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Education, labor markets, and natural disasters

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FREEDOM TO RESEARCH

Dedication

For the kids.

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Contents

List of Figures

List of Tables

1. Introduction

Obtaining quality education is possibly one of the strongest predictors for individual life outcomes and societal welfare. On an individual level better education relates to diverse aspects such as better health, higher success on the labor market, and more favorable fertility decisions as well as improved life outcomes of the offsprings. Education outcomes hence touch every aspect of one's life and the lives of next generations. As such it is not surprising that *quality education* features prominently among the United Nations Sustainable Development Goals.

Today, more children obtain formal education than ever before, more people know how to read or write than ever before, and access to schooling is more equal than ever before. However, despite strong historical progress to improve access to and high-quality education, tens of millions of children and adolescents remain without formal education, a problem which is particularly relevant in less affluent societies. Factors preventing access to education relate to the financial costs of education, for example in form of tuition, testing fees, and uniforms and materials, as well as the distance of schools which relates among other aspects to travel expenses, time costs, and security concerns. These supply side constraints are compounded by demand side factors such as possibly low perceived returns to education and the immediate opportunity costs as the time related to schooling cannot be used to generate income, establish a family, or pursue other 'leisure' activities.

Another element affecting educational attainment is exposure to adverse shocks. On the one hand, idiosyncratic shocks such as the death of the household's principal income generator can yield binding household budget constraints, preventing kids from attending school due to lack of financial means or the need for children to supply labor. On the other hand, also covariate shocks such as natural or man-made disasters can hamper educational attainment. Focusing on natural disasters, their prevalence and intensity are increasing, i.e. more disasters occur, affecting or destroying more human and physical capital. On the supply side this can imply unusable schools, destroyed access paths to schools, or deceased or migrated teaching staff. On the demand side disasters may also affect households' budget constraints or change individual (time and risk) preferences, possibly diminishing demand.

Apart from influencing educational attainment, covariate shocks also affect local labor market dynamics. They can destroy a large share of physical capital, for example structures like residential and nonresidential buildings or local infrastructure. Equipment may suffer, too, such as machinery, computers, or medical tools. On the one hand, the destruction can imply a negative impact on production and trade chains. On the other hand, it yields a drop in labor demand, for example due to destroyed workplaces or given the overall drop in output. The potential negative impacts of disasters on local labor markets relate to possible consequences for the individual monetary value of previous educational attainment. Subject to the type of local destruction, the returns to education can decrease, which possibly depresses the perceived value for of education for future generations as well. Since education outcomes have been shown to improve when increasing perceived returns to education, natural disasters which may ultimately lower perceived returns to education can yield an indirect negative effect on educational attainment, especially in disaster prone regions.

This thesis will touch on the three topics above, concerning access to education, the impact of natural disasters on educational attainment, and the impact of natural disasters on monetary returns to education in the labor market. The work focuses on less affluent societies for three main reasons. First, in the applied economics literature much of our current understanding of (causal) relationships in the areas of education, labor markets, and disasters is still driven by data from wealthy societies. However, lessons drawn from the European Union or the United States of America may not be applicable to a country such as Haiti. Second, in less affluent societies, by and large individual and societal needs are of greater urgency. For example, the overwhelming majority of kids who neither did not do attain education live in poor countries. Lastly, relatively more affluent societies were – and are – the trailblazers and principal causers of actions which led to today's prevalence of climate change and other re-current covariate shocks. However, it is the less affluent societies which bear the majority of the brunt, and hence this is where investigations shall focus on. Methodologically the thesis derives evidence by exploiting experimental and quasi-experimental interventions such as a randomized controlled trial (RCT) and natural experiments related to natural disasters. Below follows a summary of the three main chapters of the thesis.

Chapter (2), co-authored with Melissa Adelman and Peter Holland (both World Bank) considers the aspect of access to education. Using an RCT in Haiti, the impact of a tuition waver program is being assessed. The RCT consisted of providing schools with conditional money which the schools' managements could use for items such as paying teachers, investing in infrastructure, or providing school feeding programs. However, in return the schools are required to wave tuition for the incoming cohorts of students, from grade one to six. Based on school census data the demand as well as supply side of the tuition waver program was investigated.

The results of the evaluation display a strong increase in enrollment after lowering the costs of schooling for low income households in Haiti. This hints to a substantial local demand for education which is partially unmet given high tuition fees at locally available schools. The study also shows that the program reduced grade repetition, the share of students who are too old for their level of schooling, and the number of staff such that the student teacher ratio increased only slightly. The evaluation was highly relevant given the context of an impoverished country with very low educational attainment, its specific target on low income families, and the fact that the program entailed providing nonpublic institutions with public money. Indeed, Haiti is among the poorest countries on Earth showing declining per capita gross national income between 1980 and 2010, possibly due to repeated adverse covariate shocks but also poor institutions and other factors.

The estimates of the evaluation suggest that in response to the treatment, the average number of students who are enrolled in classes 1-4 increased by 88, all else equal and controlling for the commune where the school is located. Based on descriptive statistics the observed treatment effect yields a net-change of plus 62 students per treated school, which – under strong assumptions – yields a cost effectiveness per \$100 of about 0.25 additional years of student participation. This makes the intervention cheaper than the PROGRESA conditional cash transfer program and maybe as cost effective as an information campaign in the Dominican Republic. However, these numbers are only rough estimates. A limitation of the program evaluation was the impossibility to assess if the increase of students is due to students switching schools from control to treatment, if previously unenrolled children did now go to school, or if other factors were crucial. A concern that the observed outcomes are driven by migration in light of Haiti's repeated disaster exposure such as the 2010 earthquake and Cholera outbreak is unlikely to hold due to the design of the program as well as specific regional implementation and location of these shocks as also elaborated in chapter (4).

In a more general context, the results indicate that tuition waver programs may be a means to improve access to education, at least in such specific context. However, it is paramount to appreciate that access to education is but one brick in the wall. Making it easier for families to send their kids to school by lowering tuition does not cover the costs for school uniforms or transport, does not assure that any received education is of high quality, that children are safe at, to, and on their way from school, or that they will obtain degrees which are relevant for the young people's labor market prospects.

Chapter (3), co-authored with Anousheh Alamir (European Center for Advanced Research in Economics and Statistics) directly builds up on chapter (2) but considers the end of the schooling career. In particular, the chapter investigates how the impact of different classes of natural disasters affects adolescents' completion rates of upper secondary education. Building up on previous literature as well as the expected economic impact of different types of natural disasters, three distinct disasters classes are being compared: geological disasters (for example earthquakes or landslides), climatic disasters which affect only living capital (for example droughts), and climatic disasters which can also affect physical capital such as infrastructure (for example floods or storms). Using national census data in combination with DesInventar, an international disaster database, the investigation focuses on Mexico given its wide range of prevalent disaster and its lack of obligatory upper secondary education in the time span under consideration.

The findings of the paper show a substantial decrease in the completion rates of upper secondary education of 17-18-year-olds in response to exposure to natural disasters. Climatic disasters which only affect living capital tend to have the most negative impact, depressing educational expansion by over 40%. In turn, geological disasters yield the weakest effect, lowering the inter-temporal increase of secondary education by about 23%, which is still economically significant. Climatic disasters which can also damage physical capital such as infrastructure range in between. The results are heterogenous across urbanization. The impact of geological disasters is virtually absent in urban settings while the impact of climatic disasters is highest in those places. Notably, the impact of the disasters on educational attainment is not gender-specific, i.e. young women and men are seemingly affected to the same degree.

The observed effects appear to be predominantly driven by demand side effects, including dropping out of school while not entering the labor market, and increasing fertility especially for young women. These effects may be influenced by an observed drop in parental employment and income. Binding household budget constraints may force students out of school or disincentivize them to obtain their degree, given a potential fall in the students' expected returns to education. Supply side effects appear to be driven solely by infrastructure-destructive climatic shocks. On the one hand, via the destruction of infrastructure, including schools and roads. On the other hand, this class of climatic disasters also yields a decrease in the municipal share of teachers, thereby indicating deteriorating learning conditions for students.

Beyond Mexico the study hints at two key elements. On the one hand, the channels how natural disasters affect educational attainment are very diverse on both the demand side as well as the supply side. On the other hand, notwithstanding the fact that different settings will yield different implications, the study shows yet another path how the climate crisis jeopardizes the future of young – and not so young – people by threatening adolescents to build a solid human capital foundation. Hence, increasing resilience or better, tackling the climate crisis at its roots, is extremely urgent.

Chapter (4) is single-authored and goes one step further. It considers the post-education labor market experience by exploring how individual monetary returns to education are affected by exposure to a natural disaster. By building up on economic theory the chapter first derives testable predictions what type of changes can be expected subject to the type of local disaster impact. In particular, the model distinguishes between destruction of high and low skilled human capital, physical equipment capital (for example medical equipment or industrial machines, i.e. complementary to skilled human capital), and physical structures capital (for example roads or buildings, i.e. not complementary to human capital). To investigate the relationships empirically, the chapter returns to Haiti and studies the 2010 earthquake as a natural experiment, using pre- and post-shock household survey data as well as exogenous geological indicators for how strong the earthquake was in different parts of the country. The disaster was devastating and tragic, though it also allows to investigate the given case as it affected both types of physical capital and caused between 63,000 and 220,000 human casualties.

In the given case, theory suggests that the returns to education of individuals will decrease if equipment capital suffers since the complementarity of equipment capital and skilled human capital is expected to lead to a decline of wages for skilled human capital, relative to unskilled human capital. However, the returns to education may increase in case of over-proportional suffering of high skilled human capital, possibly due to increased scarcity of that type of capital in post-shock settings. Destruction of structures capital is predicted to be without effect on the returns to education. The 2010 Haiti earthquake also induced a multi-dimensional shock, affecting both human and physical capital for which economic theory does not yield clear predications.

The quantitative analysis largely confirms the theoretical findings for the one-dimensional shocks. The returns decreased on average by 37% and even more so in the most heavily affected areas. Regions which harbor a lot of equipment-capital intensive industry suffer especially. For dual shocks, the impact on physical capital dominates the effects channeled via destruction of human capital. In the given case higher educated individuals tend to adjust into low-paying self-employment or jobs in the agriculture culture, the lowest paying sector of the country. The returns are particularly shock-sensitive for urban residents, internal migrants, males, and people over the age of 25.

While the theory is tested in a very specific setting, it yields relevant conclusions for Haiti and possibly beyond for other shock-prone settings. Aside from decreasing the monetary returns to education, the potential future occurrence of disasters induces increased uncertainty of future expected returns. This can affect individual and household level decisions concerning future investments into education. As such disaster exposure may constitute another dimension for low educational attainment as well as hidden costs of intergenerational shock transmission.

Lastly, chapter (5) will conclude the thesis. The various aspects of the benefits of educational attainment are widely acknowledged. However, neither the challenges preventing schooling, nor the wide scope necessary to address these challenges are fully appreciated.

Concerning natural disasters, our understanding of their great risks and potential impacts is starting to grow. In particular it becomes increasingly clear that exposure can affect individuals at all stages of their lives, for example during educational attainment as well as in the labor market. The thesis underscores the need for a holistic understanding when tackling low educational attainment and disaster response in less advantageous settings, especially against the backdrop of reoccurring natural disasters and their implications. Notwithstanding the multitude of intertwined challenges, it is important to acknowledge the hard work which is done by many to improve the lives of millions. Global educational attainment is higher than ever in human history, and so are the opportunities that come with it. However, confronting new challenges, sustaining past progress, and sharing the enormous (potential) benefits will be what future generations will judge us by.

2. Increasing Access by Waiving Tuition: Evidence from Haiti†

2.1. Introduction

Despite impressive gains over the last 20 years, the Millennium Development Goal (MDG) for 2015 of Universal Primary Education was missed by a wide margin. It is estimated that in 2011, 58 million primary school age children were still not enrolled. While more than half of these children (nearly 30 million) live in sub-Saharan Africa, there are nearly 3 million out-of-school children in the Latin America and Caribbean region (UNESCO, 2015). About 200,000 of these children live in Haiti, where tuition fees charged by a predominantly nonpublic sector have historically presented a large barrier to families wishing to send children to school. However, progress in Haiti has been steady: by 2010, an estimated 90 percent of primary school aged children were enrolled in school, up from about 60 percent in 2001 (World Bank, 2014a; UNESCO, 2015).

Around the world, both the direct costs of going to school–tuition and other fees, uniforms, transport, books, and so forth–and the opportunity costs, particularly in the form of lost labor for the household, remain barriers to achieving universal primary enrollment and completion. A range of interventions aimed at reducing these costs have been rolled out across regions, falling into roughly four categories: cash transfers to households; vouchers to households to facilitate school choice; providing goods or services that are required for school for free (e.g., uniforms or books); and abolishing enrollment, tuition, and other fees. While these approaches to stimulating demand for schooling may be effective, they may not result in increased enrollment, attendance, and ultimately learning if the supply-side response is inadequate. An outright absence of schools could make demand-side interventions ineffective for the most poorly served communities, while overcrowded classrooms, excessively high student-teacher ratios, and lack of materials could deteriorate the learning environment for all students and deter new students from entering. The literature on the impacts of each type of intervention continues to grow, but broadly

† This chapter is published under the same title in *Comparative Education Review*, vol. 61, no. 4. doi: 10.1086/693904. The paper is co-authored by Melissa Ann Adelman, Tillmann Heidelk, and Peter A. Holland (Adelman, Heidelk, and Holland, 2017).

speaking, the results are positive: enrollment and grade completion increase (although impacts on learning are often zero; Fiszbein et al. (2009); Krishnaratne et al. (2013)).

Research from several countries, particularly in Africa, shows that abolishing school fees over the last two decades has had positive impacts on enrollment in primary school (World Bank and UNICEF, 2009). Gross enrollment rates are estimated to have increased by 73 percent in Uganda, 100 percent in Malawi, and 12 percent in Mozambique shortly after each of these countries declared primary school to be free (Bategeka and Okurut, 2005; Fiszbein et al., 2009; Petrosino et al., 2012). In Kenya, Lucas and Mbiti (2012) exploit prepolicy geographic variation in dropout rates to estimate the impacts of the 2003 abolition of primary school fees, concluding that the program increased access and completion rates, particularly among poorer students. South Africa's experience was less successful, however, likely because of a context characterized by already high enrollment rates and relatively low school fees (Borkum, 2009).

Relatedly, a small but growing literature on the effects of burgeoning private school sectors in many developing countries suggests that low-cost private provision has contributed to increased enrollments and that public financing of such providers may be an effective way to further increase access to education (Dixon, 2012; Andrabi et al., 2015). In the program context most similar to the one evaluated in this article, Barrera-Osorio and Raju (2011) find that enrollment subsidies to private schools in the Punjab province of Pakistan substantially increase enrollments and some schooling materials in participating schools.

In Haiti, over 90 percent of primary schools were nonpublic in the early 2000s, and the vast majority of these schools charged tuition. Coupled with the cost of mandatory uniforms, books, and other inputs, the direct costs of schooling were prohibitively high for many families. Estimates showed that total average costs (direct and indirect, including uniforms, transportation, and feeding) of sending a child to school were about definition $131 per child per year (Merisier, 2004; World Bank, 2007). At a time when gross national income (GNI) per capita was estimated to be about $400, nonpublic education was essentially unaffordable. In addition, financial constraints were also believed to be a leading cause of the problem of overage students in school, as parents may have had to cycle children in and out of school, depending on their cash flow. The average age of students in grade 6, for instance, was 16 years old according to the 2003 school census, when the corresponding age for that grade should be 11 or 12 years old.

Given the importance of schooling costs as a barrier to access and the large role of the nonpublic sector, a demand-side response in the form of a program to abolish school tuition fees was initiated in 2007. This article utilizes the experimental nature of the program, whereby schools were randomly selected to participate from among qualified applicant schools, to estimate the impact of this program on school enrollments, student-teacher ratios, grade advancement, and other indicators at the school level.[1]

We believe that the results of this evaluation are of interest beyond Haiti, as key features of the Haitian system–low state capacity and weak public delivery of education services–are common to many low-income countries, while the rapid growth of nonpublic schools in many of these countries makes the Haitian case, with a large and vibrant nonpublic sector, increasingly relevant. The next section of this article describes the Haitian context and the program itself; the following section describes the program randomization, data and estimation method, and results; and the final section presents conclusions.

2.2. An Approach to Increasing Access: Tuition Waivers

2.2.1. The Haitian Context: Many Nonpublic Schools and Out-of-School Children

Haiti's education system today stands out for its preponderance of private actors, the result of a rapid expansion of private provision throughout the latter part of the twentieth century (Easton and Fass, 1989; Demombynes et al., 2010). Though Haiti was one of the first nations worldwide to state that education should be free and compulsory in the country's founding constitution of 1805 (Haggerty, 1989), the actual delivery of education as a public service to the majority of the population failed to materialize. As Fass (1988) details, Haiti's educational system in the nineteenth and early twentieth centuries was characterized by periods of modest public expansion interspersed between long eras of government neglect. An accord between the Catholic Church and the government, as well as the growth of Protestant missions, contributed substantially to expanding school supply, and net primary school enrollment is estimated to have peaked at about 15 percent at the turn of the century. Following the turbulent period of US occupation, primary school enrollment grew roughly 1-2 percent annually through the first half of the twentieth century, with the number of public and nonpublic schools growing at roughly similar rates (Fass, 1988; Demombynes et al., 2010). With the onset of the Duvalier regimes in 1957, public expansion continued its slow pace, while nonpublic provision grew dramatically. Between the early 1960s and the early 2000s, the number of public primary schools in Haiti increased from about 800 to about 1,300. In contrast, the number of nonpublic primary schools grew from about 800 to nearly 14,000 during the same period (Demombynes et al., 2010). Consequently, in the early 2000s, public schools accounted

[1] While these indicators relate to education access and conditions within schools, the data available do not allow an examination of learning outcomes.

for fewer than 10 percent of primary schools but served about 20 percent of all primary students because of their larger average size.

Nonpublic schools are a highly heterogeneous group. They include religious schools, community schools, schools started by nongovernmental organizations (NGOs), and for-profit schools. The impetus for their creation varies. A historical trend of religious groups motivated by proselytization and charity opening schools accelerated under the second Duvalier regime of "Baby Doc," who instituted a rule that any newly opened church was required to also create a school. By the early 2000s, religiously affiliated schools, including affiliations with local and international churches and religious NGOs, accounted for about 45 percent of nonpublic primary schools. In other cases, the motivation was a desire to serve the less fortunate through secular NGOs, while others were at least partly driven by profit-seeking and established schools as for-profit enterprises. This diverse group of privately run secular schools accounted for about 40 percent of all nonpublic schools. Finally, for some the motivation was necessity, given the state's limited ability to provide services, especially in rural areas. Under these conditions, community leaders often organized to respond to the latent parental demand for schooling. These community schools accounted for the remaining 15 percent of all nonpublic primary schools.[2]

Despite the rapid and diverse growth in the nonpublic school sector, by 2005, the primary net enrollment rate in Haiti was estimated to be as low as 50 percent, far below the sub-Saharan Africa average of 70 percent.[3] This low rate was believed to be directly related to the costs of education: according to the 2002-3 Haitian School Census, over 90 percent of all nonpublic primary schools charged tuition, averaging about US$20 per student per year (equivalent to 5 percent of GNI per capita) plus other fees (for enrollment, exams, and so forth). Consequently, large numbers of children were out of school or overage for their grade (e.g., in preschool rather than primary; Cayemittes et al. (2007)). If the country was to make rapid progress toward the goal of universal primary education, Haiti's government and society needed to increase access, which meant, at least in the short term, working with the nonpublic sector.

2.2.2. Determinants of Educational Demand and the Tuition Waiver Program

In the classical economic formulation, education is considered an investment, with demand for education resulting from a rational weighing of the costs against the expected stream

[2] For a more complete discussion of the evolution of the Haitian education system, see Demombynes et al. (2010).

[3] The estimate from Haiti comes from Cayemittes et al. (2007), while the sub-Saharan Africa average comes from the World Bank's Global Education Statistics, available at `http://databank.worldbank.org/data/home.aspx`.

of future benefits (Becker, 1962). The benefits, or returns to education, are primarily measured in terms of higher wages in the labor market and globally have been estimated to be substantially higher than average returns to alternative investments (Psacharopoulos and Patrinos, 2004). Important nuances to this formulation have been developed in recent years, as some authors have pointed out that returns to education extend far beyond higher wages and can include better health and happiness, while others have made the case that the perceived returns to education, rather than actual economywide averages, actually drive decision making (Jensen, 2010; Oreopoulos and Salvanes, 2011).

For the poor in Haiti, evidence suggests that perceived returns to primary education have been quite high for several decades. As described in Easton and Fass (1989), Haitian parents value primary education as their children's best possible route out of poverty, as a prerequisite for further education and professional employment, or as a de facto minimum requirement for international migration.[4] These beliefs have been reaffirmed by more recent qualitative research with parents in Haiti, who believe education is critical to realizing their children's full potential and for enabling their children to later help provide care in their old age (Jayaram, 2015).

