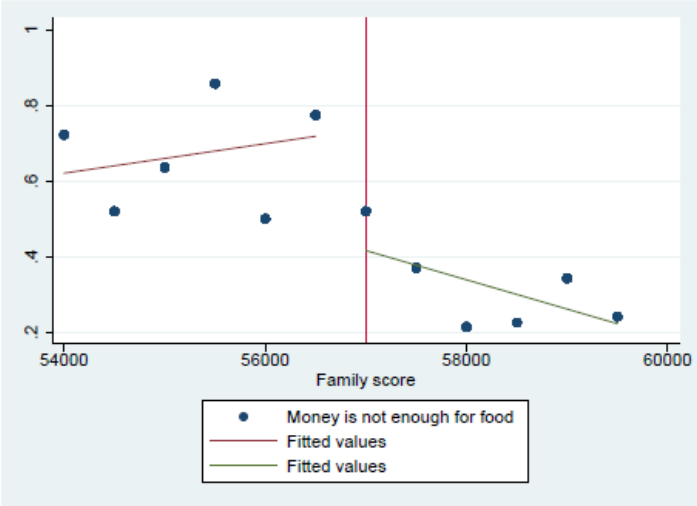
Visual representations of the results are provided below in figures 2.A1 and 2.A2. Please note that the dots on the graphs denote the percent of households within each bin that responded “yes” to the survey questions.

Figure 2.A1: Percent of households responding positively to the question "is your household's income in 2015 expected to be higher than in 2009?" before and after the threshold



Household with a poverty score below 57000 get social assistance.

Figure 2.A2: Percent of households responding positively to the question "does your household not have enough money for food?" before and after the threshold



Household with a poverty score below 57000 get social assistance.

The figures exhibit the unexpected results mentioned in the introduction, that program recipients expect to earn less on average in 2015 and believe more so, on average, that there is not enough money for food than their non-recipient counterparts. This may indicate a crowding

out effect or a dynamic change in preferences influenced in part by receiving or not receiving the cash transfers. Additional explanations of these results are discussed below.

The table also displays the effect upon likelihood of level of income and spending, saving, and more. The income is measured inclusive of the program's cash transfers, being below USD 100 per month or below USD 250 per month (exact income figures were not enquired about, only whether income and spending fell into one of six intervals). Being a recipient has no significant effect on most of these measures; though it may be worthwhile to note that, though insignificant, the point estimates correspond to a higher likelihood of lower income and spending post program participation. Program recipients are more likely to report that they have had to borrow to pay for utilities in the last six months, as well as having had to borrow to pay for food, although only the former is significant. There is virtually no difference between program recipients and the control group in terms of the likelihood of being in debt and the likelihood of having savings. The effect of program receipt on the rate of happiness, life satisfaction, self-reported health, and the perceived economic condition relative to non-recipient households is not significant.

Table 2.3: Effect of the program on inability to afford certain items

	(1) Bread	(2) Milk	(3) Poultry	(4) Meat	(5) Pork	(6) Fish
Program receipt	0.214** (0.097)	0.273** (0.107)	0.293*** (0.101)	0.098 (0.080)	0.101 (0.080)	0.175* (0.101)
Observations	331	329	328	330	330	329
Distance	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	0.20	0.52	0.67	0.81	0.81	0.69
	(7) Vegetables	(8) Potatoes	(9) Chocolate	(10) Electricity	(11) Gas	
Program receipt	0.228** (0.109)	0.157 (0.104)	0.236* (0.103)	0.089 (0.099)	0.033 (0.102)	
Observations	329	327	324	328	305	

Distance	Yes	Yes	Yes	Yes	Yes	
Mean of dependent variable	0.40	0.25	0.64	0.65	0.65	
Standard errors in parentheses. *p < 0.1, ** p < 0.05, ***p < 0.01						

Inability to afford certain household items over the course of a typical month is further detailed in table 2.3. Program recipients are significantly more likely to answer that they have to limit their consumption of bread, milk, poultry, fish, vegetables, sweets, and chocolate due to the budget difficulties. For the other items we asked about (meat, pork, potatoes, electricity, gas), the effect of program recipients is not significant but the point estimates go in the same direction.

Table 2.4: Effect of the program on items owned

	(1) Color television	(2) Digital camera	(3) Washing machine	(4) Fridge
Program receipt	-0.073 (0.070)	0.013 (0.043)	-0.125 (0.098)	0.029 (0.105)
Observations	331	328	329	331
Distance	Yes	Yes	Yes	Yes
Mean of dependent variable	0.20	0.52	0.67	0.81
	(5) Air conditioner	(6) Car	(7) Land line phone	(8) Cell phone
Program receipt	-0.024 (0.026)	-0.013 (0.087)	-0.121 (0.112)	-0.036 (0.099)
Observations	329	329	328	330
Distance	Yes	Yes	Yes	Yes
Mean of dependent variable	0.02	0.15	0.59	0.74
Standard errors in parentheses. *p < 0.1, ** p < 0.05, ***p < 0.01				

Lastly table 2.4 reports the effect of the program on ownership of certain durable items, including color televisions, digital cameras, washing machines, fridges, air conditioners, cars, landline phones, and cell phones. None of the coefficients are significant but most of the point estimates are negative.

These results seem to draw a worrying picture regarding the effect of the program on basic economic conditions. Amongst the inquiries of the survey, there is no area where the program showed a significant effect that could be interpreted as an improvement in household conditions. In fact, most point estimates indicate a negative effect of program receipt and, in several cases, are significant. However, our results should be interpreted with caution given measurement issues and the relatively low power associated with our sample size.

2.6 Discussion and Conclusion

We evaluate a social assistance program in Georgia using an original household survey and a regression discontinuity approach. Contrary to our expectations, we find that receiving the program leads to a worsening in (self-reported) basic economic conditions, such as the ability to afford food. We find patterns corresponding to a worsening in economic conditions in the answers to a sizeable number of different questions, though the results are significant only for a subset of those.

The mean income of the households just below the threshold is slightly lower than the households just above the threshold. While specific figures are not available in the data, the transfer amount certainly increased the mean income of the households just below the threshold to significantly higher levels than their above-threshold counterparts; at least at first. So, the differences in responses to the survey by the two groups cannot be attributed to income, which were not significantly different amongst the groups at the time of the survey.

These results should be interpreted with caution given the following caveats. First, our sample size is relatively small, both in absolute number and relative to the population. Second,

the regression discontinuity approach identifies the local average treatment effect around the threshold, and not the average treatment effect of the program. Third, we are relying on self-reported data, which necessarily involves noise. A potentially greater concern is that program recipients may conceivably be less truthful in their answers if they are concerned about losing the program. Fourth, we are not measuring consumption (not to mention welfare) with precision due to the nature of the questions asked.

To the extent that the recipients' worsening of economic conditions relative to the control is genuine, a natural question to ask is why that might be happening. One possibility is that the program crowds out other sources of income. In principle, the cash transfers could reduce receipts from other social programs, but that is not the case in the Georgian context. More relevant in our context may be reductions in remittances from family members abroad or in Georgia or other forms of private cash transfers. Alternatively, receiving the program could reduce incentives to work, as a previous study of the same program has found (Kits et al. 2013). However, it would require a very large elasticity of these other sources of income to the public transfers to generate the observed patterns. A very large elasticity may seem implausible, at least in the absence of other contributory factors.

Another possibility is that the recipients invest both the transfers and additional resources in investments in durable goods or human capital. This could then lead to a lower ability to afford food (and other items) in the time window we are observing. In our data we do not observe that recipients own more durable goods than the control group. However, a previous study (Abramishvili & Lanchava 2015) has found that the same program had an effect on university enrollment for the children of the recipients.

Other explanations are possible. The program may change perceptions of conditions rather the conditions themselves. For example, a household may feel better off at first as they are able to consume more, but their preferences and behavior may actually dynamically change over time such that they perceive more goods that they now want but cannot afford (Dasso and Fernandez 2013). Alternatively, the cash transfers may lead to a 'Dutch disease' situation by encouraging consumption of "sin" goods, such as alcohol, tobacco, gambling, etc. (Devereux 2002). Further, it is possible that differential (mis-) reporting by respondents drive our findings (Baird et al 2011). Which (if any) of the explanations suggested here holds has important implications for how the program should be viewed.

Given the limitations of our study, our conclusions should be treated with caution. However, we believe that evidence subject to limitations is better than no evidence at all and that this study can be a useful step in understanding the effect of this program, as well as other cash transfer programs in transition countries. In light of our findings, we recommend that a thorough and extensive evaluation of the social assistance program in Georgia be conducted. For example, researchers could run another, much more extensive round of the survey that would reach a larger portion of the program applicant population as well as be more comprehensive in obtaining a financial assessment of the households similar to the one done as part of the program application.

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Chapter 3: An Impact Evaluation of Mass Replacement of School Principals in Georgia

3.1 Introduction

This paper investigates whether a unique education policy positively affected university enrollment rates of public school students in Georgia. Under the Georgian political initiative to decentralize school governance, the Ministry of Education and Science issued an order (N543) in July 2007, officially dismissing all public school principals and subsequently "randomly" assigning qualified candidates to public schools across the country, under the assumption that the replacement of the principals with randomly assigning qualified candidates to public schools would fairly decentralize and improve school governance across Georgia. About half of the public school principals were actually replaced with new candidates, a majority of whom were assigned through a random allocation mechanism.

Accordingly, this paper uses a standard difference-in-differences methodology to compare treated public schools with private schools that are not affected by the policy, in order to identify how this reform impacted education outcomes. Using the National Assessment and Examination Center university admissions data, it can be seen that the public schools with replaced principals increased university enrollment more than the control schools by an average of 4%. The largest part of this increase comes from schools with randomly assigned principals.

The positive findings herein could impact education policy in developing (and perhaps developed) countries and invites further research where applicable. The statistically significant and strong effects of this type of reform could cause a positive domino effect in the developing world, especially in countries with similar characteristics and predicaments in their education system.