However, low economic growth and the many market distortions that contribute to poverty kept effective demand, and thus enrollment rates, low. With nearly one-third of Haitians living in extreme poverty in 2000 and the scarcity of public schools, paying for primary education did not make economic sense because of the extremely high marginal value of money, or was simply not possible (World Bank, 2014a).[5] As has been extensively documented in Haiti and globally, the poor constantly face high-stakes financial decisions, such as borrowing money at very high interest rates to survive liquidity shortages (Easton and Fass, 1989; Collins et al., 2009). In these contexts, investing in education could be extremely costly or nearly impossible for many families. In addition to the direct monetary costs of schooling, opportunity costs of losing out on children's labor (in the household or in the marketplace) and distance to schools (particularly for rural families) also serve to reduce effective demand.

At the time when the intervention in Haiti was designed, a USAID-financed initiative had just issued a report synthesizing these perspectives and identifying the primary determinants of household demand for education throughout the developing world (Educational

[4]Easton and Fass also describe the monetary consumption benefits of education for poor families in urban Port-au-Prince in the late 1980s: many nonpublic schools offered students school meals and social insurance for their families, creating a large net benefit to poor families who contributed tuition payments. However, these benefits seem to have become much less common: by the 2002-3 school year, only 12 percent of nonpublic schools reported offering school meals.

[5]It is important to note that in Haiti, as in many developing countries, public education was not entirely free: parents were required to make a small fee payment of 100 Haitian gourdes per school year until 2011, when this fee was abolished. However, the costs associated with attending–uniforms, books, transport, etc.–are still borne by parents.

Quality Improvement Program II, 2007). On the basis of more than 120 separate studies, mainly in low-income countries, the authors highlighted four major factors driving or repressing demand: expected monetary and non-monetary returns to education; direct costs in the form of tuition and fees, uniforms, and materials; opportunity costs such as lost child labor; and distance to schools (plus related safety concerns), which impose additional direct or indirect costs on households in the form of transport expenses, time spent by adults taking children to school, or psychological costs of worrying about children. Although there was much variation due to contextual factors such as local market demand for child labor, the main conclusion was that household demand for education is highly sensitive to cost–and, as expected, poorer households tended to be more sensitive in this regard than more prosperous ones.

Beyond household calculations of costs and benefits, there is also a strong economic rationale for public financing of primary education. Much recent economic research documents how individual acquisition of education benefits society by making individuals more engaged and responsible citizens and better parents, as well as by potentially increasing the overall level of productivity and growth in the economy (Moretti, 2006; Oreopoulos and Salvanes, 2011). To the extent that individuals do not take these benefits into account when making investment decisions, government funding can increase the efficiency of investment in education.

In order to exploit what was perceived order to meet the high latent demand for primary education among the poor, the government of Haiti launched a new program aimed at increasing access by lowering the costs of attending school, with financial and technical support from the World Bank and the Caribbean Development Bank (CDB).[6] The program, called the Programme de Subvention (and referred to as the "tuition waiver program," or TWP, in English), provides an annual per-student payment to participating nonpublic schools that agree not to charge any form of tuition fees to students. The TWP was first rolled out in the Nippes and Artibonite Departments n 2007.[7] Its primary objective was to reduce the direct costs that families experienced in sending children to school and thereby increase both enrollments and average duration of schooling.[8]

The theory of change behind the program is that if the direct costs of education are substantially reduced in nonpublic schools, this releases a binding demand-side constraint, allowing more parents to send their children to school and enable them to stay there. The approach of focusing on nonpublic schools was taken because of the public sector's

[6] The program would later be financed by other donors as well, including the Canadian International Development Agency (CIDA), the Global Partnership for Education (GPE), and the Inter-American Development Bank (IDB).

[7] Departments are the first-level administrative division in Haiti. The country is divided into 10 departments.

[8] The TWP is part of the Stratégie National d'Action/Education pour Tous (SNA-EPT).

limited size, and in order to exploit what was perceived to be existing excess capacity in nonpublic schools because parents could not afford to keep their children in the nonpublic institutions through the full 6-year primary cycle.[9] In addition, in areas where schools were already full, it was expected that the tuition waiver incentive would trigger a supply response by private actors, encouraging them to start or expand nonpublic schools.

This theory of change was confirmed in the results of focus groups conducted by NGOs contracted to conduct independent program verification with parents of students attending treatment schools. As part of annual verification, the NGOs randomly selected five to 10 participating schools per department in which to organize focus groups with parents of students attending participating schools.[10] These parents reported that the TWP had significantly reduced the financial burden of educating their children and helped alleviate their concerns about how to keep their children in school (Fondation Haitienne de l'Enseignement Prive, 2013; Fonds de Parrainage National, 2013). Many parents reported episodes prior to the TWP of their children having been sent home from school for days, weeks, or even months because of lack of payment and the stress and unhappiness such episodes had caused their entire families. Importantly, while parents consistently expressed the importance of the program to their and their children's well-being, they also reported continued financial pressures from the costs of schooling that they continue to bear, including uniforms and transport.

Participation of schools in the TWP is subject to several conditions, aimed at preventing opportunistic efforts to profit from the program, improving the learning environment, and improving grade-for-age accordance. First, in order to qualify, schools need to have a permit to operate from the Ministry of Education. Second, only children entering grade 1 for the first time, between the ages of 6 and 8, are eligible for the subsidy.[11] Each entering grade 1 cohort is then supported for the subsequent years through grade 6, subject to compliance with the program rules. Third, in addition to not charging parents any fees, schools are required to provide students with at least three school textbooks. Finally, there is a limit of 45 children per class and a maximum of two classes per grade that can benefit from the program per school.

Compliance with these conditions has been verified annually by independent organizations hired by the ministry to carry out technical audits. All participating schools are subject to the technical audit. This has revealed that, in practice, the program has

[9]Observation of dozens of nonpublic primary schools in Haiti by the authors finds that the first three primary grades usually have two to three times as many students as the last three, because of high rates of repetition and dropout in the early grades.

[10]In general, NGOs requested that school directors contact eight to 12 parents per school to participate in the focus groups. Given that directors had discretion about whom to invite, the focus groups may not capture the full range of parents' perspectives and experiences.

[11]This is a potentially binding constraint in many cases, as the average age of a new first grader is nearly 8 years old (World Bank, 2014a).

been implemented fairly faithfully according to its design, though not without its problems. As with nearly all development projects in Haiti, implementation suffered from a number of delays, which often resulted in schools receiving their payments late in the school year. While the majority of schools have largely complied, the technical audits find that most fail to fully comply with at least one of the conditions. Most common of these is the failure to provide students with the requisite number of books (more than a quarter of participating schools in the Department of Artibonite in the 2014-15 school year), although cases have also been documented in which parents have been charged fees (7 percent of schools in Artibonite in 2014-15) and the 45 student-teacher ratio limit has not been respected (9 percent in Artibonite in 2014-15). However, the ministry has taken little to no action to enforce the conditions (either by encouraging compliance or by sanctioning noncompliance).[12]

The amount of the subsidy was set at US$90 per student in 2007, well above estimated average school tuition to account for the extra costs associated with providing textbooks, limiting class size, and improving school inputs. Covering these indirect costs was deemed of paramount importance to ensure a basic level of quality in schools.[13] The subsidy amount has not changed since.[14] Funds are managed by school management committees (SMCs), constituted by the school director, the president of the parents' committee, and representatives of teachers. Funds are transferred to dedicated bank accounts opened in the name of the SMCs, with signing authority given to both the school director and the president of the SMC. The money can be used for any of 10 purposes outlined in the operational manual, including paying teacher salaries, small rehabilitation projects, and school feeding programs.

In order to target poor communities, the program relies on self-selection into participation by schools serving poor families. The level of the subsidy at $90 per student is well below the level of tuition charged by those schools serving children in upper-income quintiles, and they therefore self-select out of the program. On the other hand, many schools serving poor children stood to benefit from participation, since they could earn more per child through the subsidy than by continuing to charge low levels of tuition. With these additional resources, it was expected that schools would invest in improvements to

[12] For more updated information on program performance, see the publicly available implementation status reports available through the World Bank website (`http://www.worldbank.org/projects/P124134/education-all-project-support-second-phase-education-all-program?lang=en`).

[13] While annual data on school tuition are not available, as shown in tables (2.1) and (2.2) below, tuition was approximately $20 in 2003 and rose quickly over time to reach approximately $75 in 2012. Data on textbook prices from 2007 are not available; a back-of-the-envelope calculation using average prices in 2016 discounted for inflation suggests that in 2007, primary school textbooks cost roughly $2 per book, implying that the subsidy amount would have easily covered the purchase of the three required books.

[14] As mentioned in n. 13, rapidly rising tuition fees have eroded the value of the subsidy (which is paid in the Haitian gourdes equivalent of US$90).

benefit students, such as upgrading poor infrastructure, hiring more effective teachers, and providing more pedagogical materials. However, participating schools did not receive any specific support from the Ministry of Education to help guide their investments, and other than meeting the program conditionalities and spending within the outlined purposes, schools were effectively free to spend the subsidies received as they saw fit.

As evidenced from the annual reports compiled by the NGOs contracted to conduct independent verification, the expenditure categories were respected by participating schools, with the largest amount allocated to teacher salaries. Nearly all participating schools spend part of the subsidy on textbooks for students, and nearly 90 percent spend resources on the infrastructure and materials side, namely, making small upgrades to classrooms, purchasing school furniture, improving roofs, and fixing doors and windows (Fondation Haitienne de l'Enseignement Prive, 2013).

2.3. Measuring Results

Using national school census data, we provide answers, albeit partial, to two critical questions regarding the program's impacts. First, did the program meet its primary objective of increasing poor children's access to schooling? While a direct answer to this question is not possible because of the lack of detailed enrollment data, we examine the size of student populations, as well as rates of progress through school, as proxy indicators for the impact of reducing financial barriers on access. Second, did program participation, and the extra resources it entailed, improve learning conditions within schools? To address this question, we utilize available measures of various school-level characteristics related to teachers, materials, and infrastructure as indicators of the conditions students experience within schools.[15]

2.3.1. Initial Randomization, Identifying Schools across Years, and School Survival

In the school year 2008-9, because of a limited amount of program financing, the Ministry of Education and the World Bank agreed that only approximately 100 schools from each of the five newly participating departments in the country would be randomly selected among the 1,034 qualifying schools that applied across all five departments (Centre, Grand-Anse, Nord-Est, Nord-Ouest, and Nord). A total of 547 schools were selected to participate: 110 in Centre, 115 in Grand-Anse, 112 in Nord-Est, 111 in Nord-Ouest, and

[15] Certainly, the ultimate objective of providing access to school is to increase students' learning. Unfortunately, data on student learning are not currently available but may be the subject of future research.

99 in Nord. The random selection of schools was carried out in a meeting in Port-au-Prince attended by the nine-member steering committee of the tuition waiver program.[16] In the meeting, the director of the Ministry of Education's Department for Private Education and Partnership (DAEPP) used Microsoft Excel to assign a random number to each qualifying applicant school. Schools were then sorted by their random number, with a separate list for each department, and those with the lowest random numbers were selected to participate (up to the allocated total for each department, which had been assigned on the basis of the number of schools that applied). The randomization was conducted so that at least one school from each commune of each of the departments was represented in the sample, in order to favor rural areas and maximize the geographical distribution of the program.[17] Education indicators have historically been worse in rural areas; for example, as of 2012, children living in rural areas are two percentage points less likely to be in school and 19 percentage points more likely to be over-age for their grade compared to children living in urban areas (World Bank, 2014a). All applicant schools were then notified through letters from the Ministry of Education on whether or not they had been selected to participate, and all selected schools in fact chose to participate.

Following a large economics literature, we exploit the "natural experiment" created by the random selection of schools into the TWP to estimate the program's impacts. This literature takes advantage of naturally occurring random variation–ranging from acute weather events to policy decisions–to estimate the causal impacts of a range of factors on outcomes of interest. While this approach is attractive given its low cost and ease relative to a purposeful randomized experiment, major challenges with the interpretation of such estimates lie in the validity of the assumption of randomness and in the generalizability of the results (Rosenzweig and Wolpin, 2000). In the case of the TWP, the selection procedure was purposefully random (and is assessed below). However, it is important to note that qualifying schools are not representative of the totality of schools, but rather represent those schools that meet the eligibility criterion of having a permit to operate from the Ministry of Education. In particular, as shown in table (2.1), qualifying schools that applied to the TWP charged slightly lower tuition, had larger student bodies, and were founded somewhat earlier than the average nonpublic school. At the same time, the schools are similar on student-teacher ratios and the number of shifts taught. On balance,

[16] The steering committee comprised representatives from the Ministry of Education (2), Ministry of Finance (2), nonpublic education sector (2), teachers union (2), and national association of parents (1).

[17] In practice, this meant checking ex post that at least one school from each commune had been selected into participation, and in cases in which this was not true, to manually add the school with the lowest random number from each unrepresented commune and remove the school with the highest random number that had been selected into participation (assuming another school from the same commune was already included). The geographic divisions of Haiti are department, arrondissement, commune, and section communale. There are 140 communes in the country.

while any results may not be applicable to all schools, they would likely be relevant to the broader population of non-public schools in Haiti.

Table 2.1.: School sample compared to all nonpublic schools

	Mean Values Observed in 2003	
	Qualifying Applicant Schools	Nonpublic School Average
School founding year	1984	1988
Average annual tuition (Haitian gourdes)	581	777
Number of shifts taught	1.0	1.0
Total number of students	191	124
Student-teacher ratio	30	30
Observations	354	13,983

Source: World Bank staff estimates using Haiti National School Census, 2002-3.

The available waves of the national school census (2002-3 and 2011-12) provide information on school staffing and infrastructure, as well as the size of the student population.[18] Because the schools were randomly selected when the program was expanded in the 2008-9 school year, estimates of the program's impacts on these measures can be made. As mentioned above, a direct measure of the program's results in terms of its primary objective–increasing the number of children in primary school–is not possible. Such a measure would require baseline and follow-up data on overall school enrollment in local communities where qualifying schools are located, and these data were not collected. In addition, the approach of selecting at least one school per commune to provide access to the program across a wider geographic area also ensures that program schools are not closely clustered, making it more difficult to measure effects on area enrollment rates.

In order to assess the success of randomization and so the extent to which causal impacts of the program on schools can be estimated, the 1,034 qualifying schools that applied were identified first in the 2011-12 school census and then matched back to their entries in the 2002-3 school census. Given the time lapse between the census waves and program rollout, the rapid turnover of nonpublic schools in Haiti, and the lack of census identifiers in the data collected from qualifying schools, only 64 percent of the schools chosen in 2008 were identified in the 2011-12 census.[19] In the next step, 55 percent of these schools

[18] Perspectives on the quality of the school census data are provided below. The 2011-12 census was conducted less than 2 years after the January 20, 2010, earthquake. While the five departments studied here were not directly affected, population movement following the earthquake may have affected the schools in the study. This is particularly true in Centre, the closest of the five departments to Ouest.

[19] Identification is done manually using commune, school name, and school address. Spelling variations and slight differences in information recorded in the TWP application data and across census years

were then matched back to the 2002-3 census. In other words, 35 percent of the 1,034 qualifying schools are observed across the two census waves (table 2.2).

Table 2.2.: School sample and identification rates

	Number of Qualifying Applicant Schools in 2008		% Identified in 2011-12 School Census		% Matched to 2002-3 School Census	
	Treatment	Control	Treatment	Control	Treatment	Control
Nord	99	114	84	66	55	56
Nord-Est	107	139	76	28	56	33
Centre	110	59	65	53	44	74
Grand-Anse	113	58	80	55	49	44
Nord-Ouest	110	115	75	58	63	69
Total	539	485	76	50	53	57

Treatment and control schools were equally likely to be identified in the 2002-3 school census and had similar characteristics, suggesting that randomization was successful. Since baseline data were not collected in 2008, we use the 2002-3 school census data as a proxy for baseline, or pretreatment, data. Following randomization in 2008, an initial test for balance was conducted in which 64 percent of treatment and 68 percent of control schools were successfully identified in the 2002-3 school census. These results, presented in the appendix, also demonstrated that treatment and control groups were observationally equivalent at that point. However, the fact that a large share of both treatment and control schools were not matched back to the census data is a cause for concern. There are three likely causes for the low matching rate: (a) the lack of consistent unique identifiers for schools; (b) the generally low quality of the census data; and (c) the fast-changing nature of the education sector, where nonpublic schools frequently change their names and/or locations. While data collection approaches have varied somewhat over time, in general, the school census in Haiti is carried out manually, with school directors completing paper questionnaires with the support of local inspectors, who then send these questionnaires to Port-au-Prince for processing and data entry. The manual nature of the process and the lack of consistent error-checking procedures are believed to substantially reduce the overall quality of the data. In aggregate, these factors create a major challenge for consistent identification of schools. While this adds appreciable "noise" to the exercise, it is not likely to have introduced a major bias, since these factors are assumed to have applied to treatment and control schools equally.

Treatment schools appear more likely than control schools to have remained open through the 2011-12 school year. As shown in table (2.2), schools participating in the TWP were

required that each school be manually identified in the 2011-12 census and then matched back to the 2002-3 census.

much more likely than control schools to be identified in the 2011-12 census: 408 of the 539 (76 percent) participating schools were identified, compared to 244 of the 485 (50 percent) control schools. However, we cannot say with certainty that the schools not identified in the census had closed down. As mentioned above, without unique identifiers, tracking schools over time becomes very difficult, and even small changes in name and street address can cause schools to fall out of the sample. There are two factors that may account for the different degrees of success in identifying treatment and control schools in the 2011-12 census. First, having received consistent financing for several years, and knowing that they could count on continued transfers through the TWP for the next few years, participating schools may have been more likely to remain open, in the same location and with the same "branding," than control schools. Second, because treatment schools had more contact with the Ministry of Education through the TWP, they may have been more readily locatable when it came time to conduct the census. However, because the TWP and census are run by separate departments within the ministry and one has no implications for the other (e.g., no data from the census are used for the TWP), this seems unlikely to be an important factor.

Across both census waves, the low rates of school identification are likely driven by both random and nonrandom attrition, as well as sources of error in the data. As discussed above, the lack of consistent school identifiers and the poor quality of the school census data contributed to limiting the proportion of schools successfully matched, and these issues seem likely to have had similar effects on treatment and control schools. Treatment also affects attrition: there is a 26 percentage point difference in the identification rate between the treatment and control schools with the 2011-12 census. This differential survival may have implications for the measures of the program's impact on school characteristics: as in all randomized experiments, attrition that is correlated with the treatment can bias impact estimates. In this context, survival depends in large part on a school's ability to attract students and operate in a financially sustainable manner. If attriting schools are those that would have had the fewest students, then the impact estimates may be understated.

Using the 2002-3 school census, a test for balance between the treatment and control schools that are identified in both census waves also suggests that even after selection (only nonattritors are observed) the groups appear to have been observationally equivalent prior to the program. Schools are compared across five key types of characteristics measured in the school census: scope (levels taught and number of shifts), infrastructure (building materials, water, latrines, etc.), physical materials (desks, chairs, blackboards, etc.), staffing, and students (number, gender, and repetition rates).[20] As shown in ta-

[20]Following Kling et al. (2007), indices are constructed to measure infrastructure and physical materials (separately) in order to avoid spuriously significant correlations due to a high number of variables. Each index is constructed as the equally weighted average of z-scores of each included variable, where

ble (2.3), treatment schools were founded earlier than control schools, on average, and have slightly more students in grades 5-6 (the final two grades of primary school). Schools founded earlier may be more well regarded in the community, in part because they may have been more likely to have educated children's own parents. While this factor did not create a significant difference in enrollment prior to the TWP, it may have helped parents decide to send their children to treatment schools once the program started. Along all other dimensions, treatment and control schools observed in both waves of the school census (and therefore having survived from 2002-3 through 2011-12) were observationally equivalent in 2002-3.

2.3.2. Results: Changes in School Characteristics

To estimate the effects of the program on participating schools, we compare characteristics of treatment and control schools using the 2011-12 school census. Because qualifying applicant schools were observationally equivalent in the 2002-3 school year and were randomly selected into (treatment) and out of (control) program participation, any subsequent differences observed between average characteristics of treatment and control schools can be plausibly attributed to participation in the program. This causal impact of the program on school characteristics can be estimated from a set of simple ordinary least squares regressions of the following form:

$$Y_s = \alpha + \beta T + X_s + \epsilon_s, \tag{2.1}$$

where Y_s is the outcome of school s as measured in the 2011-12 census, T indicates assignment to the treatment group, and X_s indicates the commune in which school s is located, in order to account for the fact that the probability of selection into the program differed across communes (Duflo et al., 2006). Each regression measures the impact of T (program participation) on a different Y_s outcome indicator. Results are presented in table (2.4), in which each row corresponds to the estimate of β for each regression.[21]

The TWP permits a fairly wide range of potential uses of the subsidies, as discussed above, so directors of treatment schools could have invested in improving existing infrastructure, expansion, staffing, furnishings, learning materials, and so forth. Our results are limited, however, to the data available in the school census on the five areas of school characteristics measured in the census: scope (levels taught and number of shifts), in-

more beneficial outcomes have higher scores. The infrastructure index includes variables measuring the existence of a sports field, library, director's office, recreation area, kitchen, latrines, water, electricity, as well as the quality of roof, wall, and floor materials. The materials index includes variables measuring the number of desks, chairs, and blackboards.

[21] Results run on the 652 schools matched only to the 2011-12 school census (rather than to both 2002-3 and 2011-12) are very similar to those presented in table (2.4).

frastructure (building materials, water, latrines, etc.), physical materials (desks, chairs, blackboards, etc.), staffing, and students (number, gender, and repetition rates).

Table 2.3.: Comparison of treatment and control groups

	Treatment	Control	p-Value
School founding year	1981	1986	.004**
Average annual tuition (Haitian gourdes)	611	562	.578
Scope:			
Number of school levels taught	2.1	2.1	.698
Number of shifts	1.0	1.1	.177
Infrastructure index	−.04	−.09	.147
Materials index	−.03	−.06	.445
School has a canteen (%)	17.9	18.2	.941
Staffing:			
Number of staff	6.35	5.93	.215
Share of male teachers	.69	.73	.154
Student-teacher ratio	35	37	.422
Students:			
Total number of students	198	186	.382
Grades 1-4: total number of students	152	148	.713
Grades 1-4: number of female students	71	73	.711
Grades 1-4: number of male students	81	75	.351
Grades 5-6: total number of students	47	38	.083*
Grades 5-6: number of female students	22	19	.156
Grades 5-6: number of male students	24	19	.059
% repeaters grades 1-6	.15	.14	.761
Observations	354		

Source: World Bank staff estimates using Haiti National School Census, 2002-3.

Note: Following Kling et al. (2007), the infrastructure and materials indices are the equally weighted average z-scores of each component, where more beneficial outcomes have higher scores. The infrastructure index includes measures of a sports field, library, director's office, recreation area, kitchen, latrines, water, electricity, roof, wall, and floor materials. The materials index includes measures of desks, chairs, and blackboards. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Across departments, treatment schools did not change the number of shifts offered, nor did they change the levels of school taught. The majority of treatment and control schools teach one shift per day and offer preschool and primary. The treatment does not appear to have had substantial effects on prompting improvements in infrastructure and physical materials: both coefficients are positive, but only the materials index is marginally significant. It is important to note that the average level of infrastructure and materials is low in both participating and control schools; for example, the majority lack sufficient chairs and desks for all students. The number of staff did increase by nearly one person, on average, in treatment schools.

Table 2.4.: Estimated impacts of treatment observed in 2011-12 school year

	$\hat{\beta}$	Standard Error
Scope:		
Number of school levels taught	−.01	.04
Number of shifts	−.01	.01
Infrastructure index	.05	.05
Materials index	.09*	.05
Staffing:		
Number of staff	.93**	.38
Share of male teachers	−.42	2.28
Students:		
Student-teacher ratio	8.3***	1.68
Total number of students	84***	14.53
Grades 1-4: total number of students	88***	11.66
Grades 1-4: number of female students	41***	6.14
Grades 1-4: number of male students	47***	5.92
Grades 5-6: total number of students	1.0	4.71
Grades 5-6: number of female students	0.0	2.74
Grades 5-6: number of male students	1.0	2.30
% repeaters grades 1-6	−.10***	.02
% overage grades 1-4	−.09***	.03
% girls overage grades 1-4	−.10***	.03
% boys overage grades 1-4	−.09***	.03
% overage grades 5-6	−.06*	.03
% girls overage grades 5-6	−.07**	.03
% boys overage grades 5-6 Observations	−.06*	.03
Observations	354	

Source: World Bank staff estimates using Haiti National School Census, 2011-12.

Note: Following Kling et al. (2007), the infrastructure and materials indices are the equally weighted average z-scores of each component, where more beneficial outcomes have higher scores. The infrastructure index includes measures of a sports field, library, director's office, recreation area, kitchen, latrines, water, electricity, roof, wall, and floor materials. The materials index includes measures of desks, chairs, and blackboards. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

The number of students, male and female, in 2011-12 is substantially higher in treatment compared to control schools. Notably, this increase is limited to grades 1-4, which correspond to the four cohorts funded by the TWP from 2008 through 2011; no increase is observed in higher grades, which were not yet funded by the program, which had been running for only 4 years (figure 2.1). Across departments, treatment schools have, on average, 88 more students in grades 1-4 compared to control schools. This large and significant increase indicates the strong demand from families for education at lower cost. However, we do not know what share of the additional students came from other schools (including control schools) and what share had not been in school previously. In other words, a simple comparison of the student population between treatment and control schools as an estimate of the program's impact on the number of children in school could be misleading, as the stable unit treatment value assumption is violated: the participation of some schools in the TWP almost certainly affects the student population at control schools (Angrist et al., 1996).

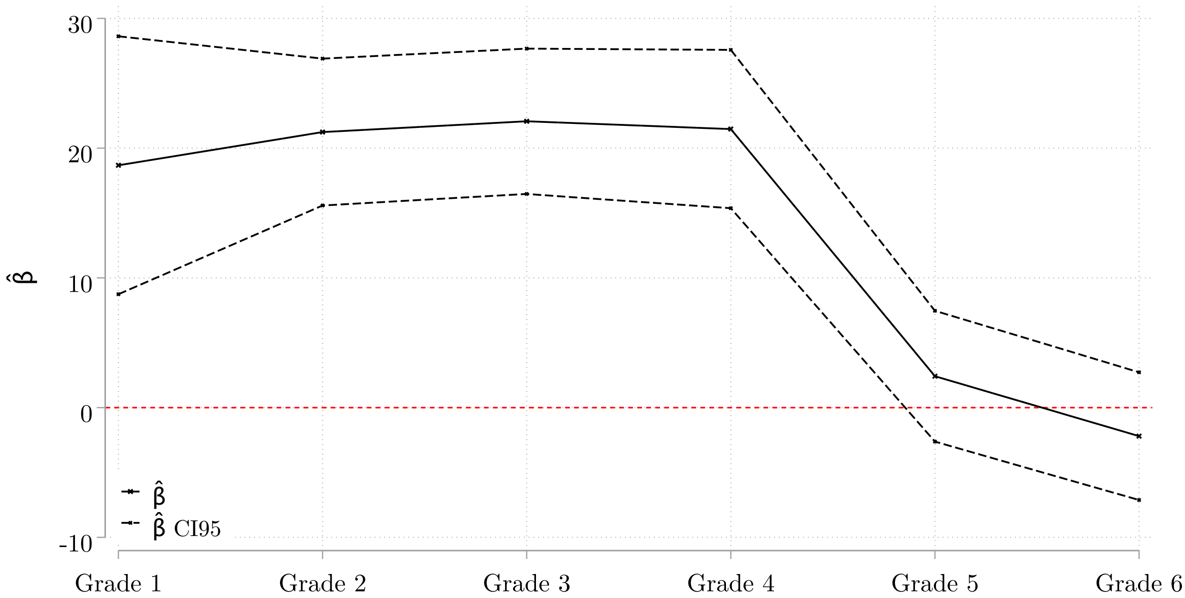

Figure 2.1.: Impact of TWP on class size in grades 1-6.

Figure (2.2) shows that between 2002-3 and 2011-12, the average number of students grew substantially in treatment schools while declining slightly in control schools across departments. Overall, the growth in treatment school student populations is larger than in control schools. Treatment schools gained an average of 78 students, while control schools lost 16 students. However, a share of the control schools not identified in the 2011-12 census may have closed down as a result of competition from treatment schools. Because treatment schools were geographically dispersed by design (with one per commune), 95 percent of control schools were located in the same commune as at least one treatment school. Communes (the second-largest administrative level) are fairly large, and even the least populated communes have over 12 primary schools. Therefore, it is not possible to conclude to what degree the program had an effect on overall enrollment in the communities affected.[22] It is worth noting, however, that at the national level, the

[22] In order to assess the impact of treatment schools on control schools' survival, more geographically disaggregated data at the time of program randomization would be needed.

net enrollment rate at the primary level increased from approximately 50-60 percent in the early 2000s before the program to something like 70-80 percent in 2012 (based on data from the 2001 ECVH (Enquête sur les conditions de vie en Haïti), the 2005 and 2012 Demographic Health Survey (DHS), and the 2012 Enquête sur les Conditions de Vie des Ménages Après le Séisme (ECVMAS)).[23]

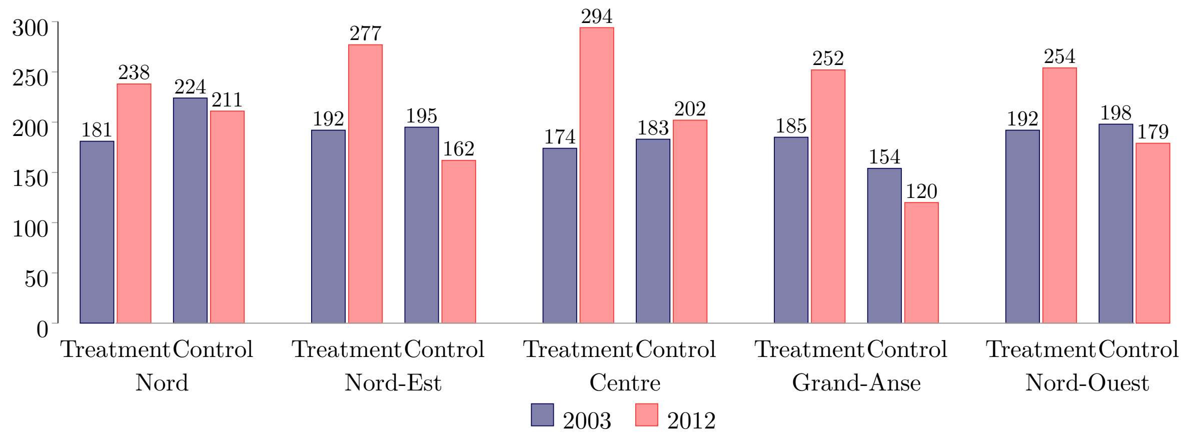

Figure 2.2.: Total number of students (grades 1-6) in treatment and control schools (2002-3 and 2011-12).

Encouragingly, the percentage of primary students 2 or more years over-age for their grade fell by 10 percentage points in the treatment group. This is driven in part by lower repetition rates across grades, and these effects are similar for boys and girls. These impacts suggest that the reduced financial burden may allow students to participate in school more consistently. In addition, since the program funds only students who are within 2 years of the prescribed age for grade, schools also have an incentive to ensure that children do not repeat a grade multiple times. Interestingly, the effects are also seen in grades 5 and 6, not yet funded by the program, suggesting a potential spillover effect on nonsubsidized cohorts. Specifically, families may be better able to finance the education of older siblings when younger siblings are covered by the program, allowing them to stay in school more consistently and not have to repeat grades.[24]

Finally, given the large increases in student numbers, the student-teacher ratio increased by eight on average; the average ratios remain under 40 in treatment schools across four of the five departments, well below the limit of 45 students per class mandated by the

[23] The 2001 ECVH measured net primary enrollment at 60 percent, the 2005 DHS at 50 percent, the 2012 DHS at 77 percent, and the 2012 ECVMAS at 72 percent (Institut Haïtien de Statistiques et d'Informatique, 2003; Cayemittes et al., 2007, 2013; World Bank, 2014a). Differences in these measures arise both because they are sample surveys and because each was conducted in different months and years.

[24] Another potential cause for impacts on grades 5 and 6 repetition rates and share of students overage is that schools utilized the resources of the program to improve the learning environment across grades, such that student learning and grade passing rates increased. However, given the lack of effects on measures of the learning environment, this possibility seems less likely.

program (figure 2.3). This increase in ratios may be a cause for concern, as the results of well-identified research, using natural experiments or randomization, suggest that class size differences of this magnitude can affect student learning and long-term outcomes, though the issue continues to be debated.[25]

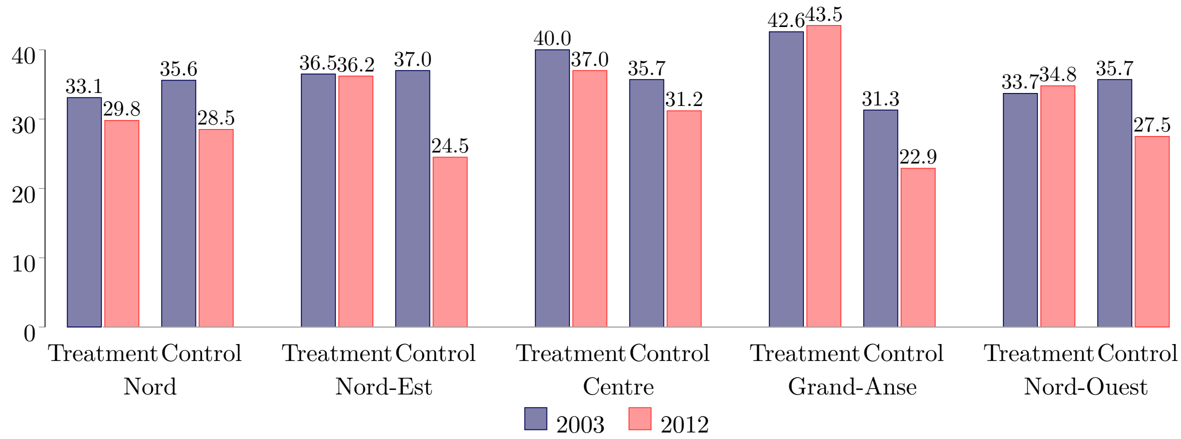

Figure 2.3.: Average student-teacher ratios (grades 1-6) in treatment and control schools (2002-3 and 2011-12).

2.4. Conclusions

This article has sought to evaluate an important program in Haiti designed to increase primary school access in the context of low state capacity by provision of tuition waivers that reduce the direct costs to the families of poor children. Our purpose is both to inform future policy development and practice in Haiti and to share lessons with those countries that still have large numbers of children outside the education system.

Prior to considering the results, the process of the study itself highlights several important lessons regarding the importance of collecting and using good-quality data for program evaluation, as well as policy making in the education sector as a whole. When the TWP started in 2007, 5 years had already passed since the last school census and the last population census. Since no additional data were collected for the program, government officials and program managers had limited reliable information on which to base decisions for allocating resources geographically. This constraint may have affected the program's effectiveness. For example, as the program's primary stated objective was to increase access, a data-driven approach to targeting the TWP to communes with the highest shares of out-of-school children could have increased its overall impact. In addition, the many limitations of the school census data, discussed above, coupled with

[25]See Angrist and Lavy (1999); Krueger (1999); Hoxby (2000); Chingos (2010). Also see Duflo et al. (2015); Walters (2015).

the fact that they are not made publicly available, means that they are rarely used by researchers, constraining the production of knowledge on the education sector in Haiti. Without both reasonably good-quality data and useful research on key aspects of the education sector, policy makers and other actors have often been left to “fly blind” in their decision making, likely to the particular detriment of poor families. In the post-earthquake period of the last 5 years, increased attention has been given to data in many sectors including education. Three nationally representative household surveys have been conducted, a new population census is being planned, and several international organizations are supporting the Ministry of Education in particular to strengthen its data systems at all levels. However, continued financing and technical work from the government and its development partners will be needed for many years to come in order to achieve sustained improvements.

Despite the challenges regarding the data, we are able to conclude that a school’s participation in the TWP results in more students being enrolled, more staff, and higher student-teacher ratios. The program also reduces grade repetition and the share of students who are overage. While the increase in students at treatment schools cannot be proven to represent an actual reduction in the number of children out of school, it does demonstrate strong demand from families for their children’s schooling. This demand is also evident in the results of focus groups conducted by NGOs, as previously discussed in the second section.

The program therefore seems to be achieving its objective of reducing the financial barriers to primary education faced by families in Haiti and may be supporting higher enrollment rates. Despite the program’s estimated effectiveness, it has faced many operational challenges. Improvements in these areas–including enforcement mechanisms to create accountability for schools that do not provide all the inputs required by the program–could further increase the effectiveness of the program.[26] At the same time, abolishing fees cannot address the full set of financial barriers that keep children out of school in Haiti. The costs of uniforms, books and other materials, and transport are substantial, as are the opportunity costs for many households, who rely on children for domestic work or other labor. Addressing these issues requires more comprehensive social programs, such as the cash transfer schemes that exist in many Latin American countries (UNDG, 2010). Moreover, participation in the program does not appear to improve learning conditions within schools, as measured by the availability and quality of infrastructure and materials. Therefore, moving beyond providing access to a classroom to ensuring that students learn will require greater investment, in terms of the waiver value, conditionalities, support provided to schools, and accountability.

[26]See, e.g., a recent evaluation funded by the Inter-American Development Bank (Forstmann and Cuenin, 2014).

The cost-effectiveness and financial sustainability of the TWP are additional important aspects to consider. If the net increase in student enrollment observed between treatment and control schools in fact represents students who otherwise would not have been in school and if all other students do stay in school regardless of the program (admittedly, two strong assumptions), then \$100 in waiver value results in an additional 0.25 year of student participation–outcomes that are in line with those found for several other interventions such as merit scholarships in Kenya (Dhaliwal et al., 2012; Evans and Popova, 2014).[27] However, this rough back-of-the-envelope calculation does not account for the program's administrative costs on one hand nor the impact of the program on temporary and permanent dropout on the other. At a broader level, there is strong economic motivation for public financing of primary education as a public good, and this approach (public financing of private providers) may be more cost-effective and quicker than expanding public supply in Haiti. Reliable data on the costs of educating primary students in the public sector are not available, but building new public schools, a priority after the earthquake, has moved slowly and at high cost, and many costly inefficiencies continue to exist in the public system (Haiti Ministry of Finance, 2012).

A cautionary note is in order regarding the critical role of strong oversight mechanisms for achieving these positive results. In 2011, the Haitian government initiated a program called PSUGO (Programme de Scolarisation Gratuite, Obligatoire, et Universelle–Program for Free, Obligatory, and Universal Schooling) with a component modeled closely after the TWP. Under this component, thousands of nonpublic schools received subsidies financed by national tax revenues to educate children tuition free. However, PSUGO lacked the TWP's strong monitoring processes and suffered from numerous cases of fraud, undermining its credibility and contributing to the government's decision to phase it out starting in 2014.[28] This experience highlights the fact that governments' role in education in no way diminishes when financing and provision are separated–public management and oversight become perhaps all the more important.

Taken together, the results from Haiti have several potential implications for policy to increase access further afield. Many countries with high numbers of out-of-school children have conditions similar to Haiti: these states' ability to provide services is low (either because of weak capacity or because of a lack of authority over parts of their territory), and they face problems preceded by political instability, climate change, or both. For example, UNESCO estimates that half of those out of school are living in conflict-affected countries. In these situations, private actors are often crucial to service delivery, and the

[27] Treatment schools gained an average of 78 primary students while control schools lost an average of 16, for a net gain of 62 (assuming no schools closed because of competition from the program). The estimate provided in the text assumes that all primary students in a treatment school (an average of 270 during treatment) must be provided a waiver in order to observe the net gain.

[28] The primary driver for the government's decision was that the program had expanded too quickly and did not have sufficient resources to cover its commitments.

government's role may be limited to financing the service and establishing and enforcing the governing mechanisms. This role is not an easy one to fulfill effectively, particularly in low-capacity settings, but with sufficient attention and resources it can be done. The success of the Haitian TWP in the face of multiple implementation challenges adds credence to the notion that public financing of nonpublic provision of services is a viable and promising approach for reaching those children still outside of the system. This bodes well for getting more children into school, keeping them there, and getting them to completion. Such results are certainly not enough to transform education in countries like Haiti, given the broader systemic challenges that such initiatives cannot address, but they represent a necessary and encouraging start.

As private schools catering to the poor multiply quickly, research on their impacts is growing in response, but many questions remain to be answered. From 1990 to 2010, the share of children in low-income countries enrolled in private schools has doubled from 11 to 22 percent (World Bank, 2014b). Across Africa and South Asia, where these trends have been strongest, evidence suggests that these schools are relatively low cost, provide an education at least on par with public schools, and can increase enrollment rates (Andrabi et al., 2015). Going forward, several questions remain for researchers, regarding both the role of the private sector in basic education and the best approaches to addressing the unfinished agenda of universal enrollment.

Regarding the role of the private sector, two key questions for future research stand out. First, given the current realities of rapid growth, how can governments effectively regulate and in some cases finance private education providers? There is a strong economic rationale for not "picking winners" by financing individual schools in communities or particular school chains, but rather making financing available to all private providers in a community and supporting information dissemination to enable parents to make their own choices (Das, 2016). However, evidence for how this works in practice is needed to help guide policy making. Second, what effects does a growing private sector have on segregation and inequality in education and on the public education sector? Answers to this question are not straightforward and are likely to depend on many country-specific factors, but research to address them is very much needed to inform the broader policy discussion around what should be the role of the private sector in providing basic education (Dixon, 2012; Aubry and Dorsi, 2016).