The main objective of any school system is to improve student learning outcomes, cognitive skills, and socialization in society. In order to reach this objective and make schools more efficient, specific efforts are made by teachers, staff, and the principal school-wide. It is widely believed (Branch et al, 2012; Bloom et al., 2015 and Oduro et al, 2007) that the quality of the principal plays an important role in a school's organizational success, as well as significantly affecting student scholastic achievements.

As the majority of schools are financed by the government in most countries (including Georgia), public finance efficacy makes it necessary to create and implement policies that ensure that the highest quality principals are selected (or assigned) to public schools.

School governing mechanisms vary significantly across developed and developing countries (Day and Sammons, 2013). In the developing world, many countries continue to maintain a centralized governance of schools, while other countries have taken steps towards decentralization (Patrinos and Fasih, 2009). Recently, much interest has been devoted to post-communist countries where the totalitarian regime collapsed and the countries started building their own policies independently. The state of Georgia is one of the most studied countries among the former Soviet states. Indeed, Georgian government reforms implemented since 2003 have received considerable attention from researchers and policy makers worldwide (The World Bank, 2012).

3.2 Georgian Education Reform of 2007

Georgia became a sovereign state in 1991 and has since experienced unstable economic and political transition periods. In 2003, a reformist government took power through the Rose Revolution with the aim of modernizing the state, eliminating corruption, ensuring equal opportunities, and stabilizing the political-economic situation. The new government launched reforms in almost all systems of governance ranging from law enforcement to healthcare, including the decentralization of public school governance from the state.

The Georgian governmental reform of the secondary school system was implemented as follows: First, in 2005, the public financing of elementary and secondary education was replaced with an enrollment-based voucher system.[11] In 2006, following the initiative of the Ministry of Education and Science, each public school then elected a local governing board,[12] which consisted of the principal, vice-principals, teachers, parents, and student representatives. The 2006 initiative's reorganization of school management paved the way for the principal replacement reform, with the government aiming to replace all public school principals in 2007. The Ministry of Education and Science officially dismissed all public school principals and announced a public tender for the open positions, that included a skills based exam, an interview process, and the combination of a meritocratic and random assignment mechanism.

More specifically, the Ministry of Education and Science took the following steps: 1) The Ministry dismissed all public school principals and announced an open tender for new leaders. The potential candidates (job seekers) registered themselves in a single administrative

[11] This reform was uniform to all schools in Georgia. Each school received funds in an amount equivalent to the enrollment voucher times the number of students.
[12] Teachers, parents, and students were given the opportunity to participate in schools' governance. This reform concerned only public schools.

district in which they would compete for a principal position. There were a total of 2200 open positions in 68 districts. Around 15000 registered candidates took a comprehensive, four-component examination[13] followed by individual, in-person interviews with regional representatives of the ministry.

2) Based on the results of the standardized examination and passing the individual interviews, 5500[14] candidates advanced to the final step in the hiring/replacement process. The examination evaluated not only literacy levels but also managerial skills. The content of each test covered managerial theories, such as resource management and planning for education. While this exam may not be a perfect proxy for principal ability, it was designed by the Georgian government to best appraise the knowledge and skills deemed most appropriate and necessary for principal success.

3) The meritocratic part of the process gave the top 20% of approved candidates from each district the right to designate the school where they would undergo the final step—an interview with the school's local governing board. Each school had a maximum of three candidate slots available. When more than three of the top 20% candidates expressed interest in the same school, their test results were sorted and priority was given to the higher scorers.[15] Each top 20% candidate could choose only one school. However, if higher ranking candidates filled all three slots of their selected school, the candidate was permitted to name another target school; a process that repeated until all top 20% scorers had been assigned to a school. This important mechanism of the design removed the incentive for candidates to strategically choose schools based upon their ranking (thus eliminating a serious potential source of endogeneity from this

[13] The examinations were in General Skills, Georgian Language, Law, and Case Study Analysis.
[14] This number is approximately a third of all initial participants.
[15] Priority of slot allocation was always based on the results of the test and individual interview.

natural experiment). While self-selection endogeneity remains in terms of these candidates choosing schools with better socio-economic conditions and/or where they strategically expected to have a better chance of obtaining the position due to social ties, this is directly accounted for in the conclusions to this study. The bottom 80% candidates were assigned by a random allocation mechanism (by lottery) to the remaining vacant slots in the final step—an interview with the local governing board. The lottery was transparent and all candidates had the opportunity to watch the results in real time.

4) The local governing board of each school made the final selection decision. Each candidate underwent a single interview and was either chosen as the new principal or was dismissed from the process. The selection of a candidate depended solely on the decision of the governing board. A rejection of all three candidates resulted in the dismissed principal retaining his position until the next round of the replacement reform process.[16]

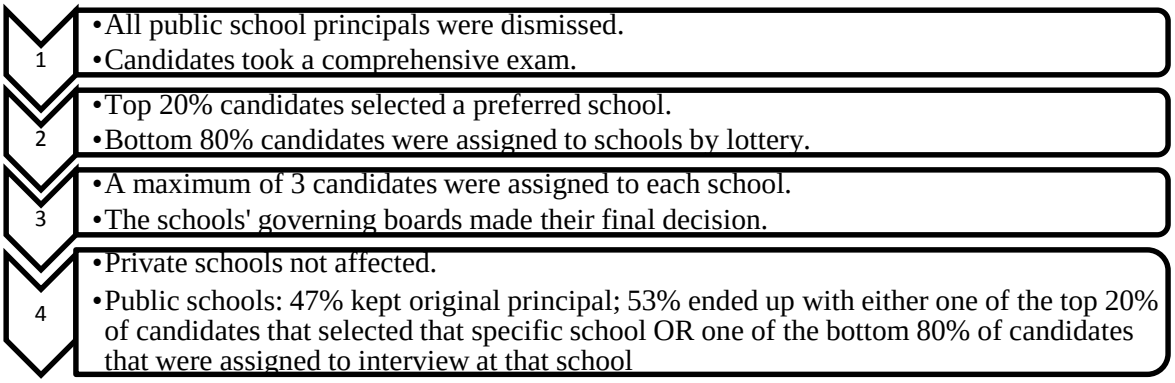
Ministry representatives carefully monitored all processes to avoid nepotism or the intentional rejection of candidates in order to retain the incumbent principals. Only 53% of public schools selected new principals with 5-year terms based on the new policy. The remaining 47% of public schools kept the existing principals. Unfortunately, as the data does not specify the identities of the candidates, there is no way to assess how many or which schools had the incumbent principal as one of their candidates.

However, based upon the replacement mechanism design, the number of principals who could retain their position through successfully passing the exam as a top 20% principal, and thus who had the ability to choose the school where they had previously worked, cannot be large or significantly affect this analysis.

[16] Next round of the principals' replacement took place in 2011 and in case the principal resigned before the next rotation, the Ministry of Education had the right to assign a new candidate to the school.

New principals began their administration from the 2007/2008 academic year. The entire process is shown in the chain of blocks below.

Illustration: Steps of Education Reform in 2007



Overall, the reform represented a major transformation of the system and consisted, in part, of a randomization mechanism for the allocation of the candidates that scored in the bottom 80% of the approved participants. Unfortunately, there is no comprehensive description explaining why the principals retained their positions in 47% of the schools. However, Ministry officials publicly disclosed the top two reasons incumbent principals remained in many rural and ethnic minority schools (2.9 % of schools constitute ethnic minority schools), which made up the vast majority of schools where the reform did not lead to a change of principal. While the most common reason at rural schools was a lack of candidates, at ethnic minority schools the linguistic issues were the most significant barriers to policy implementation.

The reform resulted in the partitioning of all Georgian schools into four different groups:

A) Private schools that were not affected by the policy.[17]

[17] The convincing reason the reform would not change the behavior of the private school principals is compensation. Private sector principals are better paid than public sector principals. Unfortunately, the data did not provide information about teacher and student mobility between public and private schools.

B) Public schools that did not replace the existing principal (47% of schools).[18]

C) Public schools that chose one of the top 20% candidates that specified their school of preference (27% of schools).

D) Public schools that chose one of the randomly assigned (by lottery) candidates from the bottom 80% of those that passed the exam (26% of schools).

Since the majority of newly elected principals (based on standardized test scores and passing the in-person interview) were assigned through a random allocation mechanism (by lottery), the reform offers a quasi-natural experiment that partitioned schools into control (group A) and treatment groups (groups B, C, and D, where only group D is randomly assigned).

3.3 Literature and Contribution

A major part of the empirical literature that studies the characteristics of effective school governance focuses on principals. Leadership is often considered a main factor in the organizational success of schools and it is believed that it has direct and indirect effects upon student academic performance. Indirectly, principals may promote student outcomes through enhancing conditions for teaching and learning (infrastructure, safety, monitoring, etc.). Directly, school leaders could have an impact on teaching quality through teacher turnover and training (Day & Sammons, 2013; Robinson et al., 2009). Usually, it is difficult to disentangle the causal effect of school principals on learning outcomes (Branch et al, 2012), because schools are heterogeneous and leadership might contribute less at schools of higher quality and vice versa.

[18] Group B contains both the schools that chose their former principal as well as those that rejected all applicants. As discussed earlier, the former cannot make up any significant portion of the whole group.

Although some variation in student/school learning outcomes is attributed to principals, no consensus exists on the amount or the mechanism of how principals impact the educational outcomes within/across schools.

Literature on school leadership in developing countries also focuses on principals, their role in managing schools, 'plant-level' management tasks, and on external reform initiatives promoted by governments. Bloom et al. (2015) find a strong association between the quality of a principal and the learning outcomes of pupils. They surveyed 1800 schools across 8 countries (including developed) and concluded that higher management quality is strongly associated with better educational outcomes, with half of the variation being attributed to principal leadership and school governance.

As education systems vary across countries due to cultural diversity, it is important to study this concept in the within-country context (Heck 1996). Systemic reforms in school governance in developing countries have attracted many scholars who have pointed towards the importance of principals. Studies demonstrate that school leaders still face non-bureaucratic challenges even after decentralizing policies have been implemented in developing countries (Oplatka, 2004). However, there are only a handful of studies available on the effectiveness of school leaders in developing countries (Oduro et al, 2007), which makes new evidence based analyses necessary.