Finally, much more systematic research is needed on effective approaches to bring the remaining out-of-school children into the education system. These children are often the most vulnerable in society and include children facing extreme poverty, disability, and conflict. If the world is to meet the new, more ambitious goal of universal, quality education for all children under the Sustainable Development Goals, learning how to reach and educate these children will be critical.

3. Natural disasters and educational attainment‡

3.1. Introduction

Natural disasters affected around 217 million people every year since 1990, causing damages of US$ 143 billion per year from 2001 to 2010 (Guha-Sapir et al., 2012). The Center for Research on the Epidemiology of Disasters (CRED) defines a natural disaster as "*a situation or event that overwhelms local capacity, necessitating a request at the national or international level for external assistance; an unforeseen and often sudden event that causes great damage, destruction and human suffering*" (CRED, 2018, p. 8).

The disaster literature typically classifies those covariate shocks as either 'geological' or 'climatic' (Skidmore and Toya, 2002; Raddatz, 2009). Geological events, such as earthquakes and tsunamis, are most often caused by shifts in tectonic plates and seismic activity. They have been affecting humanity since its inception (Kozák and Čermák, 2010) and tend to be infrequent and hard to predict. On the other hand, scientific evidence has shown that human-induced climate change has considerably increased the likelihood and intensity of weather-related 'climatic' events, including droughts, floods, and wildfires (National Academies of Sciences, Engineering, and Medicine, 2016). This class of events tends to re-occur in the same locations (Raddatz, 2009) and during specific periods of the year (Skidmore and Toya, 2002). The two classes of disasters thus differ in both their root causes and predictiveness, though they can both be highly destructive.

Past analyses have tried to compare macroeconomic 'costs' of these disaster classes in order to understand which class would need international help first (Albala-Bertrand, 1993; Klomp, 2016; Raddatz, 2009; Skidmore and Toya, 2002). Conflicting results, however, have made this literature largely inconclusive. Such analysis is also crucial at a smaller scale given that when disasters occur, local authorities are faced with budget constraints that translate into the need to prioritize. This issue even persists in countries where municipalities can rely on emergency funds once their financial capacity is surpassed. It is notably the case in Mexico where the growing occurrence of climatic events over the past century (Gobierno de la república, 2015) has meant that their Natural Disaster Fund

‡This chapter is co-authored by Anousheh Alamir and Tillmann Heidelk.

(*Fondo de Desastres Naturales*, FONDEN) frequently faced liquidity constraints, thereby preventing it from responding to all funding requests (de Janvry et al., 2016). This thus calls for a detailed microeconomic analysis in order to decipher which disaster class affects individuals, households, and communities most strongly and which mechanisms drive these effects.

Among the various demographic, economic, and social indicators that can be impacted by natural disasters, this paper concentrates on educational outcomes, given their potential long-lasting consequences for the affected individuals. Although previous studies have shown the effects of specific disaster types or episodes on various schooling outcomes, a dedicated comparison of the impact of geological *versus* climatic is lacking.

Using Mexican census data and disaster information from the Disaster Inventory System for Mexico (*Sistema de Inventario de Desastres*, DesInventar) and the Emergency Events Database (EMDAT; Guha-Sapir et al., 2015), this study compares the effects of three classes of disasters: geological disasters (for example earthquakes; "GEO"), climatic shocks which affect living capital and are not infrastructure-destructive (for example droughts; hereinafter "LIV"), and climatic disasters which can damage physical capital such as infrastructure (for example floods or storms; "PHY"). Mexico offers a unique setting to pursue this investigation. First, its geographic characteristics led to a wide range of impactful disasters including earthquakes, hurricanes, floods, and droughts. Second, Mexico's nationally harmonious schooling system allows for comparisons across its regions. Furthermore, upper secondary education only became compulsory in 2012 (OECD, 2013a). Until then, Mexican adolescents had several outside options, making them an optimal case study for what can be expected in the absence of strict schooling laws when the observed types of events occur.

Empirically, the variations in space, time, and intensity of disasters are exploited to construct exogenous disaster exposure indicators for each municipality. Following a wide applied literature which analyzes the impacts of natural disasters on life outcomes, this paper employs a difference in differences framework to investigate the disasters' effects on upper secondary attainment rates of 17 and 18-year-old individuals.

Results show that all disaster types have a significantly negative effect on education. LIV disasters have the strongest impact, decreasing the inter-temporal educational attainment growth rates by over 40%. PHY disasters yield a lower impact, followed by GEO disasters, with a decreasing effect of about 23%. Hence even the least strong impact is economically significant. However, the impact of GEO shocks seems to fade away in highly urbanized areas. As pointed out by Porfiriev (2009), this may be explained by the fact that more urban settings typically have better logistics and media support. And as geological disasters tend to be more destructive (Klomp, 2016), they are more likely to get priority in receiving emergency funds in those areas. For all disaster classes it appears that the effects are solely driven by the extensive margin (i.e. when using a dichotomous indicator), and not the intensive margin (i.e. when using a continuous measure counting the number of disasters). The results underscore the increasingly high urgency to tackle the climate crisis and improve resilience, especially against the backdrop of increasing rates of urbanization.

Regarding the channels, the findings seem to be predominantly driven by demand side factors such as leaving school while not entering the labor market and increasing fertility, especially for young women. The effects may be influenced by deteriorating parental labor market outcomes via binding household budget constraints or perceived decreases of the value of education, from the perspective of the students. Supply side effects only appear in the case of infrastructure-destructive climatic shocks.

The paper contributes to the literature in five ways. First, it provides a comparison between different classes of disasters which adds to the understanding of future trends and helps in managing policy priorities. Second, the analysis distinguishes heterogeneous effects across urbanization rates when comparing the impact of the various disaster classes. Third, the article focuses on educational attainment which is possibly more relevant than enrollment or years of schooling for life and labor market outcomes. Given the implied destruction by disasters and changing returns to different factors of production, attainment rates can be expected to increase or decrease. The focal point is 17-18-year-olds, an age group with possibly attractive outside options. Fourth, given the varying results found in the macroeconomic literature, the paper contributes to the cost analysis of natural disasters by exploring their possible long-lasting effects through schooling. Fifth, demand and supply side channels for the observed effects are explored to enhance the comprehension of the local context and possible applicability of the findings to other settings.

This article proceeds as follows: section (3.2) introduces the related literature. Section (3.3) provides background on Mexico, its education system, disaster exposure, and emergency framework. Section (3.4) presents the data and empirical approach employed. Section (3.5) shows the main results and section (3.6) tests their robustness. Section (3.7) explores the channels that could explain the results. Section (3.8) concludes.

3.2. Literature review

The literature on natural disasters goes back to the 1950s (Lemons, 1957), although recent concern about how the climate induces disaster occurrence has led to growing research on this topic. This section first presents macroeconomic studies which broadly distinguish between geological and climatic disasters in order to contextualize the separation undertaken in this paper. Micro level research has hardly compared the impact of different disaster classes on life outcomes. Hence, the review then considers studies which analyzed the impact of specific disaster types or episodes on various socioeconomic outcomes at the individual, household, and locality level. The focus will be on those which use human capital and labor as their outcome of interest, in order to understand the possible mechanisms that would help explain this paper's main results.

3.2.1. Geological versus climatic disasters

The literature here below compares the effects of geological and climatic disasters on various macroeconomic outcomes. All mentioned studies are at the cross-country level. This paper thus adds a microeconomic perspective to these comparative analyses.

Barkun (1986) was amongst the first to acknowledge the distinction between disasters perceived as 'acts of God' and those that were 'human-caused'. However, this differentiation became more salient with the emergence of large-scale, human-caused disasters, including the Santa Barbara Oil Spill (1969), Love Canal (1978), and Three Mile Island (1979) (Picou and Marshall, 2007).

In particular, Albala-Bertrand (1993) examines the economic and social effects of disasters, distinguishing between 'sudden, natural, geophysical events' such as earthquakes, hurricanes, and volcanic eruptions, and the 'slow-developing' and 'man-made' ones such as droughts and famines.[1] They show that the former generally lead to null or even positive GDP growth in the long term, which they suggest to be due to the fast local, national, and international responses that they tend to engender. The latter predominantly affect poorer societies whose economic situation tends to worsen and remain so for a longer-lasting period following the event. Their results are supported by this paper, which finds significant adverse effects of climatic disasters, especially those that affect living capital (like droughts), and potentially null effects of geological disasters.

Skidmore and Toya (2002) also separate 'climatic' from 'geologic' disasters, explaining that climatic disasters, for example floods and storms, tend to occur more frequently, during specific times of the year, and are thus more predictable. Hence, people can protect themselves more easily by taking cover or evacuating prior to the event. They infer that climatic disasters are a good proxy for risk to physical capital, while geologic disasters are better proxies for life threats. Similarly to this paper, they find relatively weaker effects of geological disasters. However, they also find that at a macroeconomic level, the higher frequencies of climatic disasters are correlated with higher rates of human capital accumulation, as measured by secondary school enrollment and years of schooling, increases in total factor productivity, and economic growth. Though disaster risk, which is highest for

[1] This also relates to the broader literature on the cost of man-made shocks, including conflicts, on GDP growth (Alamir et al., 2018; Collier, 1999; Organski and Kugler, 1977).

this class of events, reduces the expected rate of return to physical capital, this risk also serves to increase the relative return to human capital. Thus, physical capital investment may fall but there is a substitution toward human capital investment. Disasters can also provide the impetus to update the capital stock and adopt new technologies, leading to improvements in total factor productivity.

In a similar study, Raddatz (2009) finds insignificant effects of geological disasters, as well as a significant reduction in GDP per capita when climatic disasters occur. In accordance with Albala-Bertrand (1993) and this paper, Raddatz finds that droughts are particularly cumbersome to low-income countries, making them the costliest climatic disaster.

Loayza et al. (2012) evaluate the effects of droughts, floods, storms, and earthquakes. They find that over a five-year period, growth falls by 0.6% after a drought, and the agriculture and industrial sectors are most affected. However, overall growth rises by 1% following a flood, to the extent that they are moderate. Their suggested reasoning behind this is that while too much water is damaging, floods tends to be localized and concentrated in time, thereby affecting only one of the multiple growing seasons. In these circumstances, they are also indicative of a plentiful annual supply of water nationwide, which should positively affect total factor productivity in agriculture, including through the collection of irrigation water. Thus, as long as they are not too severe, these positive effects can outweigh the negative effects coming from the destruction of public infrastructure and land. This somewhat echoes Skidmore and Toya (2002)'s reasoning.

In line with this paper, Klomp (2016) finds that the frequency effects of climatic disasters slow down economic growth. This effect is aggravated in developing countries as recovery investments often take long to implement due to the financial and capacity constraints present in these countries. However, the amount of damage caused by a single geological event is on average 2.5 times larger than the impact of floods or droughts. As a result, only these types of events are found to have long-lasting effects, which are mostly positive, possibly due to the large-scale replacement investments that trigger technological progress. This Schumpeterian destruction argument thus relates to Skidmore and Toya (2002) and Loayza et al. (2012)'s suggestion, although for the other disaster class.

3.2.2. Micro level effects of natural disasters

Disaster analyses undertaken at the micro level typically focus on specific disaster types or episodes. By comparing climatic and geological disasters, this paper contributes to the microeconomic literature by adding a more generalized perspective.

Pane et al. (2008) and Spencer et al. (2016) both find negative effects of hurricanes on school performance. The former find this result especially significant for students who remained displaced for the duration of the academic year. The latter find it specifically when the disaster strikes during school term. The authors explain this through the fact that hurricanes occurring during the academic year increase the possibility that school days are lost and hence the number of days of classroom instruction is reduced. These results corroborate other findings, including this paper's, which show that natural disasters cause a reduction in school attendance (Jacoby and Skoufias, 1997; Pane et al., 2006; Baez and Santos, 2007; Santos, 2007; Baez et al., 2010). Further, Poertner (2008) finds that hurricane shocks in Guatemala lead to decreases in education attainment. In contrast to this paper, Poertner (2008) also finds a reduction in fertility shortly after the shock, which is however offset by an increase at a later stage. These patterns are explained by parents' needs for insurance.

In a similar exercise to this paper, De Janvry et al. (2006) analyze the impact of various disasters (earthquakes, hurricanes, floods, plagues, and droughts) on school enrollment for 8-17-year-olds in Mexico in the late 1990s. While this paper analyses most of the 2,456 Mexican municipalities, De Janvry et al. (2006) restrict their sample to poor households coming from 506 rural localities. Possibly due to those differences in sample and period of observation, their paper does not find any significant effect from droughts contrarily to this paper, although they do find negative effects from all other disaster types. Regarding the channels, their results are explained by an income effect following the shocks, which corroborates this paper's findings.

Caruso and Miller (2015) provide evidence on the existence of an intergenerational transmission of human capital effects of the 1970 Ancash earthquake in Peru. They find that males who were affected by the earthquake in utero completed, on average, 0.5 years less of schooling while females completed 0.8 years less. Those individuals also do worse in the marriage market and marry younger. Finally, children of mothers affected at birth by the earthquake have 0.4 less years of education while those whose father was affected show no effect. Given the increased fertility rate of disaster-affected 17-18-year-olds found in this paper, these results are especially worrisome as it means the observed adverse educational outcomes may be passed on through generations.

Beyond education, previous literature considered the impact of disasters on labor market outcomes. Kirchberger (2017) finds that individuals who worked in the agricultural sector in Indonesian areas affected by a large earthquake saw a significantly higher growth in earnings. One reason, she notes, could be that employment in the agricultural sector contracted in the aftermath of the earthquake, while it expanded in the construction sector thereby resulting in greater wage growth for agricultural workers. Similarly, this paper finds that following geological disasters (including earthquakes), maternal earnings significantly increase, possibly due to post-shock labor market adjustments. In the same line, Gignoux and Menéndez (2016) find that Indonesian earthquakes lead to short-term economic losses, but individuals recover their earnings and can even experience gains in the long term after 6 to 12 years. The reason for this is that the stocks of productive assets, notably in farms, get reconstituted and public infrastructures are improved, partly through external aid, which allows productivity to recover.

Rodríguez-Oreggia (2013) finds that hurricanes in Mexico have a positive effect on wages and formal jobs, especially for low educated workers. In contrast, Sperling (2019) finds that the 2007 Tabasco flood caused an immediate decline in work income for the affected population. The result is mainly driven by an increase in the number of predominantly male workers who continue to work after the flood but now earn no or less than the minimum wage. This corroborates this paper's finding that fathers' incomes seem to decrease following climatic disasters, although the weekly hours are not significantly altered. Remaining in Mexico, Rodríguez-Oreggia et al. (2013) find a significant increase in poverty and a decline in Human Development Index in disaster-affected municipalities. In line with this paper's findings, floods and droughts (both climatic) show the strongest adverse effects. Lastly, Heidelk (2019) investigates the impact of the 2010 Haiti earthquake on individual monetary returns to education. The latter are found to decrease on average by 37%, especially in equipment-capital intensive industry. Higher educated individuals adjust into low-paying self-employment or agriculture. As in this paper, these adverse outcomes are found to be particularly shock-sensitive for urban residents.

3.3. Background on Mexico

This section provides insights into natural disasters and the education system in Mexico.

3.3.1. Natural disasters in Mexico

Mexico's geographical characteristics and the adverse social conditions suffered by almost half of its population (Wilson and Silva, 2013) make it one of the most vulnerable countries to the impacts of natural disasters (Gobierno de la república, 2015). Approximately 41% of Mexico's territory and 31% of its population are exposed to a range of natural hazards including storms, floods, earthquakes, and volcanic eruptions (Sagastume, 2014).

In just over 100 years, both land and sea surface temperatures have increased across the country (Gobierno de la república, 2015). This has been accompanied by an increased number of extreme hydro-meteorological phenomena. Figure (B.2) shows the frequencies of 'impactful' disasters over time, based on data from DesInventar (2019a). Here, impactful relates to disasters which caused quantifiable damage. The graph highlights the overall increasing trend over time, in line with global trends of major disasters (Guha-

Sapir et al., 2015). The map in figure (B.3) presents the relevant spatial distribution of impactful disasters in Mexico, averaging all data over time. It shows that disasters occur throughout the entire country.

In 2010, Mexico was organized into 32 states and 2,456 municipalities.[2] When a disaster strikes, the national coping mechanism follows the subsidiarity principle, meaning that municipalities have the primary role in responding to the situation through the local civil protection office. This level of government is also in charge of all urban land-use provisions and many public services, particularly water and sanitary services as well as basic infrastructure (Ruiz-Rivera and Melgarejo-Rodríguez, 2017). However, once the municipal emergency management capacities are exceeded, they can request assistance from their state authorities, who can themselves call for federal support under the same conditions (OECD, 2013b). Once the National Civil Protection System is asked to intervene, they coordinate with each level of government, the army, and the navy to organize and execute the post-disaster recovery and reconstruction phase (UNDP, 2014).

Every year, federal and state governments in Mexico spend close to US$ 1.5 billion on reconstruction of public assets and low-income housing after natural disasters. In response to the continued need for *ex post* budget reallocations, in 1996 the Mexican government established FONDEN. This financial vehicle allows the federal government to allocate budget *ex ante* for post-disaster response and the reconstruction of federal and state infrastructure, as well as low-income housing and eligible natural environment assets. FONDEN finances 100% of the reconstruction of federal assets and 50% of local assets the first time it is required. The percentage then decreases for local assets if insurance is not purchased (Ishizawa et al., 2013).

The high variability of disaster losses has meant funding needs have exceeded FONDEN's resources in 5 out the last 14 years (Ishizawa et al., 2013). This is an additional motivation to understand which disaster type is most costly and long-lasting, and which share of the population is most vulnerable to its adverse effects.

3.3.2. Education in Mexico

Following the 1992 *National Agreement for the Modernization of Basic Education*, the governance of schools in Mexico is largely decentralized. 31 states have the autonomy over their education systems and the operation of basic education services for pre-primary, primary, secondary, and initial teacher education (OECD, 2018). At federal level, the Secretariat for Public Education (SEP) is in charge of establishing norms and regulations, as well as operating Mexico City's basic education system. The municipalities' role in education is limited, involving the building and maintenance of school infrastructure,

[2]In 2000, Mexico had 2,480 municipalities.

equipping school spaces, and participation in specific education programs. Their role might however be more influential in rural and isolated areas (Santiago et al., 2012).

Funding for schools comes from various sources. Some schools receive funds from the federal government through the SEP and other state secretariats or federal agencies. Autonomous schools usually receive a subsidy from federal and state governments which they administer themselves. Privately supported schools are self-financed and administered (OECD, 2013a). In 2009, considering all levels of education, the funding of education was shared between the federal government (62.1%), state governments (15.6%), municipalities (0.2%), and the private sector (22.1%) (Santiago et al., 2012). However, the public funding of schools is not equitably spread across schools as a significant part of it is provided through specific programs with their own budgets (OECD, 2013a). As a result, public schools often rely on parental donations to cover their operational needs. Concentrating on primary and secondary education, Mexican households cover around 17.3% of the total expenditure on educational institutions. Although public schools have no enrollment fee, parents are required to pay for textbooks, uniforms, and transport starting from the lower-secondary school level (OECD, 2014). As an example, in San Miguel Tlacotepec, Oaxaca, expenses at the high school level are roughly US$ 21 per month per family, or 8% of monthly per capita income (Sawyer, 2010). Furthermore, the standardized tests that students take at the end of secondary school is organized by a private company, Centro Nacional de Evaluación para la Educación Superior (CENEVAL), and students have to pay to take it (Secretariat for Public Education, 2010b).[3] Given binding household budget constraints, these various monetary costs may prevent some adolescents from graduating or from attending school altogether.

Up until 2012, education was only compulsory until the lower secondary level, normally reached by age 15 (OECD, 2013a). After that year, it became compulsory for the upper secondary level too. Upper secondary graduation rates increased at an annual average of 3.6% between 2000 and 2011 but remain well below the OECD average. In 2014, Mexico was the sole OECD country where 15-19-year-olds were expected to spend more time in employment (6.4 years) than in education (5.4 years) (OECD, 2014). Furthermore, only 56% of 15-19-year-olds were enrolled in upper secondary education, and 24.7% of 15-19-year-olds were neither in employment nor in education and training (NEET) in 2011 (OECD, 2013a). OECD (2014) further points out that one in ten Mexican 15-29-year-old men were NEET in 2014. For women this number climbed to three in ten. Studies based on IMJUVE (2011) indicate that most young NEET women are housewives, suggesting that the gender gap may be largely related to cultural matters, such as early marriages and pregnancies (OECD, 2014). Finally, large performance and completion gaps persist for indigenous and low-economic status populations (OECD, 2013a).

[3]Prices are available at CENEVAL (2019).