Compared to existing studies, this work investigates a large-scale, unique, and partially exogenous variation of principal turnover and its effect on school outcomes. In addition, it exploits the fact that the reform replaced a majority of the principals through a lottery mechanism, which makes the reform distinctive and worth studying.

Moreover, this paper is the first study of this particular education reform policy in Georgia, and it investigates the effects of this education policy on scholastic achievement. At the time of this reform, however, no standardized exams—such as the secondary school final exams that were implemented in the 2010/2011 school year—were taken by the students affected by this reform. Therefore, Georgian university enrollment rates are employed as a proxy for this measure. There are two advantages of studying university enrollment rates: 1) it proxies the student's scholastic achievement as well as willingness and ability to acquire higher education and increase human capital and 2) it is the most accurate and universal measure covering the full panel of schools throughout the years of the study. Furthermore, there is no other measure that would uncover the school-level dynamics of scholastic achievements in Georgia. Using standard Difference-In-Differences (DID) methodology, the results show that the public schools with replaced principals increased university enrollments by an average of 4% more than the control schools.

Interestingly, the results also reveal the importance of the assignment mechanism. The largest part of this increase comes from schools with lottery-assigned principals. Those public schools where the principals were replaced through random assignment performed better in terms of university admissions (6% more than control) than those schools that had principals who were able to influence their school assignment. Specifically, schools with replaced principals (groups 3 and 4) improved their academic standing by 6.4 % and the schools with the lottery assigned principals (group 4) increased the relative enrollment rate by 9.5%. Given that the national average enrollment rate[19] is 63%, this reform produced considerable gains.

[19]Enrollment rate is the share of applicants who were admitted.

In addition, this paper investigates the school-choice preferences of principals. The results show that, with respect to the year before the reform, there is no significant difference between the university enrollment rates of the schools with principles from the quintile above the threshold (principals who chose the schools) and schools with principals from the quintile just below the threshold (who were assigned based on random allocation). In Georgia, the roles of principals are almost the same as in most other countries (improve learning environments, monitor the teaching process, administration, etc.), but there is a cultural specificity in terms of the education system and schooling, i.e. the magnitude of the social connections is probably higher than the international average. It is assumed that randomly assigned principals lack social and political ties at the school, which allows for more intra-school reform, while the principals who chose the schools are less likely to enact significant reforms due to likely existing political ties. Evidence of this effect exists, but is not a documented variable in the dataset.

Furthermore, covariate plots between the groups show that there is a selection effect occurring, with the top 20% principals choosing already better schools in terms of socio-economic characteristics. Along with the DID results, these findings indicate that the average lottery-assigned principal was able to improve their school's university enrollment rate more than the average principal who was able to influence their school assignment. However, it is unclear which underlying mechanisms are causing which effects.

3.4 Data

To assess the effect of the principal on school-level scholastic achievements, nation-wide standardized university admissions data from 2005 to 2010 were used to compile university enrollment rates and were linked to the principals of the schools. The school level university

enrollment rate is a proxy of student scholastic achievement as well as willingness and ability to pursue higher education after graduating from secondary school. The necessary data was acquired from the National Assessment and Examination Center (NAEC), affiliated with Georgia's Ministry of Education and Science. The NAEC collects data annually on student admissions, entry examinations, and scholarship allocations related to accredited universities in Georgia. Since the 2005 reform, secondary school graduates who wish to enter university take mandatory exams (unified tests) on general skills, Georgian, a foreign language, and a fourth subject corresponding to the student's specialization.

Table 3.1 below shows the numbers of schools with students (at least one student) registered for university admission exams administered by the NAEC for the 2005-2010 periods. Schools are categorized based on the reform partitioning. There are four different groups of schools in the sample. Private schools (no direct effect from the policy), public schools without principal replacement, public schools with replacement of principals by top 20% candidates, and public schools with principal replacement by lottery candidates (bottom 80% of principals based on the test results).

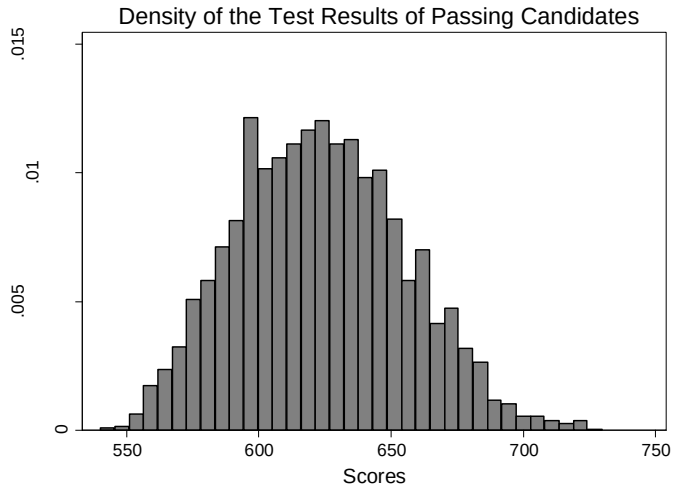
The numbers are stable over the years except for the 2008/2009 academic year when the government extended the years of schooling from 11 to 12 and, consequently, demand for higher education and associated exams declined. Although there were some schools that fell outside of the regulated extension, e.g. schools for ethnic minorities, 2008 is omitted from the analysis as it represents a small part of the sample.

Table 3.1. Private and public schools in the NAEC data from 2005 to 2010.

Schools in Georgia	Year						
	2005	2006	2007	2008	2009	2010	Total
Private	217	173	198	20	180	204	992
Public Without Principal Replacement	953	994	804	132	833	953	4669
Public with Top 20% Principal	316	326	295	28	346	341	1652
Public with Bottom 80% Principal	226	246	221	19	249	242	1203
Total	1712	1739	1518	199	1608	1740	8516

For the purpose of this analysis, data was obtained from the Ministry of Education and Science on school characteristics such as size, address, share of socially disadvantaged pupils, and the number of teachers for each school. In addition, the ministry provided statistical data on the education reforms such as candidates' registration district, their identifiers, test results, and the names of their assigned/chosen schools based on the top 20 or bottom 80 percent categories. Figure 3.1 below presents the distribution of all candidates' test results. It is clear that the distribution resembles a normal density function, which might mean that the exam was well balanced.

Figure 3.1. Distribution of test results of all passing candidates.

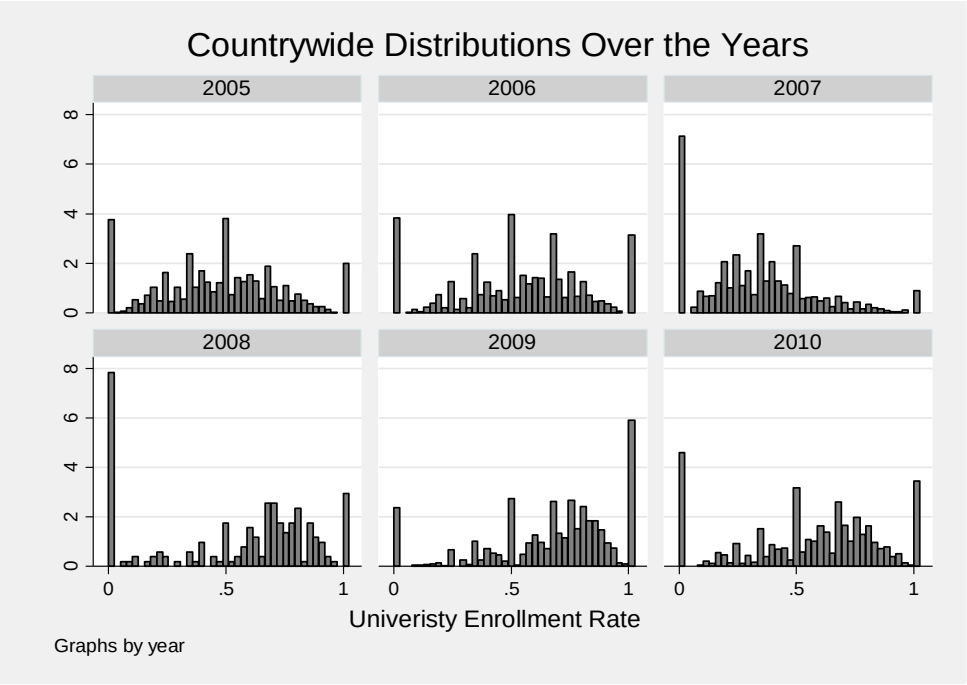


Source: The Ministry of Education and Science of Georgia

Merging these two data sets forms a panel of the schools from 2005 to 2010, which combines all schools with the covariates and the outcome variable. Specifically, the outcome variable is the university enrollment rate attained by the annual cohort of students from a school. It is defined as the ratio of students successfully admitted to university from a particular school out of the total number of university applicants from that school; i.e. the latter being equivalent to the number of the secondary school's graduates that take the NAEC exam. This variable proxies scholastic achievement and measures the size of ability and willingness to continue schooling in higher education. It varies from 0 to 1.

Figure 3.2 presents the distributions of the university enrollment rates across the country. Following descriptive analysis, it was concluded that the main reason for the considerable numbers of 0s and 1s in the data is mostly due to the high number of very small schools from where only a few students apply to university each year (regression results do not materially change with the weighted inclusion of the small schools; see table 3.A3 in appendix 3.A). Schools with five applicants or less were designated as small schools in the sample. This threshold was determined as optimal since adding another marginal extension of the number (from five to six) does not change the results, and the overall findings become and remain stable over the specification. Hence, smaller schools were omitted from the analysis in order to avoid over/under estimating results. Figure 3.2 also suggests that the distributions of the university enrollment rates are skewed to the right for 2009 and 2010. While this change could be the outcome of the additional year of study (except at ethnic schools) in 2008, it does not affect the evaluation herein as this effect is averaged/canceled out through the DID methodology.

Figure 3.2. Distribution of university enrollment rates from 2005 to 2010.

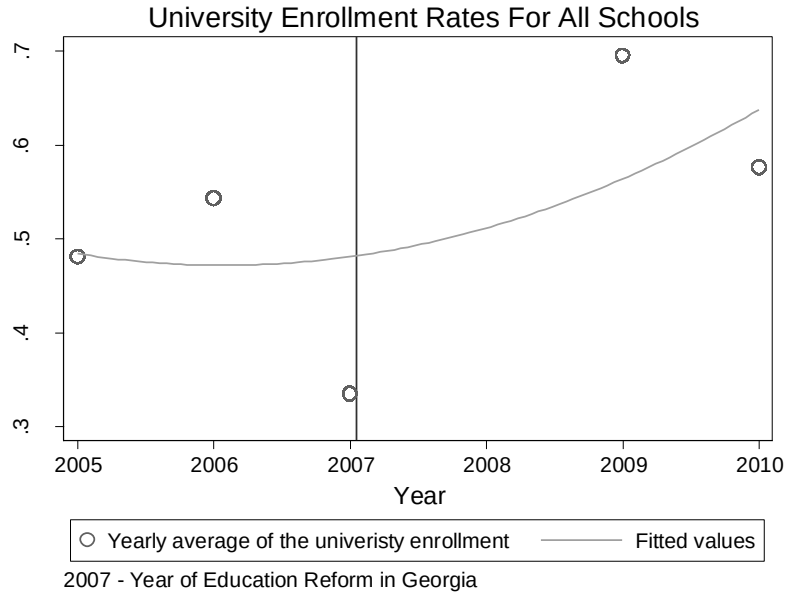


Source: The National Assessment and Examination Center of Georgia

Figure 3.3 plots annual averages of university enrollment rates over the years. Based on the fitted values line, schools in Georgia have been positively trending in terms of the university enrollment rate since 2005. This is accounted for in the analysis through time effects. Further, figure 3.3 shows that the country level average of the university enrollment rate noticeably decreased in 2007. The main reason for this decline is the university accreditation process.[20] As places were limited because some universities could not admit students in that year, the rate dropped uniformly for public and private schools.

[20] The Georgian government created new quality control requirements for public and private universities. They were required to meet the new minimum standards during the 2006/2007 school year to be able to continue to enroll students in the 2007/2008 school year and onwards. Many universities did not pass the accreditation program until the following school year.

Figure 3.3. School level university enrollment rates in Georgia from 2005 to 2010.



Source: The National Assessment and Examination Center of Georgia

3.5 Empirical Models

Given that the education policy partitioned schools in terms of a clear control group (private schools) and quasi-treatment groups (the three categories of public schools affected by the policy), this study aims to estimate the effect of the education policy on school scholastic achievements (through the proxy of university enrollment rates). To do so, a difference-in-differences methodology is applied (Abadie, 2005; Angrist and Pischke, 2009). The mathematical formulation of the model is the following:

$$y_i = \beta_0 + \beta_1 \cdot period_i + \beta_2 \cdot treated_i + \beta_3 \cdot period_i \cdot treated_i + \gamma X_i + \varepsilon_i$$

where y_i stands for the outcome variable, $period_i$ is a period indicator dummy variable, which equals 0 before the reform and 1 after the policy implementation. The variable $treated_i$ denotes the treatment status of the school i. It attains 1 if the school falls within one of the treatment groups (B, C, and/or D) and 0 if not. A vector of covariates X_i is a set of explanatory variables, which might have explanatory power in the model (size of the school, teacher-student ratio, the percentage of economically vulnerable pupils in the school, location, etc.). Coefficient estimates of β_3 measure the difference-in-difference of the outcome variable for a treatment group. While private schools serve as a clear control, given the mechanism of the reform, the multiple treatment groups can be subdivided into five meaningful treatment groups.

1) All public schools = Groups B, C, and D
2) Public schools without principal replacements = Group B
3) Public schools with replaced principals = Groups C and D
4) Public schools with replaced principals from bottom 80% candidates = Group D
5) Public schools with replaced principals from top 20% candidates = Group C

Consequently, five different DID regressions have been run.

Since the school panel data includes 2-year time spans before and after the reform, the general DID framework is extended by the addition of interaction terms with respect to time dummy variables. This modification allows us to investigate the dynamics of the outcome variables for each year. This modified version of the DID methodology is as follows:

$$y_i = \beta_0 + \beta_1 \cdot i.year_i + \beta_2 \cdot treated_i + \beta_3 \cdot i.year_i \cdot treated_i + \gamma X_i + \varepsilon_i$$

where all variables remain unchanged with the exception of the period indicator, which is now a specific year indicator. That is, $i.year$ is a dummy variable for each year, equaling 1 if for a

particular year and 0 otherwise. This modification also allows us to investigate the effect across the years.

After the reform, three types of principals governed the public schools. The principals who did not choose the school themselves because of the lottery (bottom 80%), those who chose preferred schools (top 20%), and those principals who were not replaced by the policy. In order to estimate the effect of another "treatment", that of random assignment, one must compare the outcomes of the C and D groups of principals (the top 20% that were given target school preference and the bottom 80% which were assigned randomly) through a Regression Discontinuity Design (Lee and Lemieux, 2010):

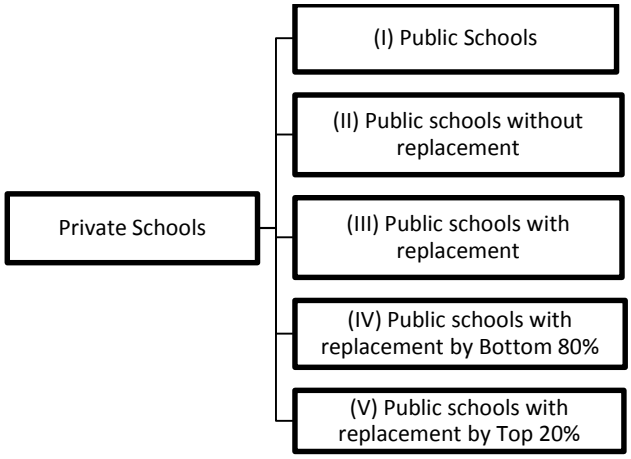
$$y_i = \alpha + \beta \cdot T_i + f(test_i) + \varepsilon_i$$

where y_i is a set of school level characteristics (socio-economic, university enrollment rate, etc.), T_i is the treatment dummy. It equals 1 if the principal is from the top 20% and 0 if not. Importantly, the observations of the RDD are limited to subjects symmetrically around the threshold. $f(test_i)$ is a polynomial function of the principals' test scores from the selection exam. A linear version, centered at the thresholds, has been applied: $f(test_i) = |test\ result - threshold|$.

3.6 Results

As discussed in the methodology section, Georgia's schools were partitioned into different groups based on the inherent characteristics of the education reform. Given that the policy intended to change the principals in all public schools but would have no direct effect on

private schools, the five groupings of public schools were used as the treatment groups studied in comparison with the private schools as the control group. To reiterate, the treatment group categorization of the public schools are the following: 1) all public schools, 2) public schools without principal replacements, 3) public schools with replaced principals, 4) public schools with replaced principals from the bottom 80% of candidates, and 5) public schools with replaced principals from the top 20% of candidates. Below is a visual representation.



For each difference-in-difference group pairing, the parallel trend assumption was checked and all divergent covariates were incorporated into the model as control variables. Specifically, all observed covariates were studied as outcome variables and were analyzed to discern whether treatment status made any difference in them. School size, teacher-student ratio, and poverty changed both over time and as treatment status. Therefore, they were added to the regressions as control variables in order to avoid misinterpretation of results. The results of the five DID regressions are presented in table 3.2, displaying the estimated differences of the impact of the policy for each public school grouping versus the private school control group. Specific year results, along with school fixed effect analyses, are presented in table 3.3.

From table 3.2, it is evident that the average public school results deteriorated in terms of university enrollment rates. However, the reason for this decline most likely results from the

highly negative outcomes of the public schools where the principals were not replaced. Those schools underperformed significantly after the reform, which is apparent from the second regression results. Comparing the second regression results to the remaining regression results seems to confirm this explanation. In particular, public schools with principal replacements had increased university enrollment rates by an average of 4% more than the control schools. Further, the largest part of this increase comes from schools with lottery assigned principals.

Table 3.2. Coefficient estimates of Difference-in-Differences for each treatment category.

Pairwise comparisons of the private and one of the 5 public school categories	Impact of the Policy (With covariates)	Impact of the Policy (Without covariates)	N of Obs.
Private vs I (All Public Schools)	-.051** (.02)	-.050** (0.02)	7353
Private vs II (Public Schools Without the New Principals)	-.059 ** (.02)	-.058 ** (.02)	4767
Private vs III (Public Schools with the New Principals)	.039* (.03)	.041* (.03)	3447
Private vs IV (Public Schools with Bottom 80% Principals)	.050** (.02)	.052** (.02)	2338
Private vs V (Public Schools with Top 20% Principals)	.02 (.02)	.03 (.02)	1790
Notes: Coefficients in all columns are DID regression estimates, robust standard errors are in parentheses; ** and * indicate significance at the 5%, and 10% level, respectively. Covariates are: size, teacher-student ratio, and poverty. 2008 is omitted from the analysis.			

Table 3.3 shows that by 2010 enrollment rates for public school students declined by an average of 5 percentage points after the reform compared to the control group. However, the magnitude of the effect increases in absolute terms for those students who came from the public schools where principals were not replaced. Their performance relatively worsened by 10 percentage points. The estimates in Table 3.3 also suggest that those public schools where the policy replaced the principal comparatively improved their university enrollment rates by an average of 4%. While the schools with the top 20% replacement principals seem not to differ

significantly with the control group, the schools with lottery assigned principals appear to have advanced the most, with a 6% increased rate over control on average.