3.4. Quantitative framework

This section introduces the data, the applied difference in differences methodology, and the pre-existing trends.

3.4.1. Data

The paper employs three types of data: (i) individual level census data, used to obtain municipal averages, (ii) municipality level data on disaster exposure, and (iii) additional municipal level information concerning aspects such as the economic and political environment as well as the prevalence of other adverse shocks.

3.4.1.1. Municipal averages of individual data

The individual level data which are used to create municipal averages are based on 10%, 10.6%, and 10% census data from the years 1990, 2000, and 2010 respectively, yielding a total of 8,118,242 observations (including 404,353 17-18-year-olds) in 1990, 10,099,182 (432,793) in 2000, and 11,938,402 (517,689) in 2010. The data are obtained via the international version of the Integrated Public Use Microdata Series (IPUMS) (Minnesota Population Center, 2019c).[4] Individual level weights are applied in order to derive municipality year level data, for example the (weighted) share of 17-18-year-olds who completed upper secondary education in each municipality, in each wave. Other data include age, gender, additional educational outcomes such as enrollment, labor market outcomes such as employment status or being neither in employment nor in education or training, migration background, and demographic data such as the overall share of the population working in agriculture. Parental income from the source data are adjusted for inflation as well as the 1993 introduction of the 'nuovo peso' (Minnesota Population Center, 2019b).

3.4.1.2. Natural disasters

Data concerning natural disasters are accessed from DesInventar (2019a), an international disaster database. For Mexico, it holds information throughout 2013. It provides daily data on occurrence and type of disasters as well as their effects, such as the number of destroyed houses, c.f. DesInventar (2019b) for details. The database is fed by reports from official emergency management agencies, official sectorial institutions such as the

[4]The 1995 wave covers only relatively few municipalities. Both 1995 and 2005 display suspicious trends in outcome variables of interest, for example five-year-on-year changes in educational attainment of 17-18-year-olds exceeding 80%, then dropping over 20%, and then again increasing. This may be due to the different sampling processes of the waves, c.f. Minnesota Population Center (2019d). The 2015 wave is excluded due to missing data.

ministry of agriculture, public works, communications, and transport, archives of relief aid organizations, academic and scientific files maintained by research institutions, and media releases, specifically written media such as newspapers. Figure (B.1) provides an example for an entry ('data card') into the DesInventar database. The disaster data are expanded using the available information from the EMDAT (Guha-Sapir et al., 2015).

In this paper, an entry is coded as 'disaster' if it has impact. The analysis thus conditions observations on those for which quantifiable information on damages, losses, or casualties has been reported. In particular, an observation counts as an impactful disaster if the relevant data card reports individuals who died, got wounded/sick or were directly affected ('victims'), for example if their property or crops got destroyed. The condition further extends to events which had damaged/destroyed houses, education or health centers, transport networks, cultivated or pastoral land or woods, or livestock, as well as to observations which report the financial loss caused by the disaster. Hence, violent storms that took place in the desert with no human, physical, or other impact, for instance, are excluded. These observations are deemed to be more relevant as well as more reliable and less vulnerable to over-reporting, compared with events for which the source data only report that a certain economic sector or a type of physical capital was affected. The latter may happen even if no actual disaster materialized. For example, disaster warnings can imply interrupted roads and businesses. Lastly, if a municipality yields no observation in a given year/period, it is coded as zero disasters in that time.

Following previous literature (Skidmore and Toya, 2002), disasters are classified as follows: (i) exogenous/geological events ("GEO"), (ii) climate-change-related events which cannot be expected to destroy infrastructure but may affect human capital, livestock, or harvests ("LIV"), and (iii) climate-change-related events which can be expected to damage infrastructure ("PHY"). Table (B.1) provides an overview of the relevant disasters in each class, figure (B.2) presents the frequencies of the impactful disasters, over time.

The reasoning for the sub-classification is twofold. On the one hand, it stems from the underlying inter-temporal dynamics of the occurrences of these events. For example, earthquakes and volcanic eruptions have always been prevalent whereas climate-related events like floods have been gradually increasing in intensity and frequency over the past century (National Academies of Sciences, Engineering, and Medicine, 2016). On the other hand, the differentiation among the two climatic event classes is due to the possibly differing channels of how these disasters affect educational outcomes. For example, a drought will not hamper access to education via destroyed schools or infrastructure, whereas a flood may. The disaster classification also considers the cause of an event, for example an explosion which is caused by a movement of tectonic plates is recorded as 'geotechnical fault' and not as an explosion. Likewise, events which are reported to have been caused by an accident, behavior, design, deterioration, human error, location, negligence, short circuit, or provocation are excluded. This thus omits man-made landslides, floods caused by deteriorated/collapsed dams, and other non-natural disasters. Climatic events are also corrected for human-induced incidences and appear-to-be climatic events which are in fact caused by exogenous events, for example earthquake-induced floods. Incidences where the disaster is due to the management/response to a previous event are omitted as well, for example a famine caused by a drought. The reasoning is that the famine may be preventable after the drought has occurred and after a certain time has passed, i.e. its occurrence is likely to be due to other endogenous factors.

While comprehensive, the DesInventar data pose challenges. First, the disaster location is at municipal level, which thus yields the treatment level of the empirical analysis. However, some events are only recorded at the state level. For example, a data card may indicate that a hurricane affected 15 municipalities and damaged 70,000 houses, making it impossible to allocate the information to the municipality level. These observations thus need to be dropped.

Second, DesInventar does not allow to infer with certainty if separate entries are distinct events. Hence, a disaster may be recorded twice for the same day, or multiple times on subsequent days, without the possibility to disentangle if the entry refers to one or many events (see also Groeve et al. (2013) for a discussion). To account for possibly inflated event entries, the data is reduced to one observation per disaster type, per day. For example, only one storm per day. While it is possible that multiple events occur daily, this is rather unlikely. For each day, the highest entry is thus kept. For example, two 'landslide' entries on the same day in the same municipality, with the first indicating 5 destroyed houses and 10 wounded people and the second showing 100 destroyed houses and 1 wounded person will be recorded a one landslide event with 100 destroyed houses and 10 wounded people.[5] Related, a disaster may last longer than a day. Indeed, figure (B.1) shows that data cards can indicate the duration of an event. However, only about 2% of the raw entries yield such information and the duration information remains questionable, for example suggesting events lasting over years.[6] As such, the duration information is ignored in the main analysis. A robustness check revisits this aspect, showing that the results are not sensitive to this choice.

Third, impact intensity indicators such as the number of destroyed houses do not distinguish between zeros and missings. Zeros can hence be treated as true zeros or, alternatively, zero-entries can be treated as missing values. Depending on the true value of the indicator, the induced bias by the choice of how to treat the zeros may be positive, negative or null, depending on the observed change in the variable of interest. This

[5] The choice of one event per day is arbitrary. However, avoiding intra-day duplicates appears to be the most obvious choice.

[6] The share increases to about 6% after adding the EMDAT data.

paper treats zero-entries as true zeros. The reason is twofold. First, to keep using the full dataset and avoid false deletion of data. Second, the paper's principal treatment indicator is driven by disasters which have a quantifiable impact. It is assumed that the impact of major events would be reported and that false zeros (zero-entries for which the true value is positive) are rather driven by minor events. Hence, zeros may be attributed to otherwise small or negligible treatments, which seems to be the least bad option.

3.4.1.3. Additional data and summary statistics

Above data are merged with background information on population size in different years, geographical size of municipalities, and as a proxy of wealth and resilience to disasters, annual budget data, all accessed from Instituto Nacional de Estadística, Geografía e Informática (1990), Instituto Nacional de Estadística, Geografía e Informática (2000), Instituto Nacional de Estadística, Geografía e Informática (2010a), Instituto Nacional de Estadística, Geografía e Informática (2010b), and Instituto Nacional de Estadística, Geografía e Informática (2019). For each IPUMS wave, the revenues of the last five years are aggregated to obtain the *log* of per capita values used in the estimations.[7] Alternatively, expenditure data could be used but revenues are the preferred as the political budget cycle literature has shown that public expenditures tend to covary with election years, including in Mexico (Gamez and Ibarra-yunez, 2007; Pérez Yarahuán et al., 2007; Vazque and Martínez, 2016). Inflation data to adjust income and budget data to 2015 prices are obtained from World Bank (2019).

For additional analyses, the above data are complemented with figures on violent conflicts in Mexico (Uppsala Conflict Data Program, 2019; Sundberg and Melander, 2013; Högbladh, 2019) as well as municipality level election outcomes, accessed from Centro de Investigación para el Desarollo A.C. (2011b).[8]

Table (3.1) presents the condensed summary statistics of the data.[9] Degree attainment increases over time. In areas without disasters attainment rates rose faster than in mu-

[7] 'Allocations' and 'contributions' from the federal and state level, as well as the 'not available' revenues are excluded from the aggregates to avoid that post-disaster interventions or other budget lines affect the aggregates since the aim is to capture municipality level wealth and resilience net of external support. Per capita values are estimated based on projected population growth for years in which no population data are available.

[8] The data are not straightforward accessible at Centro de Investigación para el Desarollo A.C. (2011b) and technically the election data for each municipality come from a separate subordinate page. To avoid adding 32 additional references, the superordinate source is being referred to. For an example source for Aguascalientes, see Centro de Investigación para el Desarollo A.C. (2011a).

[9] The final dataset yields 2,266 municipalities and 30 states per year. Some observations were dropped due to missing data, others are lost due to spatial harmonization of administrative boundaries imposed by IPUMS which retains at least 20,000 individuals per municipality and makes administrative boundaries inter-temporally consistent (Minnesota Population Center, 2019a). Hence this did not allow to consistently allocate other geographical information such as disaster information.

nicipalities exposed to disasters. The share of 17-18-year-olds who completed upper secondary education is higher in the treated municipalities which experienced disasters. This may relate to the fact that the share of 17-18-year-olds who live in a city is also higher in the disaster-affected areas. The age and gender patterns are stable across regions and time. Migration is higher in disaster regions and decreased over time. About half of the municipalities experienced a GEO disaster. The share of municipalities which experienced a LIV disaster is also at about 50%. However, a greater share suffered some PHY disaster.[10] Table (B.2) in the appendix provides the full summary statistics.

Table 3.1.: Summary statistics

A: Municipality level means/shares for 17-18-year-olds

	Year 2000		Year 2010	
	No disaster mean (SD)	Disaster mean (SD)	No disaster mean (SD)	Disaster mean (SD)
% completed upper secondary attainment[a]	7.09	13.65	10.52	16.16
	(5.52)	(6.65)	(5.86)	(6.88)
% living in urban area[a]	46.02	74.02	47.81	74.51
	(33.15)	(28.42)	(33.31)	(27.32)
Age[a]	17.50	17.50	17.50	17.50
	(0.06)	(0.04)	(0.04)	(0.03)
Gender (% female)[a]	48.70	49.09	49.76	50.48
	(6.24)	(3.88)	(4.45)	(3.42)
% state level migrants[a]	2.15	4.89	2.08	3.43
	(2.50)	(4.59)	(2.09)	(2.62)
% municipality level migrants[a]	2.35	3.44	2.16	2.82
	(3.18)	(3.25)	(3.07)	(2.92)

B: Municipality level disaster realizations in period 2005-2010

	Year 2000		Year 2010	
	No disaster mean (SD)	Disaster mean (SD)	No disaster mean (SD)	Disaster mean (SD)
% hit by any impactful disaster[c]	0.00	100.00	0.00	100.00
	(0.00)	(0.00)	(0.00)	(0.00)
% hit by GEO disaster[c]	0.00	44.86	0.00	44.86
	(0.00)	(49.76)	(0.00)	(49.76)
% hit by LIV disaster[c]	0.00	56.11	0.00	56.11
	(0.00)	(49.65)	(0.00)	(49.65)
% hit by PHY disaster[c]	0.00	86.85	0.00	86.85
	(0.00)	(33.81)	(0.00)	(33.81)
Observations	1,082	1,182	1,082	1,182

Each cell shows the mean of the relevant variable, in a given period, in an exposed on non-exposed municipality. Standard deviations (SD) in parentheses. Population weights for the number of 17 to 18-year-old individuals applied. Source(s) of variables: a: Minnesota Population Center (2019c), c: DesInventar (2019a).

[10]Note that these categories are not mutually exclusive. While each individual event is strictly assigned to one class, a municipality can experience different or even all disasters in the same period.

3.4.2. Difference in differences methodology

This paper assesses the impacts of adverse shocks in the form of GEO, LIV, or PHY disasters on the municipal share of 17 and 18-year-olds who completed upper secondary education in Mexico. The variations in space, time, and intensity of the shocks are exploited to construct exogenous disaster exposure indicators for each municipality. Following previous disaster literature, the paper applies a difference in differences framework to investigate the effects.[11] Equation (3.1) presents the difference in differences approach pursued in this paper:

$$y_{amst} = \beta_0 + \beta_1 d_m + \beta_2 t_t + \tau(d_m \times t_t) + Z'_{amst}\gamma + \nu_m + \zeta_s + \zeta_s \times t_t + \varepsilon_{amst} \tag{3.1}$$

with y_{amst} being the outcome y for age group a in municipality m in state s in time t. d_m is the disaster indicator referring to the period 2005-2010. t_t is a dummy indicating the year 2010 and accounting for the national time trend. Z_{amst} is a vector of time-varying covariates. ν_m and ζ_s are time-invariant region specific fixed effects which account for aspects such as persistent vulnerability factors, $\zeta_s \times t_t$ represents state specific trends controlling for structural inter-temporal inter-state differences, for example due to variations in schooling law enforcement. ε_{amst} are the idiosyncratic errors. τ is the coefficient of interest, yielding the difference in differences estimator and average treatment effect on the treated.

Elaborating on the disaster indicator d_m, similar to Jacoby and Skoufias (1997), in the main specification this indicator is a dummy equal to 1 if the municipality level realization of impactful disasters from the past five years exceeds the state level average from the preceding five years, plus two standard deviations (SD). For example, take municipalities A, B, and C located in state L. Municipality A records 1 disaster D in the period $[2000, 2005]$, denoted as $D_{\mathrm{A},2000-2005} = 1$, and similarly $D_{\mathrm{B},2000-2005} = 5$, $D_{\mathrm{C},2000-2005} = 0$, then the state level mean for this period is $\mu_{\mathrm{L},2000-2005} = 2$ with $SD_{\mathrm{L},2000-2005} = \sqrt{7} \approx 2.65$. Following, if $D_{\mathrm{A},2005-2010} = 3$, $D_{\mathrm{B},2005-2010} = 12$, and $D_{\mathrm{C},2005-2010} = 0$, then the realization for municipality B for the period 2005-2010 is recorded as 1 whereas for A it is 0 since $D_{\mathrm{B},2005-2010} > \mu_{\mathrm{L},2000-2005} + 2 \cdot SD_{\mathrm{L},2000-2005}$ while $D_{\mathrm{A},2005-2010} < \mu_{\mathrm{L},2000-2005} + 2 \cdot SD_{\mathrm{L},2000-2005}$. C's realization is necessarily 0 too. Applying these values to equation (3.1) this implies $d_\mathrm{B} = 1$ and $d_\mathrm{A} = d_\mathrm{C} = 0$.[12] Figure (B.4) shows the spatial distribution of the created disaster indicator for the period 2005 – 2010. Several robustness checks will explore alternative setups of the disaster indicator in subsection (3.6.5).

[11] For example, Baez et al. (2017), Caruso and Miller (2015), Caruso (2017), Jacoby and Skoufias (1997), Jensen (2000), Pane et al. (2008), Sacerdote (2012), and Spencer et al. (2016) use a difference in differences approach to investigate the impact of natural disasters on various outcomes.

[12] Slightly abusing semantics, the paper will proceed saying that B experienced a disaster/disasters.

Z_{amst} contains, for each municipality and for 17-18-year-olds: the share living in urban areas, share of females, share of inter-state as well as inter-municipality migration in the last five years, and average age. Higher urbanization rates may relate to better access to schooling and greater salience of returns to education. The gender share accounts for possibly persistent gender gaps and gender-specific effects. The average age controls for the fact that older students have had more time to complete the degree. The two migration variables capture possible difficulties for students and families to settle in the new environment and school which may affect attainment rates. It may also account for selection into perceived-to-be safer places.[13]

In addition, for each municipality Z_{amst} yields the log of aggregate per capita revenues of the last five years, total size of the population in a given wave, a dummy indicating if any deaths due to violent conflict were recorded in the last five years in the municipality,[14] and the municipality population shares of people working in agriculture, construction, or armed forces. Revenues account for municipal wealth and possibly how fast and efficiently a municipality can react to shocks to mitigate any negative effects (Kahn, 2005; Buhr et al., 2018; Kling et al., 2018). Population size captures how many individuals and assets may be affected by a disaster as well as the fact that larger municipalities are more likely to be hit by a disaster, *ceteris paribus*, and that larger municipalities may have more means to absorb a shock (Auffret, 2003; Cavallo et al., 2011). Conflict-induced deaths controls for exposure to the 'War on Drugs', another negative covariate shock in the given time period (Brown and Velásquez, 2017; Caudillo and Torche, 2014; Michaelsen and Salardi, 2018). The share of people working in agriculture within the municipal population proxies local economic settings, and in particular the importance (or non-importance) of physical capital to generate income. The share of people working in construction hints to how quickly a population can recover with ready-to-deploy workforce, for example to reconstruct schools. A higher prevalence of armed forces may relate to responsiveness as these troops can be deployed in a disaster relief context (Pelcastre, 2017).

The estimations apply population weights using the number of individuals in the specific age group a in the year 2000, $N_{a,2000}$. This aims at recovering the underlying population contributions from each municipality as well as to mitigate heteroskedasticity. Following Solon et al. (2013) and Wooldridge (2013), to test for the prevalence of heteroskedasticity in the residuals across the distribution of the underlying municipality-year population sizes, a (modified) Breusch-Pagan is applied. An altered version of equation (3.1) is employed, omitting d_m. Table (B.3) reports the results, each column referring to the cross section under consideration. The row for χ^2 shows the relevant test statistic and p the

[13] However, previous literature suggests this effect not to be prevalent, c.f. Rodríguez-Oreggia (2013), Kirchberger (2017), and Heidelk (2019) for evidence on Mexico, Indonesia and Haiti, respectively.
[14] Based on the 'best estimate', c.f. Högbladh (2019).

related statistical significance (p-value), suggesting heteroskedasticity in the year-specific cross sections. To mitigate this, first, Eicker–Huber–White standard errors are applied. Second, following Solon et al. (2013), the relevant population sizes $N_{a,2000}$ are used to weigh the estimates. This approach is followed since in all cases, σ_c is non-negligibly large and $N_{a,2000}$ showcases large variation.

An additional concern in the applied literature is the need to adjust estimated standard errors for possible serial correlation, see for example the discussion in Bertrand et al. (2004). However, this is not a concern in the given application given that $T = 2$ in the main specification and maximum 3 in all others, c.f. Bertrand et al. (2004, p. 261 and their table III, as well as p. 267 and their table VI).

3.4.3. Pre-existing trends

Identification in a difference in differences framework rests on the assumption that the treatment and control groups would have had comparable developments over time, in the absence of treatment. In the case at hand, this implies that completion rates of upper secondary education should have moved in parallel in absence of disasters and hence also before the treatment took place. It would be concerning if the attainment rates decreased in treated areas relative to untreated areas in the absence of treatment. This would suggest that the observed negative impact is related to a pre-existing trend and not the result of exposure to treatment.

Figure (3.1) presents the relevant rates, over time, starting in 1990. The graph shows that before the treatment, the trends were not parallel. The control group displayed, on average, lower attainment rates than the treatment group. From 1990 to 2000 this difference increased, i.e. the treatment group displayed greater educational expansion relative to the control group. In the given case, this is not a concern given the negative observed treatment effects. Indeed, the non-parallel pre-existing trends work in the opposite direction from the treatment effect which is evidence against the hypothesis that the observed negative response to the treatment are part of a pre-existing negative trend.

For simplicity, in figure (3.1), all disasters are taken into account.[15] Similar graphs can be obtained for the separate disaster classes GEO, LIV, and PHY. The result is unchanged. Following the main specification, figure (3.1) applies age-specific population weights as in the main specification. The results are unchanged when omitting the population weights.[16] To empirically test for these pre-existing trends, robustness checks will employ placebo tests with alternative timing in subsection (3.6.1). The results of this section here will be confirmed.

[15]This coincides with the most general ‘Any’ specification from table (3.2).
[16]Figures for the different disaster classes and under omission of weights available upon request.