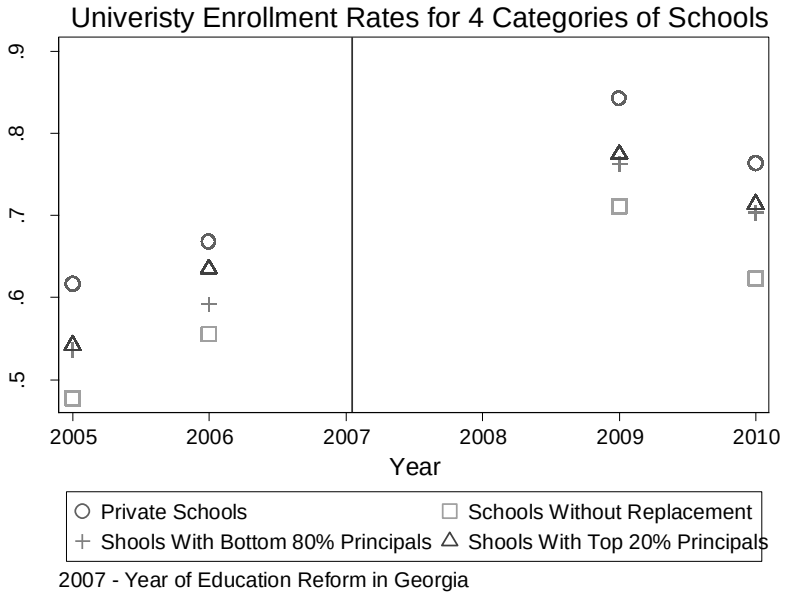
Table 3.3. Coefficient estimates of Difference-in-Differences for each treatment category, with effects with respect to academic years.

Estimation Method	Pairwise comparisons of the private and one of the 5 public school categories	The Impact of the Policy on the University Enrollment Rate				
		Before the Education Policy 2005	2006	After the Education Policy 2009	2010	N of Obs. Adj. R-squared
Difference-in-Differences	Private vs I	-.04 (.032)	-.01 (.02)	-.04** (.02)	-.05** (.02)	7353 .38
	Private vs II	-.03 (.02)	.00 (.03)	-.04* (0.03)	-.10*** (.03)	4767 .37
	Private vs III	-.03 (.3)	-.01 (.02)	.03 (.02)	.04** (.02)	3447 .38
	Private vs IV	-.01 (.04)	-.02 (.04)	.08* (.04)	.06* (.03)	2338 .47
	Private vs V	-.05 (0.4)	-.00 (.04)	-.02 (.03)	.02 (.03)	1790 .54
Difference-in-Differences with school fixed effects	Private vs I	-.06** (.03)	-.03 (.02)	-.03 (.03)	-.05*** (.02)	7353 .42
	Private vs II	-.07* (.04)	-.03 (.03)	-.04 (.03)	-.10*** (.03)	4767 .39
	Private vs III	-.07** (.03)	-.03 (.02)	-.03 (.02)	-.01 (.02)	3447 .52
	Private vs IV	.04 (.05)	.08 (.05)	.04 (.06)	.04* (.03)	2338 (.58)
	Private vs V	-.03 (.04)	-.01 (.02)	-.03 (.03)	.07 (.05)	1790 .59
	Notes: Coefficients in all columns are DID regression estimates, robust standard errors are in parentheses; ***, ** and * indicate significance at the 1%, 5%, and 10% level, respectively. All regressions control the covariate (size, teacher-student ratio, poverty) if the systematic differences are observed over the groups. Year 2008 is omitted from the analysis.					

Figure 3.4 below visually represents the four years of university enrollment rate dynamics for the different categories of public schools from table 3.3. The scatter plots

demonstrate that public schools underperformed in general, while the schools with lottery assigned principals advanced over the years, and the gap between those and private schools narrowed most in 2010.

Figure 3.4. Dynamics of university enrollment rates for each grouping of schools.



Source: The National Assessment and Examination Center of Georgia

In order to estimate the effect of the policy for schools with lottery assigned principals and those which were chosen by the principals, another DiD regression was used in which randomly assigned schools are considered as treatment group. As table 3.4 presents, the average lottery assigned schools outperform the top 20% principal schools.

Table 3.4. DID egression estimates of the effect of the policy reform

Comparison of two types of public schools	Impact of the Policy	N of Obs. Adj. R-squared
Lottery assigned principals vs top 20% principals	.034* (.019)	3447 .39
Notes: Coefficients in all columns are DID regression estimates, robust standard errors are in parentheses; ***, ** and * indicate significance at the 1%, 5%, and 10% level, respectively. Year 2008 is omitted from the analysis.		

To identify the underlying effects and understand the difference in the results from groups four and five from the DID empirical methodology, a regression discontinuity design method was applied using the test score threshold of the top 20% versus the second 20% scorers prior to the reform. That is, the distance from the threshold can go up or down by a maximum of 20% (i.e. the top scorer versus the 40^{th} percentile scorer). Appendix tables 3.A1 and 3.A2 present the RDD regression results, which compare the school-level characteristics for the schools with principals that were given preferential choice and the schools with principals assigned by lottery. The method is conducted around the threshold at both 10% and 20% distances to assess if any differences exist closer to the threshold, while attempting to keep a large enough sample size. Already at the 10% distance, the sample size reaches a questionable lower envelope of 273 observations in total.

The outcomes indicate that the top 20% principals' schools underperformed in terms of university enrollment rates relative to their counterparts, although the result is not statistically significant. Furthermore, at the 10% distance analysis, even the directionality of the effect does not hold. The variable "Distance to threshold" from tables 3.A1 and 3.A2 displays the explanatory power of the score distance from the threshold. While it is strongly statistically significant, the effect is almost zero in real terms.

Covariate balance plots over treatment status are plotted in Figures 3.A3 and 3.A4 in the Appendix. They obviously indicate a selection effect. A graphical representation of the discontinuity is given in figure 3.A1 in the Appendix. It demonstrates that there is a discontinuity at the threshold, but this visual difference is not statistically significant.

3.7 Discussion

One of the most influential factors in student scholastic achievements is principal quality (Branch et al, 2012). Even though a direct link between principals and students usually does not exist, principals impact students heavily through two different channels. First, they can improve teaching quality in the classroom through teacher turnover, regular monitoring, training, and incentive systems for teachers. Second, principals can improve the overall environment at a school, making it more conducive to learning. For instance, they can increase safety measures, improve staff quality, and introduce student achievement incentives. In addition, a good principal may be able to harmonize the cooperation between schools and parents.

In Georgia, the duties and roles of principals are much the same as in most other countries; however, there is a cultural specificity in terms of the education system and schooling. The magnitude of the social connections is probably higher than the international average. As a result of these cultural issues, terminating teacher employment as a teaching quality improvement instrument may not be a viable option for those principals with existing social ties. Antithetically, the bottom 80% candidate principals who were assigned by lottery were usually

completely foreign to the academic and/or social communities of the schools where they became principals.

Based on the intentions and the mechanism of the education reform, the average replacement principal, including those who managed to return to their schools after passing the exam, were of a higher quality than the original principals. Thus, they should improve the average outcomes of those schools. This should be reflected through a more-or-less monotonically increasing improvement, which should, theoretically, be even greater for the higher scoring principals. Since that was not the case, only three reasonable explanations remain as to why the bottom 80% principal schools outperformed all of their counterpart schools:

1) Both the unchanged principals and the top 20% candidate principals are existing members of the academic and/or social community of those chosen schools and are thus unable to enact necessary reforms due to the strong socialization issues discussed above;
2) There is a selection effect in place—the top 20% candidate principals chose schools with higher quality and were then unable to further improve the school's performance despite being high quality principals;
3) The unchanged principals remaining in predominantly lower-performing schools were under-qualified or unmotivated to improve those schools before their anticipated replacement in the second round of the reform in 2011.

While the covariate plots (in the Appendix) and the RDD results of the currently available data seem to confirm that all three of these effects are at work in this case (otherwise there would be a far greater and more significant jump at the threshold) it is not possible to distinguish amongst these effects and their magnitudes.

This lack of identification is due in part to the fact that the 2007 reform was not successful in replacing all the principals in the country's public schools. This failure automatically affected the behavior of the remaining school leaders, particularly since those principles who remained due to the failed process were informed that the government would replace them four years later. The DID regression results show that those schools were seriously negatively affected in terms of the university enrollment rate proxy. This means that this policy reform was detrimental to half of the public schools and their students. While this negative spillover effect of the reform was not intended, it provides one of the key lessons for the designers and initiators of any such future reforms elsewhere.

3.8 Conclusion

This research attempts to elucidate whether a principal really matters and, if so, does leadership make a sizable difference in educational outcomes. Specifically, it evaluates the impact between the quality of a principal (in terms of his/her standardized exam results) and student educational outcomes (in terms of standardized university admissions). It does so by analyzing and numerically documenting the effect of this unique education policy measure (the sharp replacement and random assignment of principals) on the Georgian public school system at the secondary education level. The main finding is that the new principals improved university enrollment rates more than the control schools by an average of 4%, with the majority of this significant increase coming from schools with lottery-assigned candidate principals.

Identifying the effectiveness of this reform might play a significant role in policy-making decisions, particularly in developing countries with similar characteristics and predicaments in their education systems. While there is a relatively sizeable body of research on the effectiveness of principals in OECD countries, to this researcher's knowledge, this work represents the first study in Georgia and the Commonwealth of Independent States (CIS). Further, this paper is important as it provides a pioneer study on a uniquely large-scale and contributes to the understanding of the somewhat elusive area of education economics. The findings can materially contribute to ongoing academic and political debates about how to improve educational outcomes in public schools and could be useful for policy makers in both developing and developed countries. Indeed, it now seems clear that the lottery mechanism may have a significantly greater positive effect than a preference-based allocation of principals and could be a key element in any similar future reforms, particularly in any country where education corruption, political influences, and/or social ties are of concern.

As it is not possible to disentangle the reform's effects with the currently available data, this research should be extended once the reform is completed and sufficient time has passed to allow for delayed effects to occur. A follow up study that would use the outcome data from the second (or additional) phase(s) of this policy reform could then be used to disentangle and properly identify the individual magnitudes of the quality, lottery, and selection effects that make up the current results of the two different groups of new principals described in the study. The results of the extended study could then provide more comprehensive policy design recommendations.

Alternatively, the data used herein could be employed in another vein. The differences in school characteristics chosen just above or not chosen just below the 20% threshold are quite

interesting and informative. There is little compelling evidence in the literature about principal or even teacher preferences, and this data could provide just such an analysis. While this dimension may actually hinder identification herein, using it similarly as in this paper in place of the existing emphasis on the identification of sorting effects on student outcomes, could reveal interesting parts of other economic decisions and incentives. For example, administrative data showing the distribution of principals among schools or even their transitions amongst schools cannot separate demand from supply. The RDD method, however, can compare almost identical principals across the threshold and thus uncover preferences regarding school characteristics. This could illustrate the advantages certain schools have in attracting highly skilled principals, which could also provide important evidence for academic and policy pursuits.

3.9 References

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3.A Appendix

Table 3.A1: Effect of school choice on university enrollment rates for schools with Top 20% and Second 20% scoring principals

	Type of Principal, 1 if Top 20% and 0 if Bottom 80%
University Enrollment Rate	-.01 (.01)
Distance to threshold	.0008 *** (.0002)
Teacher-student ratio	.003 * (.002)
Location	.10*** (.01)
Poverty Ratio	-.22*** (.04)
Notes: Coefficients are RDD regression estimates, robust standard errors are in parentheses; ***, ** and * indicate significance at the 1%, 5%, and 10% level, respectively. Regression includes linear distance to the threshold as well as control variables. Number of observations is 535 and Adj. R-squared equals 0.1. Location refers to the dummy variable which equals 1 if the school is in a city and 0 otherwise.	

Table 3.A2: Effect of school choice on university enrollment rates for schools with principals scoring within 10% above and below the threshold

	Type of Principal, 1 if Top 20% and 0 if Bottom 80%
University Enrollment Rate	.02 (.03)
Distance to threshold	.0005** (.0003)
Teacher-student ratio	.003 (.003)
Location	.11** (.02)
Poverty Ratio	-.27*** (.04)
Notes: Coefficients are RDD regression estimates, robust standard errors are in parentheses; ***, ** and * indicate significance at the 1%, 5%, and 10% level, respectively. Regression includes linear distance to the threshold as well as control variables. Number of observations is 535 and Adj. R-squared equals 0.14. Location refers to the dummy variable which equals 1 if the school is in a city and 0 otherwise.	

Table 3.A3: Regression results when the small schools are dropped/are not dropped from the sample

Pairwise comparisons of the private and one of the 5 public school categories	Regression results when the small schools are dropped from the sample	Regression results when the small schools are not dropped from the sample	N of Obs.
Private vs I (All Public Schools)	-.051** (.02)	-.064 ** (0.017)	7353
Private vs II (Public Schools Without the New Principals)	-.059 ** (.02)	-.085 ** (.018)	4767
Private vs III (Public Schools with the New Principals)	.039* (.03)	.032 * (.03)	3447
Private vs IV (Public Schools with Bottom 80% Principals)	.050** (.02)	.045 * (.02)	2338
Private vs V (Public Schools with Top 20% Principals)	.02 (.02)	.016 (.020)	1790

Figure 3.A1: University enrollment rates for public schools with top 20% and second 20% scoring principals, respectively

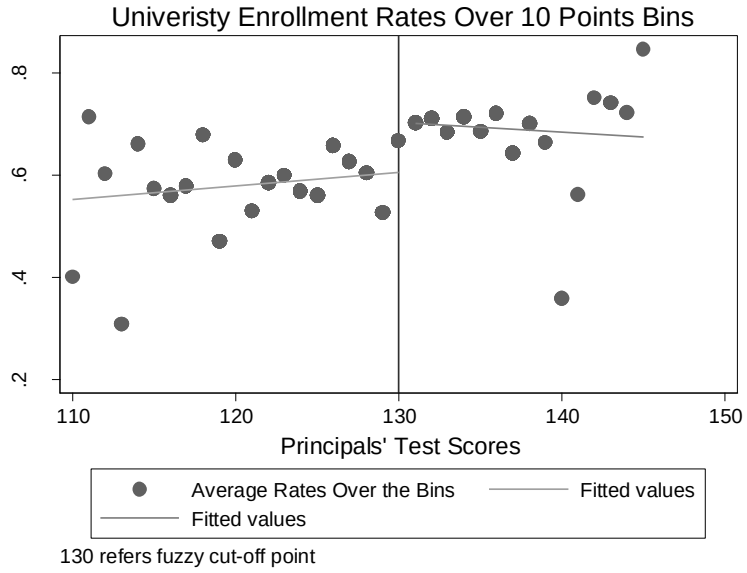


Figure 3.A2: University enrollment rates for public schools with principals scoring within 10% above and below the threshold, respectively

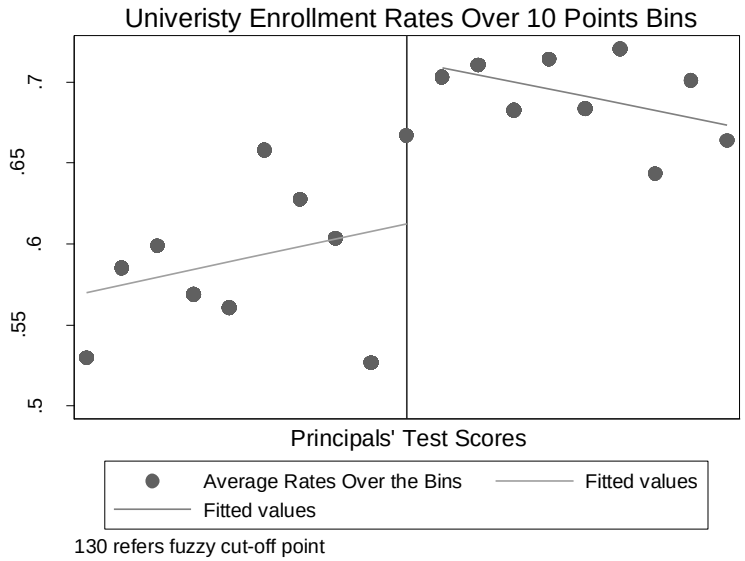
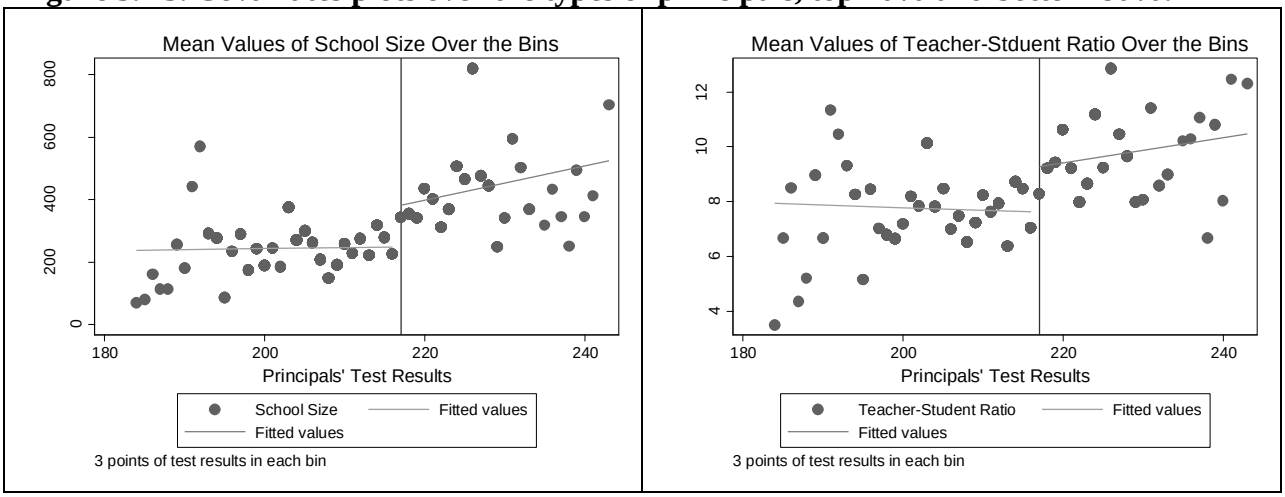


Figure 3.A3. Covariates plots over the types of principals, top 20% and bottom 80%.