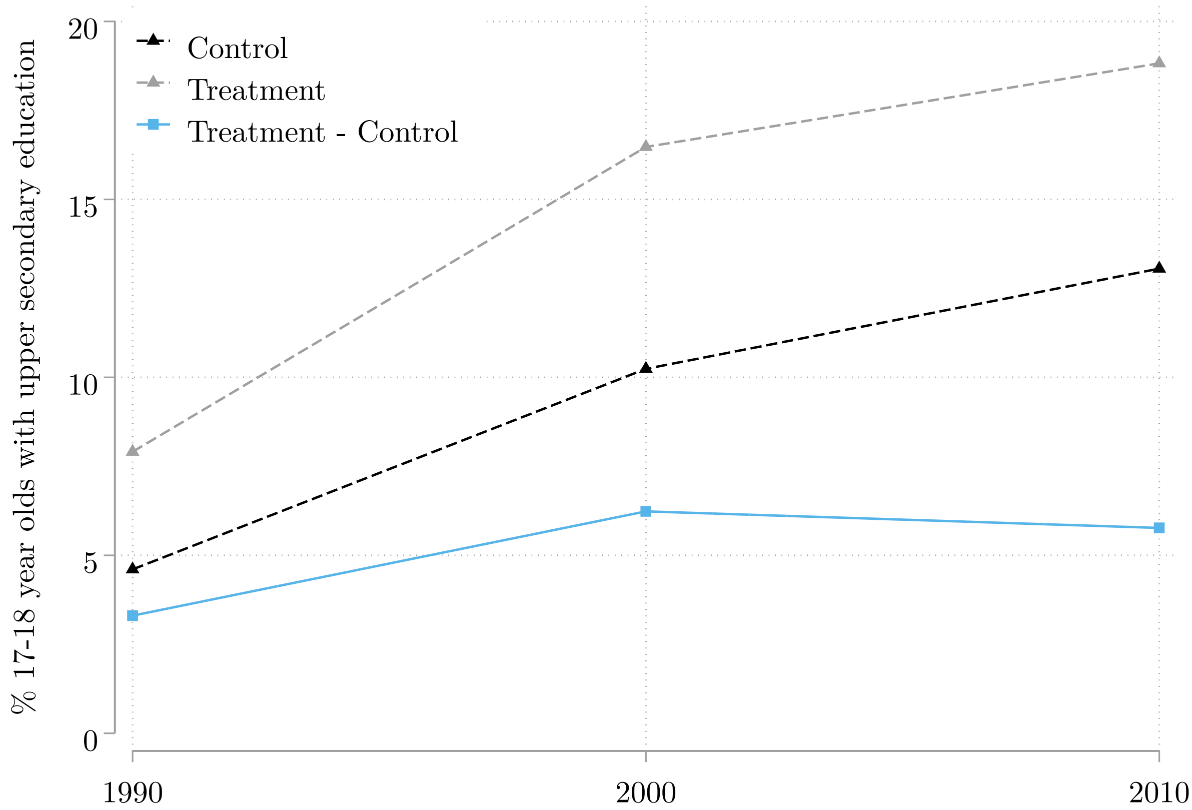

Figure 3.1.: Pre-existing trends.

3.5. Results

This section presents the comparative effects of climatic and geological disasters on upper secondary education attainment for 17-18-year-olds. It then inspects the differential effect of the intensive margin for each disaster class, before exploring how the results change when focusing on municipalities with higher urbanization rate.

3.5.1. Main results: Comparing types of disasters

Table (3.2) presents the main results, showing the impact of prevalence of different types of natural disasters on upper secondary education completion rates for 17 and 18-year-old students in Mexican municipalities. Column (1) follows equation (3.1), applying the full matrix of control variables Z_{amst} and aggregating all three disaster classes. The estimated $\hat{\tau}_{\text{any}} = -1.363$ implies that the occurrence of a disaster realization exceeding the past state mean by the set threshold of 2 SD yields a negative impact on degree attainment by -1.363 percentage points.[17] The positive estimates for the national trend are expected and indicate an inter-temporal expansion of educational attainment, all else

[17] Repeating the estimation leaving Z_{amst} empty yields $\hat{\tau}_{\text{any}} = -0.959$ with p-value = 0.0364.

equal. Table (B.4) in the appendix presents the complete results. The robustness checks will assess these findings.[18]

Table 3.2.: Main results: upper secondary education, age [17,18]

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	-1.363^{**}			
	(0.535)			
Year 2010 × $\text{GEO}_{0/1}$		-0.901^{*}		
		(0.472)		
Year 2010 × $\text{LIV}_{0/1}$			-1.613^{***}	
			(0.521)	
Year 2010 × $\text{PHY}_{0/1}$				-1.187^{**}
				(0.521)
Year 2010	3.945^{***}	3.882^{***}	4.027^{***}	3.897^{***}
	(0.941)	(0.949)	(0.922)	(0.962)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	4,489	4,489	4,489	4,489
R^2	0.43	0.43	0.44	0.43

The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Columns (2) through (4) refine the analysis from column (2), disentangling the different disaster classes applying the full matrix Z_{amst}. The results show that for each class, the prevalence of disasters has a negative impact on degree attainment. In particular, the impact of LIV disasters is higher (more negative) than for other disasters. This result is in line with Albala-Bertrand (1993) and Raddatz (2009), who show that climatic disasters, and droughts especially, are the costliest type of disasters in low-income countries. Interpreting the result for LIV disasters, $\hat{\tau}_{\text{LIV}} = -1.613$ implies that exposure depressed educational expansion by over 40% relative to the unaffected municipalities, an economically significant difference. In absolute terms, for exposed municipalities, educational attainment rates are expected to be about 10.57% to 11.65% lower than without disaster exposure, all else equal.[19] To appreciate the range of the derived estimates, for GEO

[18] The relationship from equation (3.1) is also assessed using individual level data by employing the census data as repeated cross sections. They confirm the main results from table (3.2). Individual level results available upon request.

[19] The fixed effects specification in equation (3.1) imposes restrictions on the interpretability of the comparison of the growth curves for exposed and non-exposed municipalities. Deriving the predicted realizations by imposing equal predicted attainment rates for both groups in 2000, and also repeating

shocks which displayed the lowest estimate at $\hat{\tau}_{\text{GEO}} = -.901$, the corresponding value for the growth differential is −23.20% and the absolute terms for the treated municipalities is between −6.00% and −8.91%, which is still economically significant.

3.5.2. The impact of the intensive margin

To further the comparison of the impact of different disasters, in this subsection the original analysis of the extensive margin is extended to additionally consider the intensive margin employing equation (3.2) which builds up on equation (3.1):

$$\begin{aligned} y_{amst} &= \beta_0 + \beta_1^e d_m^e + \beta_1^i d_m^i + \beta_2 t_t + \tau^e(d_m^e \times t_t) + \tau^i(d_m^i \times t_t) \\ &\quad + Z'_{amst}\gamma + \nu_m + \zeta_s + \zeta_s \times t_t + \varepsilon_{amst} \end{aligned} \tag{3.2}$$

with d_m^e equaling d_m from equation (3.1), d_m^i being a continuous variable for the relevant number of disasters in municipality m, and the remaining variables being defined as in equation (3.1). Focusing on the intensive margin, the coefficient of interest is τ^i. τ^e yields the extensive margin. This allows to distinguish the effect of disaster occurrence and increased quantity.[20]

Table (B.5) presents the results. The pattern observed in the main results in table (3.2) is partly recovered, with LIV disasters having a larger impact than GEO and PHY disasters, whose coefficients display about the same magnitude.[21] Concerning the intensive margin, for LIV events $\hat{\tau}^i_{\text{LIV}}$ is positive and statistically significantly different from zero. The net effect exceeds zero at about 16 disasters.[22] However, only one municipality's intensive margin records a value of 17 or higher: Juárez in Chihuahua, with 40 realizations.[23] The second highest value is 15. Assessing the distribution suggests this realization to be an outlier. This is supported by a graphical analysis which shows that the realizations for Juárez in Chihuahua are far outside the range of the remaining series. Numerically, the observation for LIV disasters is 31.9 SD above the mean of the series for the year 2010.[24] The above analysis is thus repeated for LIV disasters after excluding this outlier, and the positive intensive margin effect is found to be no longer prevalent in that setting.[25] Alternatively the intensive margin can be assessed by omitting d_m^e and conditioning d_m^i

the estimation with a random effects model which allows to assess the absolute margins yields the reported range. The values should however be taken with caution. The random effects specification is expected to be inconsistent, based on a Durbin–Wu–Hausman test. Results available upon request.

[20] Revisiting the introductory example in section (3.4.2), d_m^e equals d_m and d_m^i equals D_m.

[21] Notably, for PHY $\hat{\tau}^e_{\text{PHY}}$ turns insignificant, however, with a p-value of .1323 which is taken as weak evidence that the impact of the extensive margin may still be negative.

[22] $-\frac{\hat{\tau}^e_{\text{LIV}}}{\hat{\tau}^i_{\text{LIV}}} \approx \frac{2.13231}{0.13260} \approx 16.1$, i.e. if $d^i_{\text{LIV}} \leq 16$, then $1 \cdot \hat{\tau}^e_{\text{LIV}} + 16 \cdot \hat{\tau}^i_{\text{LIV}} < 0$, if $d^i_{\text{LIV}} \geq 17$, then $1 \cdot \hat{\tau}^e_{\text{LIV}} + 17 \cdot \hat{\tau}^i_{\text{LIV}} > 0$.

[23] The data for Juárez in Chihuahua record 27 cold waves, 10 frosts, 2 heat waves, and 1 drought.

[24] The corresponding value when conditioning the variable to positive observations is 15.9 SD.

[25] Results and graphical analysis for Juárez available upon request.

to be positive such that the estimates are no longer affected by any extensive margin. The same pattern is observed.[26]

This sub-analysis yields two results. First, the original conclusion concerning the relative impact of the different disaster classes is vindicated, in particular concerning LIV *versus* GEO and PHY. Second, the absence of significant impacts of the intensive margin suggests that the negative effects are driven by the disaster realizations which exceed expectations and are hence surprising shocks. Beyond that, the quantity effects do not appear to drive the educational outcomes or relevant channels. Section (3.6.2) takes this analysis further by exploring whether the duration of disasters drives the observed effects.

3.5.3. Heterogeneity over urbanization

To further substantiate the comparison of the disaster classes this subsection takes the local context into account, focusing particularly on heterogeneity across urban areas. The motivation for this is threefold. First, Mexico is an urbanized country with further increasing rates of urbanization over time, demanding a sound understanding of the local impacts. Second, urban areas tend to be different compared to peri-urban or rural localities. On the one hand, urban areas may have access to better information resources via communication technologies and harbor better established post-disaster intervention logistics, which is vital during disasters (Guha-Sapir and Lechat, 1986). On the other hand, cities are also agglomerations of physical capital used in production, i.e. items such as machinery or computers which go beyond structures capital such as the industrial building which accommodates the machines. Destruction of this type of capital may yield additional indirect effects on educational outcomes.

Table (B.6) presents the results when limiting the sample to municipalities with an urbanization rate of at least 80% and 95%, respectively. The original pattern of the results is recovered. LIV disasters have a relatively stronger effect than PHY and PHY yields a more negative impact than GEO. Indeed, the latter does not appear to have any impact in urban settings.[27] This highlights two simultaneous trends. The adverse climatic disaster effect on educational outcomes is most strongly pronounced in highly urbanized settings. This is highly concerning given the rising rates of urbanization and the increasingly intense weather events occurring in Mexico and globally. In turn, the impact of geological

[26] Results available upon request.

[27] An alternative estimation approach is to construct a synthetic municipality level urban/rural panel, i.e. expanding the original spatial panel component by one dimension. This yields comparing treated urban (rural) areas with untreated urban (rural) areas irrespective of the total share of urbanization of the municipality. Above results are confirmed, in particular that the effect of climatic shocks is driven by urban areas and the impact of GEO shocks by rural areas. This underscores that the observed patterns are not driven by sample composition effects when focusing on municipalities exceeding a certain urbanization threshold. Results available upon request.

disasters seems to be stronger in less urbanized areas, possibly due to the absence of fast post-disaster intervention means.

Deeper explanations for these results can be found in the climate change literature, which notably points out that natural disasters have become more acute over the past decades as people have crowded into cities (Kreimer, 2001). The resulting increase in hydrocarbon emissions in urban areas, combined with the heat-sink effects from asphalt and black roofs notably (Smithson, 2002), gradually led to what meteorologists call the 'heat island effect' (Oke, 1997). This increase in temperature tends to aggravate the intensity and frequency of floods, cyclones, droughts, and the gradual rise in sea level (Kreimer, 2001).

Furthermore, the physical process of building cities can exacerbate environmental risks in various additional ways. For example, paving over watershed reduces infiltration, speeds runoff, and increases flood volumes. Constructing coastal defenses reduces supplies of beach sand and can facilitate erosion during storms (Mitchell, 1999). On top of that, the rapid pace of urbanization has translated into a growing influx of people settling in high-risk peripheral areas including coastal zones, flood-prone planes and geologically unstable slopes (Bigio, 2003). Additionally, urban planning and management tend to be particularly poor in these areas of marginal habitation, thereby enhancing their vulnerability to disaster events (Control Risks, 2011).

For those reasons, climatic disasters may have a more intense impact in urban areas whereas geological events which are typically more destructive per disaster (Klomp, 2016) can be counteracted more rapidly in urbanized settings which usually have better developed warning systems, logistics and management support systems, as well as much higher political and media support (Porfiriev, 2009). This could thus help explain why the results found for geological events are most strongly pronounced in less urbanized areas.

3.6. Robustness Checks

In the following section, the validity of the main results are tested by (i) performing placebo tests by re-doing the analysis for a different timing of treatment and older cohorts, (ii) using additional information on the duration of the disasters, where available, in order to decipher whether this drives the main effects, (iii) omitting population weights, (iv) adding clustered standard errors, (v) using various alternative specifications for the disaster indicator, and (iv) assessing multiple shocks. The obtained results are largely robust to these alternations.

3.6.1. Placebo tests

3.6.1.1. Timing of treatment

Expanding on the discussion concerning pre-existing trends in section 3.4.3, this section tests a placebo timing of the treatment. First, equation (3.1) is employed but comparing the outcomes in 1990 and 2000, instead of 2000 and 2010 as in the main specification. Second, realizations of 1990 are compared with 2000 and 2010. A negative and significant estimate of the treatment effect for the year 2000 would indicate the presence of pre-existing trends, questioning the validity of the difference in differences approach.

Tables (B.7) and (B.8) show the results for 1990 *versus* 2000 and for 1990 *versus* 2000 and 2010, respectively. The results show no negative effects before 2010 in either table, for any of the specifications. The positive time trends persist. This is evidence against the prevalence of pre-existing trends and lends support to the quantitative methodology.

3.6.1.2. Older cohorts

Following the Mexican school system, upper secondary education shall be completed by ages 17-18 (depending on month of birth). As such, a certain share of the sample may not have completed their degree yet when interviewed, while being on regular track to obtain it. However, the 2010 field work took place from May 31st to June 25th, and in February/March for the other two waves (Minnesota Population Center, 2019d). Although the 2009-2010 academic year ended on July 9th, the standardized exams took place between April 19th to 23rd (Secretariat for Public Education, 2010a). Hence, if anything, this cyclicality effect should increase the number of students with just-completed degrees. Further, individuals who continue their formal education may pursue undergraduate studies and postgraduate studies. The former takes typically 4 years, the latter 1 or 2 years (ANUIES, 2004). Hence, the analysis is expanded by comparing the outcomes for the cohorts of 18, 22, 24, and 25-year-olds, with the last group being out of all education according to schedule. The sub-analysis employs equation (3.1), adjusting a for each age class, for example $a = 18$, including the age-specific population weights.

Table (B.9) presents the results. They largely confirm the previous analysis. For 18-year-olds the estimated coefficients tend to increase in magnitude. However, translated to changes in expansion and absolute changes, the effects are comparable to those for 17 and 18-year-olds together. As such, the results do not seem to be driven by age-patterns or timing-of-survey effects. Furthermore, the pattern across the types of disasters is unchanged. In particular, the effect of climatic disasters is larger than for GEO disasters. Concerning the placebo cohorts, as expected, the attainment rates of upper secondary schooling do not appear to be affected by disasters from the observed period. For 22, 24, and 25-year-olds, all estimated coefficients of the double difference are insignificant with the exception of LIV for 24-year-olds which is borderline significant ($\hat{\tau}_{\text{LIV}} < 0; p = .0942$) and GEO for 25-year-olds, displaying a positive effect.

3.6.2. Duration of disasters

Roughly 2% of the events/disaster data provided by DesInventar yield information on the duration of the disaster. After supplementing these data with all available information from the EMDAT database for Mexico, the share of entries which include duration information is about 6%, i.e. still very low. The duration information remains questionable, for example suggesting events lasting over multiple years. Notwithstanding this significant data limitation, the potential duration impact is investigated. The duration information is taken at face value but capped at 90 days. Hence, a flood which lasted for 5 days will now be tracked as five event days. The choice of 90 days as a cap is arbitrary in itself but chosen as representing roughly the length of a season as weather, climate, and long-lasting disasters are expected to correlate with the latter.

Table (B.10) repeats the main estimation with the adjusted disaster information, yielding virtually the same conclusion. In particular, the initial pattern across the types of disasters is preserved. Repeating the sub-analysis for the intensive margin as well as for older cohorts of 18, 22, 24, and 25-year-olds confirms the previous findings too.[28]

3.6.3. Weights

Following Solon et al. (2013) equation (3.1) applies population weights to account for potential heteroskedasticity amplified by varying sub-population sizes in different census waves. Table (B.11) revisits this choice, reporting estimates solely based on individual weights to obtain municipality year level data and Eicker–Huber–White standard errors in the regressions. The age-specific population weights have been omitted. While the main result is confirmed, surprisingly, the estimated standard errors slightly decrease. The reason may be that the underlaying error terms from individual level observations yield a positive intra-group correlation while at the same time the number of observations (17-18-year-old individuals in each municipality) is large enough to approximate the aggregate level error by the group level common component of the individual error terms (Solon et al., 2013). Since the deviation is minor, the weighted case will be carried on.[29]

[28]Results available upon request.

[29]As for the main results, also when omitting weights, the growth differentials as well as the estimated absolute margins are economically significant. Results available upon request.

3.6.4. Clustered standard errors

The results in table (3.2) report Eicker–Huber–White robust standard errors. Given the application of fixed effects and that the treatment occurs at the municipality level which coincides with the level of observation, the reported standard errors are the same when clustering at the municipality level. Clustering at the next higher level (states) leaves the estimations with only 30 groups which are unbalanced in size (mean$\approx$ 218.82, $SD \approx$ 204.34). Hence this approach is not recommended (Nichols and Schaffer, 2007). Nevertheless, performing the analysis for robustness shows that the estimated standard errors are hardly affected, confirming the obtained results.[30]

3.6.5. Validity of disaster indicators

Natural disasters are not always exogenous. Some regions may be more exposed to disasters than others, such that residents and public administration may anticipate the (re-)occurrence of disasters such as floods or droughts (De Janvry et al., 2006; Reardon et al., 1988; Skidmore and Toya, 2002). This implies that the original estimates may not be consistent.

The bias could be in either direction. It may be upwards (more negative) since poorer areas/municipalities may suffer more impactful disasters while also reporting relatively worse schooling outcomes, which are also more likely to be impacted by a disaster. Likewise, if the more affluent/less vulnerable population of a municipality (temporarily) migrates such that only the more perilous share of the inhabitants is affected, the estimates may be biased upwards too. In contrast, the bias may be downwards (less negative) if the population and/or public administration takes precautionary measures, thereby mitigating the impact.

To test the direction of a potential bias the disaster indicator will be modified in two dimensions. First, by varying the threshold for the inclusion of an event, and second, by imposing additional sample conditions. The subsection further assesses the validity of the measures by varying the control group.

3.6.5.1. Variation of the threshold

Table (B.12) revisits choices of the disaster indicator definition put forth in section (3.4.2) by varying the inclusion threshold. Row 1 includes all events. Rows 2 to 6 present the results for zero to four SD, respectively, i.e. row 4 reproduces the main results from table (3.2). The set of estimations yields two results: First, the pattern of the main

[30]Results available upon request.

results tends to be recovered. Climatic shocks have a stronger impact than GEO events with LIV disasters possibly having the strongest impact, followed by PHY. This gives additional confidence in the main results as they do not appear to be driven by the disaster indicator inclusion threshold.

Second, the results indicate that more anticipated realizations tend to yield slightly lower (less negative) effects for climatic disasters. This can be expected as they tend to be relatively more predictable. Less anticipated outcomes, via increasing the number of required SDs, tend to increase the estimates.[31,32] For GEO disasters, the estimates remain roughly unchanged for both lower and higher SD, with the exception of the last row. On the one hand, this corroborates the initial classification of the disaster classes as it supports the hypotheses that GEO disasters tend to be more exogenous. On the other hand, for the possibly endogenous disasters, the results suggest that the bias would drive the estimates downwards (less negative). This partially supports other findings from disasters in Mexico (Rodríguez-Oreggia, 2013), Indonesia (Kirchberger, 2017), and Haiti (Heidelk, 2019) that individuals do not tend to migrate in anticipation of disasters, but also suggests that the public/local government takes measures to limit the impact of disasters.

3.6.5.2. Variation of the sample conditions

Building up on the tests concerning the threshold, another dimension of unexpectedness is introduced if disasters strike in municipalities where they did not strike before. Table (B.13) presents the results, limiting the sample to municipalities which did not record any disasters of their class in the previous five or ten years.

In row 1, limiting the sample to municipalities which did not experience the relevant disasters in the last five years yields a similar result as previously. The climatic disasters dominate though LIV and PHY display similar magnitudes. When conditioning on ten disaster-free years in row 2, the estimate for GEO and LIV remain comparable whereas the estimate for PHY increases strongly. The latter reflects the same movement for $\hat{\tau}_{\text{PHY}}$ as observed in table (B.12) when increasing the threshold. This suggests that relatively more unexpected and unexpectedly intense realizations of PHY yield stronger impacts. In turn, this also indicates that PHY disasters may be relatively more anticipated. This implies that the obtained PHY results might be relatively less robust than for LIV and GEO, which both display more robust estimates.

[31] This holds particularly for PHY disasters. The measured impact changes the most across the specifications, over 30% decrease from row 4 to 1 and about 60% increase from row 4 to 6.

[32] Theoretically, increasing estimates as a function of the threshold could also indicate a frequency effect. However, the assessment of the intensive margin in section (3.5.2) has shown that this is not the case.

3.6.5.3. Variation of the control group

The initial definition of the disaster indicator amounts to comparing municipalities with surprisingly numerous disaster realizations (lagged state mean plus 2 SD) to those which did not experience any disasters as well as those that experienced relatively few disasters. Hence, the developments in the control group may be influenced by those with fewer disasters. To test the validity of the results, table (B.14) builds on table (3.2) by refining the disaster indicator. The basic functioning of the indicator is the same, but the control group is limited to municipalities without a disaster in the period 2005-2010. As such, municipalities with few realizations are omitted, leading to the observed change in the number of observations in the table.[33] The main results and the pattern across the disaster classes are confirmed.

3.6.6. Multiple disasters

Multiple disasters may occur at the same time, i.e. the disaster indicators in columns (2) through (4) in table (3.2) are not mutually exclusive. As such, a municipality may record both climatic shocks for the same period while others report only LIV or only PHY. Table (B.15) provides an overview concerning the overlap of the disaster indicators.

To test the impact of multiple events, dummy variables for all possible combinations of disaster occurrences are created. This yields eight possible combinations, creating eight distinct disaster indicators: (1) no disaster, (2) only GEO, (3) only LIV, (4) only PHY, (5) GEO and LIV, (6) GEO and PHY, (7) LIV and PHY, and (8) GEO and LIV and PHY. Equation (3.1) is transformed to accommodate the new setting in equation (3.3):

$$y_{amst} = \beta_0 + \beta_2 t_t + \sum_{\varsigma=1}^{8} \left\{ \beta_1^{\varsigma} d_m^{\varsigma} + \tau^{\varsigma} (d_m^{\varsigma} \times t_t) \right\} + Z'_{amst}\gamma + \nu_m + \zeta_s + \zeta_s \times t_t + \varepsilon_{amst} \tag{3.3}$$

with all variables defined as in equation (3.1), apart from d_m^{ς} which indicate the eight different disaster combinations. The eight τ^{ς}s are the coefficients of interest.

Table (B.16) presents the results, focusing on the derived $\hat{\tau}^{\varsigma}$s. The occurrence of multiple disasters has a negative impact exceeding the treatment effects of shocks to only one class. LIV disasters persist to have a negative impact, also when occurring alone. In turn, GEO and PHY disasters may not drive the outcomes if occurring alone but mainly if coinciding with other disaster classes. This extends the previous findings by suggesting specific attention shall be given to regions which experience multiple shocks.

[33] Referring to the introductory example in section (3.4.2), municipality A would be excluded.

3.7. Channels and discussion

Disasters can affect educational outcomes in a multitude of ways. This paper considers two main avenues: changes in demand and in supply for education. Amongst these avenues, the former is considered through three potential channels: (i) changes in the main activity, for example dropping out of school, entering the labor market, or being NEET, (ii) changes in parental labor market outcomes, and (iii) alterations in behavior which may be related to risk-taking and inter-temporal discounting factors. Concerning the supply of education, the paper considers: (i) destruction of human and physical capital and (ii) reduction in the municipal population share of teachers.[34]

3.7.1. Demand for education

3.7.1.1. Change in main activity

The negative effect of disaster exposure on educational attainment directly relates to school enrollment. Two avenues are conceivable. On the one hand, the school enrollment rates of 17-18-year-olds may increase after disaster exposure if students get delayed in response to disasters and hence take longer to attain their secondary education degree. On the other hand, school enrollment may drop if the adolescents opt for outside options such as working, helping out their family, or doing 'nothing', for example due to lack of alternatives. The latter could materialize if students drop out without their degree. This would make them relatively less competitive entrants to a labor market which may already be tight following a disaster, for example given the possible destruction of productive assets, decrease in employment opportunities, and hence local (temporary) oversupply of human capital.[35]

Table (B.17) presents the results for school enrollment. All disaster classes are found to have strongly negative effects on enrollment, thereby hinting that the reduced attainment results are partly explained by an at least temporary rise in dropouts. The main alternatives to schooling are paid work or becoming NEET. The disaster effects on the share of employed 17-18-year-olds and on the weekly number of hours doing paid work are thus explored. No significant effect is found on either of these two variables, i.e. adolescents seem to neither enter the labor market earlier nor work at a greater intensity following a disaster.[36] This corroborates past studies that found no or few effects of shocks on young people's labor supply (Cunningham and Salvagno, 2011; McKenzie, 2003).

[34]Disaster exposure may also negatively affect mental health, for example via post-traumatic stress disorder (Black, 2001; Chen and Wu, 2006; Evans and Oehler-Stinnett, 2006). Unfortunately, the given data does not allow to test for this dimension.

[35]Other channels for a tight post-shock labor market are possible too.

[36]Results available upon request.

In line with the weak labor market response, table (B.18) hints to an alternative channel: following the shocks, students who drop out of school tend to turn into NEETs.[37] This is likely to be the most scarring option. It implies neither investing into nor utilizing their human capital whilst its value depreciates. This development is particularly concerning given existing evidence on a positive relationship between unemployment and crime prevalence in Mexico (Dell et al., 2019) as well as in other countries (Baharom and Habibullah, 2008; Adebayo, 2013).

For enrollment, the measured impact is strongest for PHY disasters and weakest for GEO events. Likewise, the increase in NEETs is highest for PHY. The NEET rates in the treated municipalities increase by approximately 81.33% relative to the control area.[38] The absolute increase is estimated to be between 12.64% and 134.19% and thereby economically meaningful. For the class with the weakest impact, LIV, these values drop to 33.53% for the ratio of the slopes and to 2.60% and 5.21% for the absolute range. GEO displays comparable values. This suggests different underlying channels for the three disaster classes. The effect of PHY shocks may mostly travel via students leaving school, for instance. By contrast, LIV disasters – which tend to have the highest impact on degree completion but a relatively weaker impact on enrollment or NEET rates – may also work via changes in altered student performance. While not directly testable here, similar findings are documented by Zander et al. (2015). Notably, heat stress resulting from heavy temperature changes (including during heat waves and droughts) has been shown to induce concentration lapses, higher levels of fatigue, poor decision making because of time perception change (Morabito et al., 2006; Tawatsupa et al., 2013; Tamm et al., 2014), and increased stress hormone levels which also affect cognitive performance, decision quality (McMorris et al., 2006; Gaoua et al., 2011) and, transitively, productivity loss.

3.7.1.2. Parental income

Following a disaster, a negative change in the household income or the parents' labor market affiliation may affect the adolescents' attainment in several ways. First, if parents experience a drop in income, students are further susceptible to dropping out given the various financial costs related to schooling and degree attainment. Table (B.19) shows that aggregate parental income from labor (for example wages, business, farm) is significantly negatively affected by both types of climatic disasters. Corroborating past findings of the adverse effects of household income shocks on schooling outcomes, the results sug-

[37] Technically this could also be driven by a labor market response, but the relevant employment shares are not affected by the disasters.

[38] This assumes that $\hat{\beta}_{2,\mathrm{PHY}}$ is indeed 3.671. Given the estimate's statistical insignificance, the true value may be closer to zero, implying a higher potential growth differential.

gest that the observed negative effects for climatic disasters are influenced by binding household budget constraints (Jacoby and Skoufias, 1997; Jensen, 2000; Duryea et al., 2007; Gitter and Barham, 2007).

The aggregate parental income effect is predominantly driven by fathers whose income response is largely reflected in the aggregate.[39] In turn, a significant rise in mothers' income is found following GEO disasters. This is neither driven by mothers entering the labor market nor by mothers increasing their working hours.[40,41] This surprising result might partly explain the lack of effect of GEO disasters on aggregate parental income. Furthermore, as evidence suggests that an increase in maternal income positively affects children's educational outcomes (Schultz, 2004b; Skoufias and McClafferty, 2001), more so than paternal or parental aggregate income (Haddad et al., 1997; Lundberg et al., 1997; Thomas, 1990), this result could partially explain the lesser adverse effect of GEO disasters on educational attainment.

Beyond the direct income effect, an alternative explanation for the observed effect is that a decrease in the household's financial means may require children to generate income too, contributing to the aggregate household socioeconomic status. However, the results from section (3.7.1.1) do not provide evidence for this. Similarly, if parents would respond to decreasing income by increasing their labor supply to make up for income losses in other jobs, children may need to pick up tasks in the household. However, table (B.20) shows that, following disasters, the share of 17-18-year-olds with employed parents drops significantly. Mothers seem to be solely affected by LIV disasters, fathers are shown to be affected by all types of events and in particular by climatic shocks. This, in combination with the results from section (3.7.1.1), is evidence against this channel.

Lastly, the predominantly negative parental labor market response can affect an individual's perception regarding to education. In particular, the 17-18-year-old students can be expected to observe the impact of the disasters on their parents' labor market outcomes. Given constant parental levels of education, the negative income and employment effects yield decreasing individual returns to education. The latter may affect the perceived value of education in the labor market, from the point of view of the 17-18-year-olds. Hence, while not directly testable with the data at hand, the negative impact of the disasters on parental labor market outcomes may lead to deteriorated educational outcomes for the adolescents, given that perceived returns to education have been shown to matter for schooling outcomes (Nguyen, 2008; Jensen, 2010).[42]

[39] In 2000, on average, the ratio of maternal income to paternal income was below .25.

[40] Results available upon request.

[41] Also following a GEO disaster, Heidelk (2019) finds relatively beneficial post-shock labor market adjustments by females following the 2010 earthquake in Haiti. The investigation of the precise channel behind this surprising result is not explored here and left for future research.

[42] See also Orazem and King (2007) who allow for including the expected returns to schooling in a model of demand for education.

3.7.1.3. Change in behavior

Given that schooling was not obligatory for the students in the sample, the change in educational attainment rates can be the result of changes in general behavior, which in turn may be affected by shifts in risk attitudes or preferences. Past literature has indeed shown that preferences respond to exposure to adverse shocks. For example, individuals exposed to violent conflict may become more risk seeking, increase their discount rates, and alter their investment decisions (Voors et al., 2012; Evans and Oehler-Stinnett, 2006). Similarly following a natural disaster, individuals can either become more risk-loving, for example if they end up feeling more capable to deal with risky situations once they get back on their feet (Bernile et al., 2017; Eckel et al., 2009), or risk-averse, for instance if the augmented stress makes them 'hypervigilant' (Ahsan, 2014; Cameron and Shah, 2015; Cassar et al., 2017; Poertner, 2008; Samphantharak and Chantarat, 2015). As pointed out by Lopes (1987), decision making under risk is a trade-off between hope and fear. The latter may relate to how individuals discount the future which in turn can affect attainment since education is an investment into one's own human capital, thereby requiring individuals not to overly discount the future.

3.7.1.3.1. Fertility

One way to test for shifts in preferences which also relate to schooling is fertility. Selecting into teen pregnancy is indeed likely to have short and long term impacts, and can lead individuals to drop out of school temporarily or permanently.[43]

Corroborating past findings by Finlay (2009), Leyser-Whalen et al. (2011), and Nobles et al. (2015), table (B.21) shows a positive effect of disasters on the number of own children of 17-18-year-olds, in particular for LIV and PHY events. While the absolute magnitude of $\hat{\tau}_{\text{LIV}}$ and $\hat{\tau}_{\text{PHY}}$ may suggest negligibility, indeed $\hat{\tau}_{\text{LIV}}$ yields an economically significant growth differential of 48.24% and an absolute increase of fertility in exposed municipalities by about 8.88% to 10.10%.[44] This provides further evidence for the potential long-term effects on individual development that climatic disasters can have. This effect is indeed likely to impact the future social and economic outcomes for the young parents as well as for their offsprings (Caruso, 2017).

Several avenues are conceivable to explain the change in fertility rates. According to the terror management theory (Solomon et al., 1991), natural disasters augment the fear of

[43] Fertility can also rise due to increased violence after an adverse shock such as a natural disaster as also observed after the 2010 earthquake in Haiti (Hauge, 2018; Duramy, 2012; Amnesty International, 2011; Kolbe et al., 2010). No direct measure of this type of insecurity is available to control for it. However, by including municipality level violent conflict prevalence, it is hoped to approximate this aspect while acknowledging the imperfectness of the measure in this context.

[44] Correspondingly for PHY the slope ratio is 38.45%, the absolute effect is in between 8.31% and 8.46%.

death. Survivors may thus increase their commitment to their partner as a way of coping with such fear (Cicatiello et al., 2019). Similarly, the attachment theory posits that individuals may manifest attachment needs following a time of acute stress (Bowtby, 1982; Hazan and Shaver, 2004). Thus, disasters may lead to rising marriage rates, as found by Cicatiello et al. (2019) and Cohan and Cole (2002), and the increasing fertility rates could then be the result of such changes. However, no significant effect on marriage rates following disasters is found, thereby excluding both explanations.[45]

Fertility rates may also change as a result of altered preferences or risk attitudes as elaborated above, for example as a result of increased rates of unprotected sex.[46] To better understand who might be affected by this change in behavior, a gender-specific sub-analysis is undertaken. The sample is expanded by a gender dimension, with one observation per cohort, gender (male, female), municipality, and year. Following equation (3.1) is expanded by interacting $(d_m \times t_t)$ and its components with a dummy for gender. Table (B.22) shows the results, displaying that the observed changes in fertility are mostly driven by females.[47,48] This suggests that the fathers either do not know/acknowledge the offsprings, or that they belong to different age cohorts. Hence, if indeed changes in risk attitude drive the observed pattern, it appears as if the fallout concerning secondary school completion is fully carried by the young women. As such, young men may be affected less or not at all by the altered behavior and its consequences, and hence have relatively lower incentives not to pursue such risky behavior.

3.7.1.3.2. Violence in Mexico

Since 2006, Mexico has been exposed to booming homicide rates following the implementation of the 'War on Drugs' by the freshly elected National Action Party (*Partido Acción Nacional*, PAN) candidate Felipe Calderón. Dell (2015) provides empirical ev-

[45] Results available upon request.

[46] A change in preferences as indicated by increased risk taking is but one possible explanation. An avenue to test this could be to study rates of sexually transmitted diseases which should also respond positively to increased risk taking. Unfortunately, the setting at hand does not allow for testing this directly. A common dataset possibly covering this topic such as the Demographic and Health Surveys is not available for the time span under consideration in this paper (ICF, 2020).

[47] Defining a variable denoting gender as $gender = 1[\text{individual is male}]$, the estimates of the double difference remain positive whereas the triple differences are negative, leading to a zero net effect. The magnitude of the simple interaction is roughly double of the observed effects in table (B.21). The same pattern is found when estimating the coefficients separately for gender-specific samples instead. In table (B.22) for PHY, the triple interaction is just about statistically insignificant. However, employing gender-specific samples instead, the magnitude in the male sample of the coefficient $\hat{\tau}_{\text{PHY,male}}$ is over 2.5 times smaller than the corresponding value $\hat{\tau}_{\text{PHY,female}}$ from the female sample, hinting to an economically less significant response.

[48] Given the gender-specific changes in fertility, the main results as well as the estimations for marriage rates are repeated with gender-specific analyses, too, using the same methodology. However, neither yields gender-specific results, confirming the insignificant coefficient for gender from the main results in table (B.4). Results available upon request.

idence that municipalities that elected a PAN candidate in the 2007/2008 municipal election cycle witnessed significantly more drug-related homicides during the municipal president's three-year mandate than municipalities where the PAN lost. This is mostly due to the fact that the 'kingpin' strategy implemented by the armed forces, which consisted in the killing or arresting of drug trafficking organizations' (DTO) leaders, led to group splitting which heightened violence between group members (Dell, 2015). Osorio (2015) provides further evidence that areas where DTO leaders were brought down by law enforcement tended to be invaded by neighboring criminal organizations in search for territorial conquest.

Past findings from the psychological literature have shown that minors that were already experiencing anxiety prior to a disaster are more likely to experience symptoms of post-traumatic stress and general anxiety disorder afterwards (Weems et al., 2007). Meanwhile, Michaelsen (2013) finds evidence that Mexico's drug-related violence had adverse effects on anxiety amongst the local population. Michaelsen and Salardi (2018) find that this same violence had a significant negative impact on educational performance, which is primarily attributable to acute psychological stress among students in the immediate aftermath of local violence. Given these findings, and the likelihood that heightened drug-trafficking activities might increase outside options for adolescents, this subsection thus tests whether high exposure to violence induced by Mexico's 'War on Drugs' seems to worsen the adverse effect of disasters on educational attainment.

To capture this, two avenues are pursued. First, in order to reduce a potential omitted variable bias, all estimations include a dichotomous variable equal to 1 if the municipality had at least one death due to violent conflict over the past five years. Second, Dell (2015)'s findings suggest using a 2007/2008 municipal PAN win as proxy for local exposure to both violence and drug trafficking opportunities. In order to explore this potential channel, an extension of equation (3.1) is employed as follows:

$$\begin{aligned} y_{amst} = \beta_0 + \beta_1 d_m + \beta_2 t_t + \tau(d_m \times t_t) + Z'_{amst}\gamma + \nu_m + \zeta_s + \zeta_s \times t_t \\ + \psi_1 v_{mst} + \psi_2(d_m \times v_{mst}) + \psi_3(t_t \times v_{mst}) + \rho(d_m \times t_t \times v_{mst}) + \varepsilon_{amst} \end{aligned} \tag{3.4}$$

where v_{mst} is a dummy equal to 1 in municipalities where the PAN won the elections in the 2007/2008 cycle, such that $v_{mst} = v_m = 1[\text{PAN win 2007/2008 elections}]$. ρ is the additional coefficient of interest, β_5 corresponds to τ but for the political outcomes.

Table (B.23) presents the results, focusing on $\hat{\beta}_2$, $\hat{\tau}$, $\hat{\psi}_3$, and $\hat{\rho}$. Columns (1) to (4) show no significant additional effect of a PAN win. However, electoral outcomes may not be exogenous to local sociodemographic factors. In an attempt to overcome this issue, a modification of the approach suggested in Dell (2015) is applied by limiting the sample to close elections. The latter is defined as an outcome where PAN was a winner or runner up in the elections and the margin of victory or defeat was no more than five percentage points.[49] As such, the election outcome and possible PAN victory can be seen as exogenous, as in idiosyncratic or too close to anticipate. Pursuing this option leaves the estimations with relatively few (treated) observations in each disaster class. Therefore, the assessment is limited to the aggregate case of all disasters. The results are presented in column (5). The estimation mimics the previous outcome with a null result.

Above results may be conflated by the general political environment. The Institutional Revolutionary Party (*Partido Revolucionario Institucional*, PRI) was in power for the great majority of the 20th century and regarded as associated with corruption, especially with drug traffickers (Rodríguez-Oreggia et al., 2013; Shelley, 2001; Morris and Klesner, 2010; Lawson, 2000). Such political environment may further dampen the incentives to pursue degrees as other factors may be seen as more relevant to success in the society (Murphy et al., 1991). To assess this dimension, table (B.23) column (6) repeats column (5) but focuses on PRI. However, the same conclusion is derived, suggesting that neither the perceived political environment nor conflict exposure drive the main results.[50] In conclusion, this paper does not find any evidence that exposure to violence, heavy drug trafficking activities, or high perceived political corruption further impacts adolescents' decisions to pursue their education after experiencing a disaster.

3.7.2. Supply for education

On the supply side, disasters affect life outcomes via the impact on the ground, for example in the form of destruction of human and physical capital, and what follows from it. In the given context, suffering of human capital is defined as the occurrence of sick, wounded, or deceased individuals as a direct result of the disaster, c.f. DesInventar (2019b). This can imply issues such as teachers dropping out of work which prevents proper instruction, or members of the family being unable to work which limits household income, possibly forcing kids to drop out of school for lack of money or for them to work.