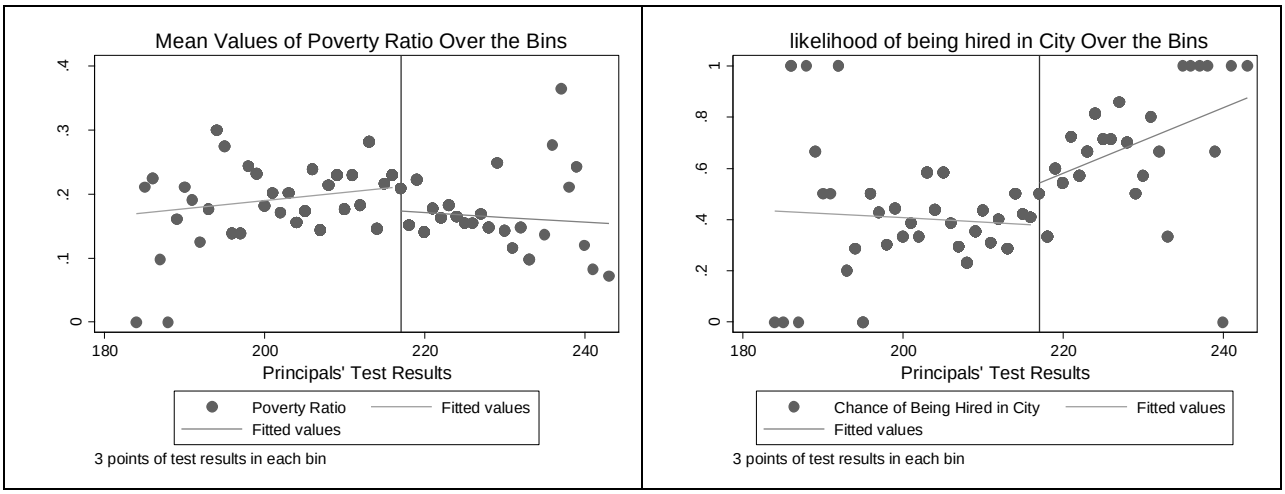
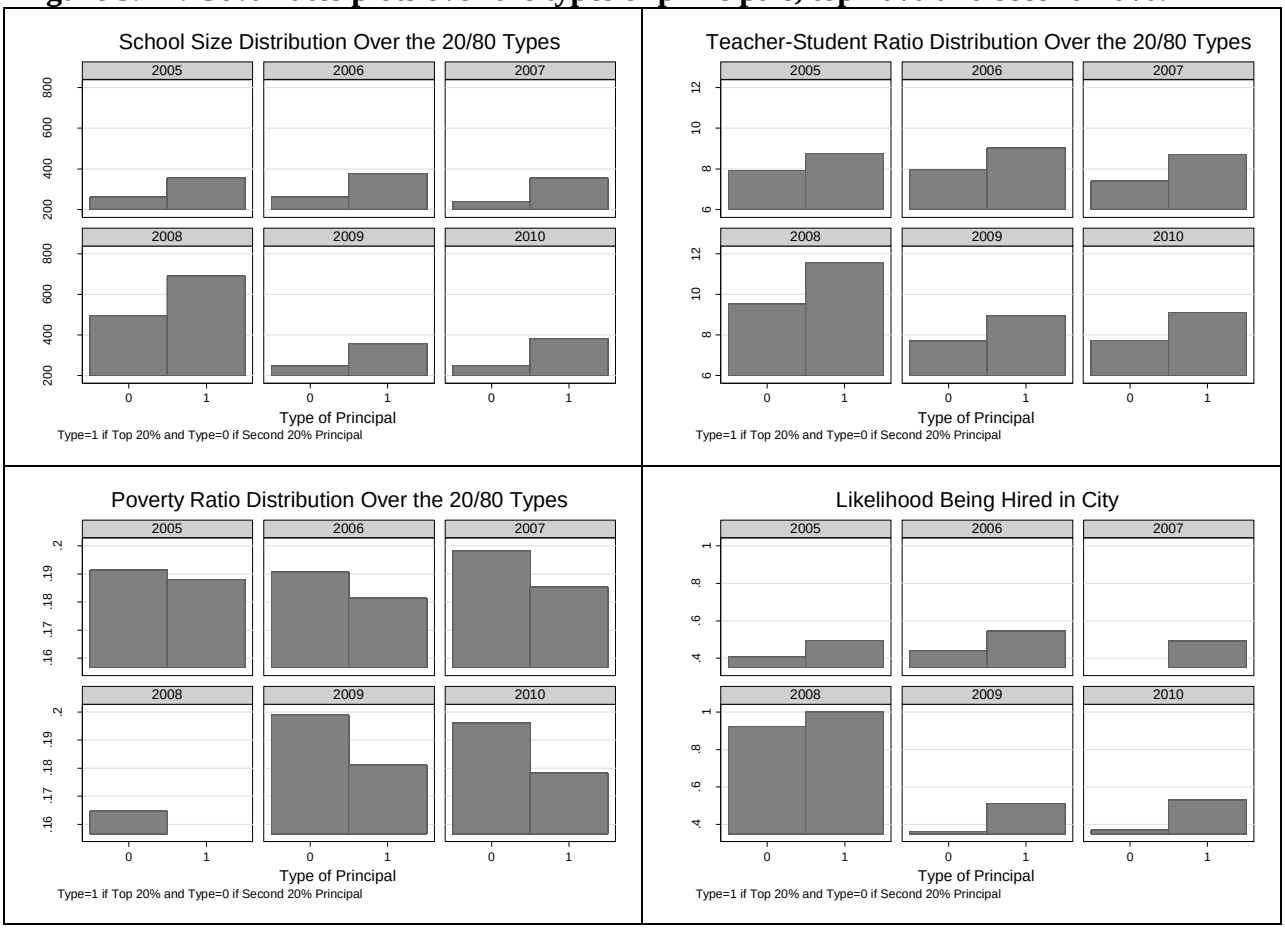


Figure 3.A4. Covariates plots over the types of principals, top 20% and second 20%.



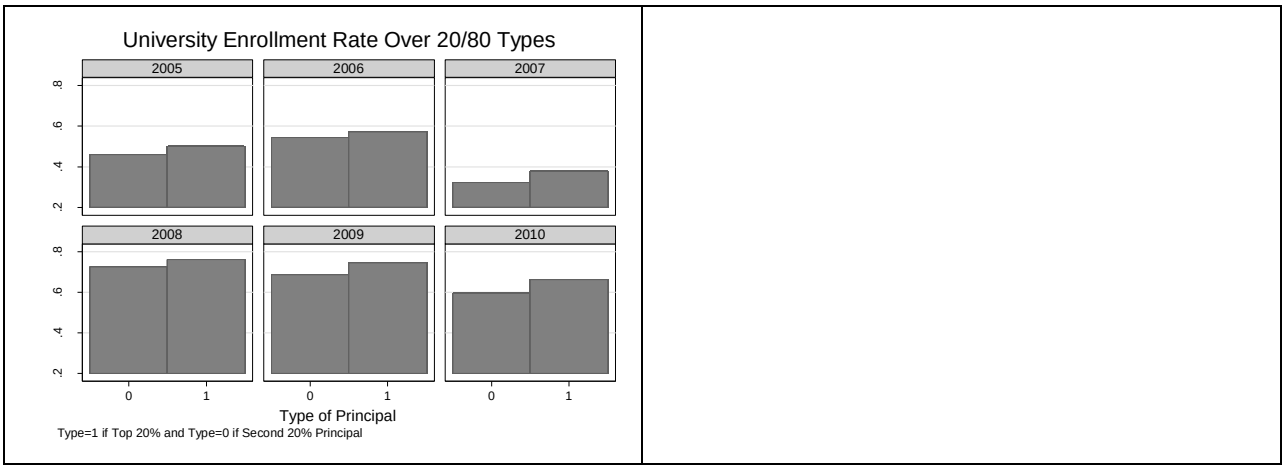
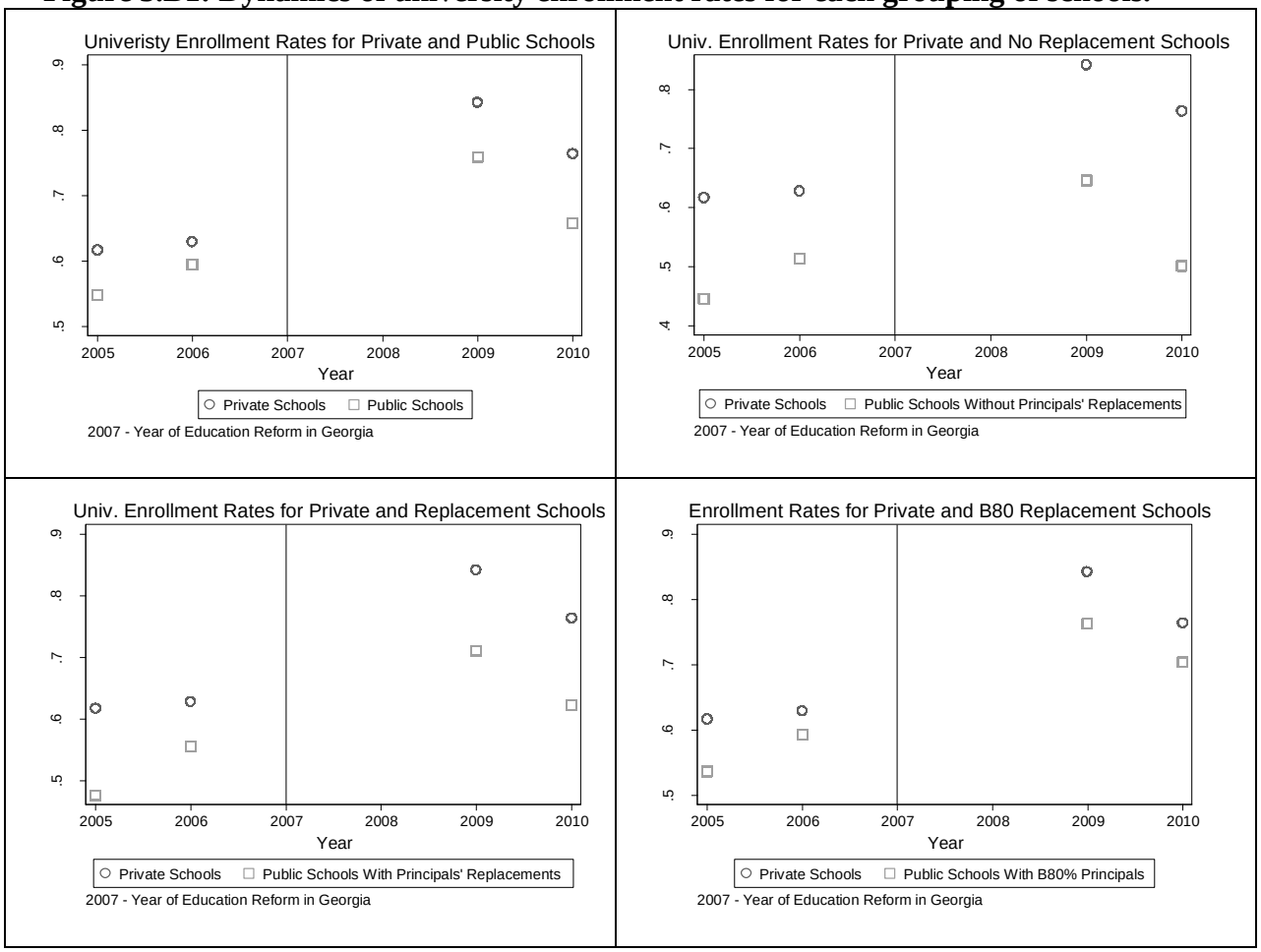
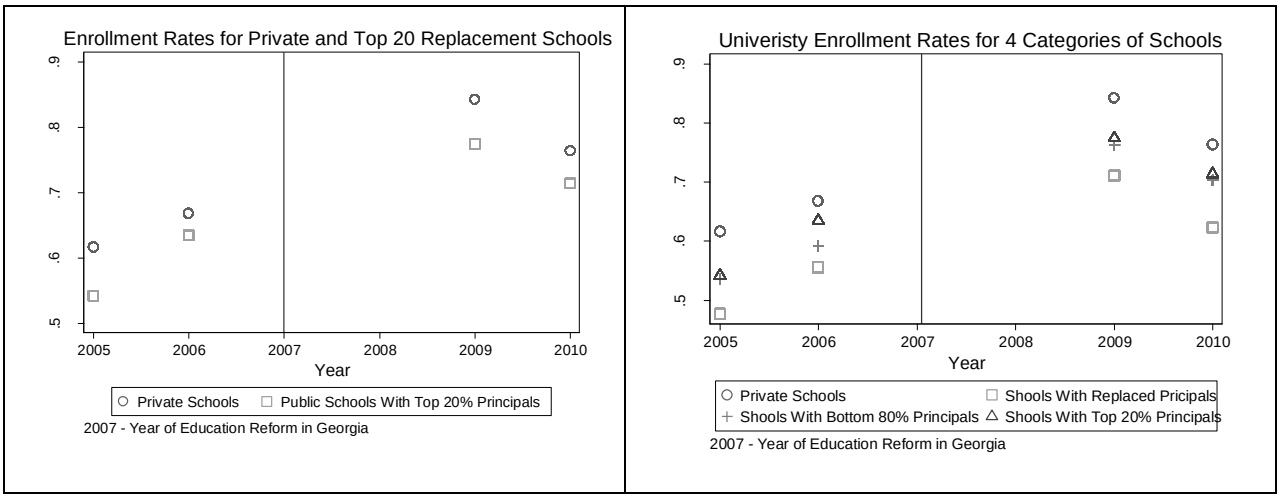


Figure 3.B1: Dynamics of university enrollment rates for each grouping of schools.




Enrollment Rates for Private and Top 20 Replacement Schools
.5
.6
.7
.8
.9
2005
2006
2007
2008
2009
2010
Year
Private Schools
Public Schools With Top 20% Principals
2007 - Year of Education Reform in Georgia
Univeristy Enrollment Rates for 4 Categories of Schools
.5
.6
.7
.8
.9
2005
2006
2007
2008
2009
2010
Year
Private Schools
Shools With Replaced Pricipals
Shools With Bottom 80% Principals
Shools With Top 20% Principals
2007 - Year of Education Reform in Georgia

Summary

The first paper investigates the impact of unconditional cash transfers in Georgia on university enrollment. The program selects recipients based upon a quantitative poverty threshold, which gives us the ability to implement a regression discontinuity approach. We use the data on program recipients from the SSA and on university admissions from the NAEC and combine these into a single dataset. First of all, we observe that the enrollment rate in the sample of poorest Georgian households is very low relative to the national average. We find that being a recipient in the program significantly increases a student's likelihood of university enrollment, by 6.3%. In comparison, Fack and Grenet (2015) report up to a 7% increase in university enrollment as a result of 1500 Euro need-based scholarships allocated to potential university students in France. The large effects of cash transfers on enrollment rates in Georgia are particularly notable. First of all, unlike in France, cash transfers in Georgia were unconditional. Second, the amount of cash transfers to Georgian households, which averaged 46 US dollars for an average family, was notably smaller relative to the 1500 Euro scholarships in France, even when adjusting for PPP and median income differences.

If unconditional transfers have such a strong impact on university enrollment by poor students, then the Georgian government may want to consider further complementary approaches to nudge the poor to invest in skills and education; which may help break the poverty cycle. Furthermore, politicians might also opt for *conditional* transfer programs, such as need-based university scholarships that could encourage students from poor family backgrounds to continue their education. Such measures could reduce the pressure to leave the educational system and start working early with low education levels and correspondingly low productivity and income levels. In fact, such conditional programs could have an even greater positive effect upon education outcomes than the unconditional transfers.

We also find a gender specific effect. While cash transfers increase overall university enrollment rates in Georgia, the effect for males is much stronger than the average effect (13.4% vs. 6.3% in the pre-modification findings and 18.1% vs. 11% in the post-modification findings). We also observe that the impact of cash transfers on university enrollment is stronger for the oldest children in a family. Finally, as noted, the negative coefficient on Tbilisi may be an

indication that cash transfers most effectively help students from rural regions, as the costs of higher education are greater for these applicants. The misallocation of regional talent may in turn adversely impact the overall quality of education and heighten the current skills mismatch in the labor sector[21] in Georgia. Effectively, this would likely lessen the productivity of workers and ultimately generate some degree of welfare loss. Therefore, this study should encourage policy aimed at increasing education outcomes, taking into account gender and location heterogeneity.

The second paper evaluates a social assistance program in Georgia using an original household survey and a regression discontinuity approach. Contrary to our expectations, we find that receiving the program leads to a worsening in (self-reported) basic economic conditions, such as the ability to afford food. We find patterns corresponding to a worsening in economic conditions in the answers to a sizeable number of different questions, though the results are significant only for a subset of those.

These results should be interpreted with caution given the following caveats. First, our sample size is relatively small, both in absolute number and relative to the population. Second, the regression discontinuity approach identifies the local average treatment effect around the threshold, and not the average treatment effect of the program. Third, we are relying on self-reported data, which necessarily involves noise. A potentially greater concern is that program recipients may conceivably be less truthful in their answers if they are concerned about losing the program. Fourth, we are not measuring consumption (not to mention welfare) with precision due to the nature of the questions asked.

To the extent that the recipients' worsening of economic conditions relative to the control is genuine, a natural question to ask is why that might be happening. One possibility is that the program crowds out other sources of income. In principle, the cash transfers could reduce receipts from other social programs, but that is not the case in the Georgian context. More relevant in our context may be reductions in remittances from family members abroad or in Georgia or other forms of private cash transfers. Alternatively, receiving the program could reduce incentives to work, as a previous study of the same program has found (Kits et al. 2013). However, it would require a very large elasticity of these other sources of income to the public

[21]World Economic Forum's Executive Opinion Survey, 2012.

transfers to generate the observed patterns. A very large elasticity may seem implausible, at least in the absence of other contributory factors.

Another possibility is that the recipients invest both the transfers and additional resources in investments in durable goods or human capital. This could then lead to a lower ability to afford food (and other items) in the time window we are observing. In our data we do not observe that recipients own more durable goods than the control group. However, a previous study (Abramishvili & Lanchava 2015) has found that the same program had an effect on university enrollment for the children of the recipients.

Other explanations are possible. The program may change perceptions of conditions rather the conditions themselves. For example, a household may feel better off at first as they are able to consume more, but their preferences and behavior may actually dynamically change over time such that they perceive more goods that they now want but cannot afford (Dasso and Fernandez 2013). Alternatively, the cash transfers may lead to a 'Dutch disease' situation by encouraging consumption of "sin" goods, such as alcohol, tobacco, gambling, etc. (Devereux 2002). Further, it is possible that differential (mis-) reporting by respondents drive our findings (Baird et al 2011). Which (if any) of the explanations suggested here holds has important implications for how the program should be viewed.

Given the limitations of our study, our conclusions should be treated with caution. However, we believe that evidence subject to limitations is better than no evidence at all and that this study can be a useful step in understanding the effect of this program, as well as other cash transfer programs in transition countries. In light of our findings, we recommend that a thorough and extensive evaluation of the social assistance program in Georgia be conducted. For example, researchers could run another, much more extensive round of the survey that would reach a larger portion of the program applicant population as well as be more comprehensive in obtaining a financial assessment of the households similar to the one done as part of the program application.

The third paper attempts to elucidate whether a principal really matters and, if so, does leadership make a sizable difference in educational outcomes. Specifically, it evaluates the impact between the quality of a principal (in terms of his/her standardized exam results) and student educational outcomes (in terms of standardized university admissions). It does so by

analyzing and numerically documenting the effect of this unique education policy measure (the sharp replacement and random assignment of principals) on the Georgian public school system at the secondary education level. The main finding is that the new principals improved university enrollment rates more than the control schools by an average of 4%, with the majority of this significant increase coming from schools with lottery-assigned candidate principals.

Identifying the effectiveness of this reform might play a significant role in policy-making decisions, particularly in developing countries with similar characteristics and predicaments in their education systems. While there is a relatively sizeable body of research on the effectiveness of principals in OECD countries, to this researcher's knowledge, this work represents the first study in Georgia and the Commonwealth of Independent States (CIS). Further, this paper is important as it provides a pioneer study on a uniquely large-scale and contributes to the understanding of the somewhat elusive area of education economics. The findings can materially contribute to ongoing academic and political debates about how to improve educational outcomes in public schools and could be useful for policy makers in both developing and developed countries. Indeed, it now seems clear that the lottery mechanism may have a significantly greater positive effect than a preference-based allocation of principals and could be a key element in any similar future reforms, particularly in any country where education corruption, political influences, and/or social ties are of concern.

As it is not possible to disentangle the reform's effects with the currently available data, this research should be extended once the reform is completed and sufficient time has passed to allow for delayed effects to occur. A follow up study that would use the outcome data from the second (or additional) phase(s) of this policy reform could then be used to disentangle and properly identify the individual magnitudes of the quality, lottery, and selection effects that make up the current results of the two different groups of new principals described in the study. The results of the extended study could then provide more comprehensive policy design recommendations.

Alternatively, the data used herein could be employed in another vein. The differences in school characteristics chosen just above or not chosen just below the 20% threshold are quite interesting and informative. There is little compelling evidence in the literature about principal or even teacher preferences, and this data could provide just such an analysis. While this dimension may actually hinder identification herein, using it similarly as in this paper in place of the

existing emphasis on the identification of sorting effects on student outcomes, could reveal interesting parts of other economic decisions and incentives. For example, administrative data showing the distribution of principals among schools or even their transitions amongst schools cannot separate demand from supply. The RDD method, however, can compare almost identical principals across the threshold and thus uncover preferences regarding school characteristics. This could illustrate the advantages certain schools have in attracting highly skilled principals, which could also provide important evidence for academic and policy pursuits.