Physical capital destruction is recorded when homes, education centers, or health centers were destroyed or directly or indirectly affected by the disaster, c.f. DesInventar (2019b). As such, physical capital destruction can be seen as a more general proxy for aspects relating to access to schooling.[51] Data on destroyed schools and infrastructure is however limited and feared not to be comprehensive enough. It is indeed conceivable that the relevant reports did not explicitly mention destroyed schools or roads to/from schools

[49]As such, PAN's vote share was between 45 and 55%.

[50]This also adds evidence to Márquez-Padilla et al. (2015)'s findings of a null effect of homicides on municipal enrollment rates in Mexico between 2007 and 2011.

[51]Evidently, physical capital destruction can also affect incomes and labor market structures which in turn may affect human capital investment decisions.

but focused more on damaged and destroyed homes or health centers. While past studies have used wrecked homes as a measure of disaster destruction (Lemons, 1957; Kirchberger, 2017), damaged health centers may induce a higher sense of emergency which is relevant for attracting media attention and (financial) support, c.f. FONDEN (2012) and World Bank and United Nations (2010). The data at hand yield 294 disasters which record that (i) homes or health centers were damaged/destroyed, (ii) the education sector has been affected, and (iii) do not provide information about how many education centers were impacted. Likewise, the data include similar cases for health centers. Hence, the different data sources appear to complement each other. The combined information from damaged homes, destroyed homes, and health centers in combination with education centers will be used to approximate the impact of disasters on education-relevant physical capital.

Table (B.24) columns (1) to (3) show the results for physical capital destruction.[52,53] For the PHY disasters, the impact is slightly lower (less negative) than when considering the aggregate of all events. For GEO disasters the effect is statistically insignificant. Columns (4) through (7) consider the impact of disasters with human casualties. For GEO the magnitude of the estimates is higher (more negative) than for disasters which cause physical capital damage. LIV disasters tend to have the largest (most negative) impact which is also reflected in the growth differential of -60.59%, even exceeding the estimates of this class from the main estimation in table (3.2). PHY disasters yield a negative though borderline insignificant effect (p-value = .1069). The results indicate that, over and above, physical capital destruction may play a relatively lower role than adverse impacts on human capital. Hence, aspects related to the physical access to schooling may not be the key driving force of the disasters' impact.

To further investigate the impact of human capital destruction, the paper explores the effect on the population share of teachers. Indeed, another supply side avenue possibly explaining the impact of disasters on educational outcome is if teachers (or their close ones) suffered injuries or death.[54] This could impede the quantity and quality of teaching received by students. This in turn may affect degree attainment. For example, students who received fewer or lower quality lessons due to the disaster may be less well prepared to take the standardized national exam at the end of upper secondary education. By controlling for migration, other fluctuations in the number of teachers are being controlled for. Table (B.25) shows the results for the change of the share of teachers in municipalities, in response to disasters. A significantly negative effect is found from disasters in general,

[52]Note, LIV disasters did not report damaged or destroyed houses, hence the estimation is omitted.

[53]Given the data quality concerning human and physical capital destruction, the results can only be indicative. Local stakeholders may have incentives to inflate numbers in order to attract domestic and international assistance, or to underestimate the numbers to facilitate a positive image of the region (World Bank and United Nations, 2010; Rodríguez-Oreggia et al., 2013).

[54]A decrease in the share of teachers may also be a response to destruction of schools and hence (temporary) elimination of teacher jobs. However, this is a supply side avenue too.

and PHY disasters in particular. The availability of teachers thus seems to be impeded in areas where highly destructive and repetitive disasters occur, thereby reducing the possibilities for students to complete their education. In turn, LIV disasters do not affect the share of teachers, shedding doubt on the existence of a supply side effect for this disaster class.

3.8. Conclusion

This paper explores the differential impact of various classes of natural disasters on educational attainment. Given its geographic disposition and as such, repeated exposure to a variety of natural disasters, data from Mexico is used to investigate the relationship. The analysis focuses on 17-18-year-olds as they are particularly vulnerable not to complete their degree in response to adverse shocks. This is due to the absence of obligatory schooling in the time span under consideration, as well as the range of possible outside options for adolescents.

The paper finds that exposure to natural disasters reduces the completion rates of upper secondary education of 17-18-year-olds. Climatic disasters which affect living capital ("LIV") tend to have the most negative impact, depressing educational expansion by over 40%. Geological disasters ("GEO") yield the weakest effect, lowering the inter-temporal increase of secondary education by about 23%, which is still economically significant. Climatic disasters which also damage physical capital/infrastructure ("PHY") range in between. While the impact of geological disasters is virtually absent in urban settings, the impact of climatic disasters is highest in those places. A possible explanation for this contrast could be the fact that urban settings typically have better logistics and media support. And as geological disasters tend to be more destructive per disaster, they are more likely to get priority in receiving emergency funds in those areas. The effects are stronger if multiple disasters occur in the same period.

The main results seem to be predominantly driven by demand side effects, including dropping out of school while not entering the labor market, and increasing fertility especially for young women. These effects may be influenced by an observed drop in parental employment and income. Binding household budget constraints may indeed force students out of school or disincentivize them to obtain their degree, given a potential fall in expected returns to education. However, more evidence is needed to substantiate this hypothesis. Supply side effects appear to be driven solely by infrastructure-destructive climatic shocks. On the one hand, via the destruction of infrastructure, including schools and roads. On the other hand, PHY shocks yield a decrease in the municipal share of teachers, thereby indicating deteriorating learning conditions for students.

All disaster types seem to increase the municipal share of 17-18-year-old individuals that are NEETs. Just like the increase in fertility following climatic events, this effect can be highly scarring for future human capital accumulation and capitalization. This paper's findings are thus a strong call for action given the expected rise in climate change, the increasing rates of urbanization, and the increasingly skill-focused local and global labor markets where individuals without degrees are at an ever-greater disadvantage. Further, the results are particularly worrisome given that the NEET status can relate to switching to illicit activities, especially in a country that is highly affected by drug-trafficking, thereby possibly perpetrating vicious cycles. Finally, the fact that the increasing share of 17-18-year-olds that declare having children is mainly observed among females living in highly urbanized areas shows that this is a particularly vulnerable population that deserves special attention from emergency agencies, especially following adverse covariate climatic shocks.

4. Natural Disasters and Returns to Education

4.1. Introduction

National governments and the global development agenda put a major emphasis on expanding educational attainment which may yield high returns on investment, i.e. returns to education. At the same time, an increasing number of (poor) people are being exposed to natural disasters and hence negative shocks to human and physical capital. Understanding the impact of these shocks is key. First, for individual returns to education as changes in the observed individual returns may affect the perceived value of education. This can influence how much people invest in the education of oneself or offsprings which is particularly relevant in settings which face persistently low levels of educational attainment. Second, for societal returns to education, to direct policy to how to protect the public investment ex ante or which interventions to focus on after a disaster struck.

Research on returns to education roots back to Mincer (1958), Mincer (1974), and Becker (1962). The global average return to an additional year of schooling is of about 9%. For Haiti in 2001, the baseline year of this paper, the returns at are about 8% (Montenegro and Patrinos, 2014; Psacharopoulos and Patrinos, 2018). Few works combined research on returns to education with analyzing the impact of natural disasters. Di Pietro and Mora (2015) and Rodríguez-Oreggia (2013) study the impact of the 2009 L'Aquila earthquake and Hurricanes in Mexico between 2000 and 2011, respectively. They find partial evidence that lower educated individuals fare relatively better following the shock. Other studies focus solely on labor market outcomes such as labor demand, supply, or income. A focus on the development of disaster-related changes in the returns to education is missing.

This paper investigates how the 2010 earthquake in Haiti affected individual monetary returns to education.[1] Earthquakes are rapid-onset disasters with little to no time to react. They can destroy a large share of physical capital, e.g. structures like residential and nonresidential buildings or local infrastructure. Equipment may suffer, too, such as machinery, computers, or medical tools. On the one hand, the destruction implies a negative impact on production and trade chains. On the other hand, it yields a drop in labor demand, e.g. due to destroyed workplaces or given the overall drop in output.

[1] The earthquake caused about $US 8 billion in damages, exceeding Haiti's GDP in 2009. About 40% of all houses were destroyed and between 63,000 and 222,000 individuals deceased.

The impact on the returns to education depends on the type and level of destruction. If only structures are destroyed, returns may not decrease as all people and education groups suffer proportionally.[2] In turn, equipment capital is complementary to human capital. If this type of skill specific capital is destroyed, the incomes of higher educated people may decrease relatively more, implying decreased returns to education. This may be amplified if the educated workers have to find new employment in lower paying sectors, for example due to lack of adequate adjustment opportunities or skill-specific over-saturation of the local labor market. Further, lower educated individuals may participate in a sector-specific post-shock expansion such as in construction, and thereby improve their income situation. Beyond physical capital aspects, the death of a significant share of the workforce yields an increased post-shock human capital scarcity. In effect, the survivors may be able to demand relatively higher wages, implying increasing returns to education. The effects may depend on the relative impacts on different skill groups.

The paper makes the following contributions. (1) it adds to the literature on how disasters affect labor market outcomes by disentangling the impact of different types of capital shocks, also exploring the channels and focusing on a poor country. Previous literature studies e.g. Italy and Mexico, without exploring the underlying labor market mechanisms. (2) the paper provides guidance concerning development and disaster relief policy. Previous research focused on the value of education, neglecting the variance of the returns as a function of the economy's capital stock. High variance may yield (public/private) underinvestment in education or intensified migration push factors. In a poor setting with high proneness to disasters, policy may focus on continued human capital accumulation and (re)building resilient capital such that returns are less volatile. (3) exploiting the exogenous shock, the paper studies and confirms implications of a macroeconomic model by Krusell et al. (2000), in a specific context using individual level data.

Figure (4.1) motivates the analysis. It shows the change in the gap of individual labor income for people with high (H) and low (L) educational attainment, over time and earthquake exposure. In the non-exposed area, the income gap increased, in the exposed areas it decreased, implying relatively decreasing returns to education in the exposed area. Education is simplified to low (at most completed primary schooling) and high (more than completed primary schooling). Individuals are exposed if they live in communes where the earthquake caused non-negligible damage in buildings of good design and construction, slight to moderate in well-built ordinary structures, and considerable damage in poorly built or badly designed structures. Exposure is measured using the spatially weighted average of Peak Ground Acceleration (PGA), on the commune level. PGA is an indicator

[2] If the local economy faces total destruction, the local (labor) market breaks down entirely, and production equals zero, returns are expected to decrease.

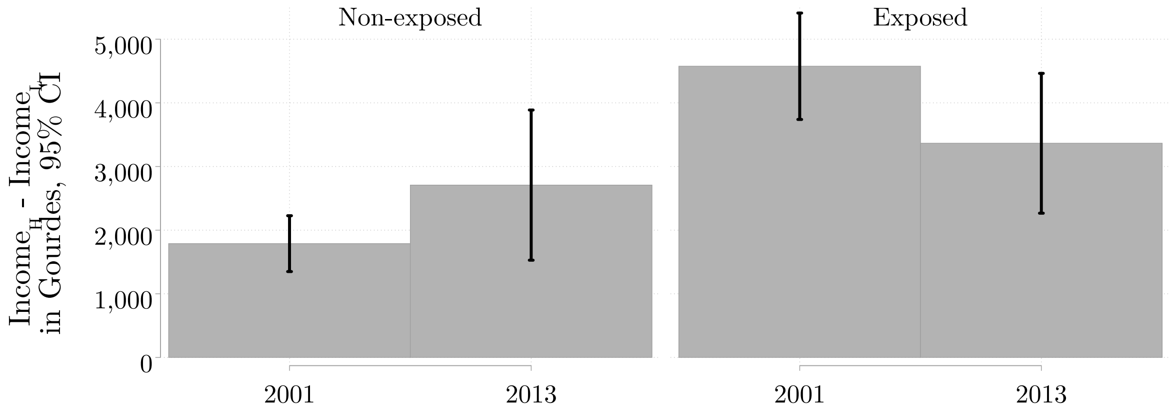

Figure 4.1.: Differences in labor income for people with high (H) and low (L) education.

for the energy released during the earthquake and local shaking intensity. It is a strong, exogenous, predictor of actual destruction on the ground (U.S. Geological Survey, 2016).

To investigate the earthquake's impact on returns to education the paper employs an extended Mincerian equation. The coefficient of interest is the double interaction of disaster exposure, time, and schooling. The paper uses individual level repeated cross section data from before and after the earthquake, in the absence of individual level panel data. Earthquake exposure is unpredictable and exogenous, yielding a natural experiment. The principal hypothesis to test is if and how the returns to education change if people live in areas which experienced a negative shock to physical and human capital. The main result is that individuals who were exposed to the 2010 earthquake in Haiti face an average 37% reduction of their individual monetary returns to education. The impact is nonlinear across the attainment and exposure distributions. Higher educated and those who experienced stronger exposure suffer a greater decrease of their returns. Channels are sector-specific declines in the returns and post-shock non-beneficial labor adjustments.

Concerns pertaining to the validity of the results may arise, especially because (1) the earthquake's proximity to the capital region and other urban areas which may affect the comparability of the exposed versus the unexposed individuals, (2) Haiti's population was also exposed to other covariate shocks such as Hurricane Sandy and the 2010 Cholera outbreak, and (3) domestic and international migration, e.g. in response to the earthquake or search for better labor market opportunities. The paper addresses these factors, the results are robust. Indeed, the effects are more pronounced among individuals living in urban environments. Further, internal migrants, males, and people over 25-year-old appear particularly sensitive to the shock.

The paper proceed as follows: section (4.2) introduces the relevant literature. Section (4.3) provides background concerning the 2010 earthquake in Haiti. Section (4.4) develops the economic theory. Section (4.5) introduces the quantitative foundation for the analysis. Section (4.6) presents the results. Section (4.7) assess robustness checks. Section (4.8) explores the channels. Section (4.9) concludes.

4.2. Literature review

The paper relates to three strands of literature: (i) returns to education in the labor market, (ii) impact of (natural) disasters labor markets, and (iii) educational attainment.

4.2.1. Returns to education in the labor market

The concept of monetary returns to educational attainment was introduced in seminal contributions by Mincer (1958), Mincer (1974), and Becker (1962). Applying so-called Mincerian estimations, globally the average return to an additional year of schooling is about 9% (Psacharopoulos and Patrinos, 2018). Existing evidence for less affluent societies such as Haiti remains scarce. For 2001, Montenegro and Patrinos (2014) measure the annual net returns for one additional year of schooling to be 8.3%. The returns for completing primary, secondary, and tertiary education are estimated at 24%, 41%, and 167%, respectively. Also for 2001, Verner (2008) estimates the returns for degree attainment at 7% and 106% for primary and tertiary education. Using 2007 data Scot and Rodella (2016) estimate the returns to be between 33% and 260% for the three different levels.

focusing on less affluent countries, factors affecting returns to education are, among others, individual characteristics (e.g. gender, family background, age, mortality, career progression, cognitive ability), individual level of attainment, and the labor market environment (Montenegro and Patrinos, 2014; Peet et al., 2015; Schultz, 2004a; Estevan and Baland, 2007; Verner, 2008; Scot and Rodella, 2016; Colclough et al., 2010; Barouni and Broecke, 2014; Fafchamps et al., 2009; Lassibille and Tan, 2005).

4.2.2. Impact of (natural) disasters on the labor market

The impact of (natural) disasters on the returns to education in post shock labor markets is hardly addressed. Di Pietro and Mora (2015) study the 2009 L'Aquila earthquake, finding that the immediate impact of the earthquake has a relatively worse impact for more educated individuals. In Mexican post-hurricane labor markets Rodríguez-Oreggia (2013) find that lower-educated persons tend to fare better than higher educated peers. However, the evidence is mixed, in some cases the opposite held true, leaving the role of education as a testable question in this paper. This paper goes beyond Di Pietro and Mora (2015) and Rodríguez-Oreggia (2013) by differentiating the impact by types of shocks, studying the channels, and controlling for individual (post-disaster) migration.

Negative shocks may have a negative impact on labor demand. This was observed following the 2010 earthquake in Haiti (Kirsch et al., 2012), in Indonesia (Kirchberger, 2017), and the 2008 L'Aquila earthquake (Di Pietro and Mora, 2015), but not after the 2010 earthquake in Chile (Karnani, 2015) or the 1995 Kobe earthquake (Horwich, 2000; duPont and Noy, 2015). Labor supply may be affected too. For the 2010 earthquake in Haiti labor supply appeared to have increased (Herrera et al., 2014).

Labor income may be affected too. For the 2010 earthquake in Haiti some evidence hints to a drop in individual income, subject to the sector of activity (Kirsch et al., 2012; Herrera et al., 2014). However, people who successfully adjusted after a shock tended to fare better afterwards, e.g. following the 1998 Bangladesh Flood where adjusting individuals had a lower decrease of income compared to their peers (Mueller and Quisumbing, 2011), following the 2006 Indonesia earthquake where adjustment into the construction sector was beneficial, yielding higher wage growth (Kirchberger, 2017), or in post-shock settings after hurricanes in Mexico between 2000 and 2011 (Rodríguez-Oreggia, 2013).

4.2.3. Educational attainment

Educational attainment is low in Haiti despite sizable returns, possibly due to limited access due to high schooling costs. Primary school attainment increases sharply when costs are lowered (Adelman et al., 2017). However, keeping aspects of supply/costs constant, low educational demand may be rationalizable in the Haitian context. For higher education to yield high returns for an individual, also other people in the society need to foster specialization. Otherwise the obtained high skills will not be in demand, leaving the higher educated individual possibly worse off. The poor institutional setting, high corruption, and stagnating (or declining) per capita income may further depress local aspirations to pursue higher education. This paper explores if the exposure to shocks may be an additional factor via dampened returns to education.[3]

[3] Evidence from the Dominican Republic and Madagascar indicates that increasing information that the returns to education are higher than expected may yield increased educational attainment (Jensen, 2010; Nguyen, 2008). As such it is conceivable that children who observe a decreasing monetary value of education, for example of their guardians, may be disincentivized to pursue schooling which could happen alongside increased pessimism concerning the economic outlook given Haiti's repeated exposure to natural disasters. More evidence is required to explore this relationship.

4.3. The 2010 Haiti earthquake

The earthquake struck Haiti on January 12th 2010 with a magnitude of 7.0Mw on the Richter scale. It was the strongest earthquake for Haiti in 200 years. The epicenter of the earthquake was south west of the capital Port-au-Prince. The disaster implied a strong negative shock to physical and human capital. The impact was spatially heterogenous.

Pertaining to physical capital, the earthquake destroyed (destructed) 105,000-130,000 (208,000-915,000) houses and workplaces, damaged 41% of all dwellings, led to a loss of many jobs, and caused a total damage of about US$8,000,000,000, exceeding 100% of Haiti's GDP in 2009 (World Bank, 2014a; Herrera et al., 2014; Doocy et al., 2013; Government of Haiti, 2010; Guha-Sapir et al., 2015). Pertaining to human capital, the estimates for deceased humans differ widely. UNOCHA (2011) give the death toll at 222,570. However, survey data suggest the casualties to be 63,000-66,000. The casualties do not differ significantly by educational attainment, possibly because the earthquake struck on a workday during working hours (Tuesday, 4.53PM local lime), combined with the overall very poor building standards in Haiti as most death occurred due to collapsing buildings. Evidence concerning casualty differentials by gender and age is mixed, c.f. Herrera et al. (2014), Doocy et al. (2013), Schwartz et al. (2011), and Kolbe et al. (2010). Further, the earthquake killed 20% of its civil servants and police force (Rosen, 2012).

Different earthquake impact regions can be identified using the Modified Mercalli Intensity scale (MMI) which is based on objective geological measures such as the released energy during an earthquake as well as the intensities of the earthquake on the ground, derived either by observational data or by instrumenting/predicting ground destruction based on existing data (Worden and Wald, 2016). For the 2010 Haiti earthquake the MMI is highly correlated with the objective PGA measure, i.e. areas which were exposed to greater released energy also experienced greater destruction.[4] Pailoplee (2012) made a similar observation in other earthquake settings.

Haiti's earthquake exposure can roughly be divided into three areas, c.f. figure (4.2). First, a region where the earthquake caused no or only minor damages, corresponding to a (scaled) PGA value between 0 and 0.3 and an MMI between 4.6 and 6.5 (World Bank, 2014a, map (4.1)). Second, an area which experienced a negative shock to physical capital. Buildings and equipment may have been damaged or destroyed to a varying degree (PGA $[0.3, 0.65]$, MMI $[6.5, 8]$). Third, a region which was exposed to negative shocks to both, human and physical capital (PGA > 0.65, MMI > 8.). Here, buildings and equipment were damaged or destroyed and humans suffered injuries or deceased. The great majority of the casualties occurred in the metropolitan area (Kolbe et al., 2010; Schwartz et al., 2011) where the earthquake also caused strong physical capital destruction. Therefore,

[4] Results available upon request.

this area experienced a dual shock. Comparing figures (4.2) and (C.1) in the appendix shows that the earthquake hit a population-dense area in Haiti in 2010. Other relatively highly populated areas where not or only limitedly affected.

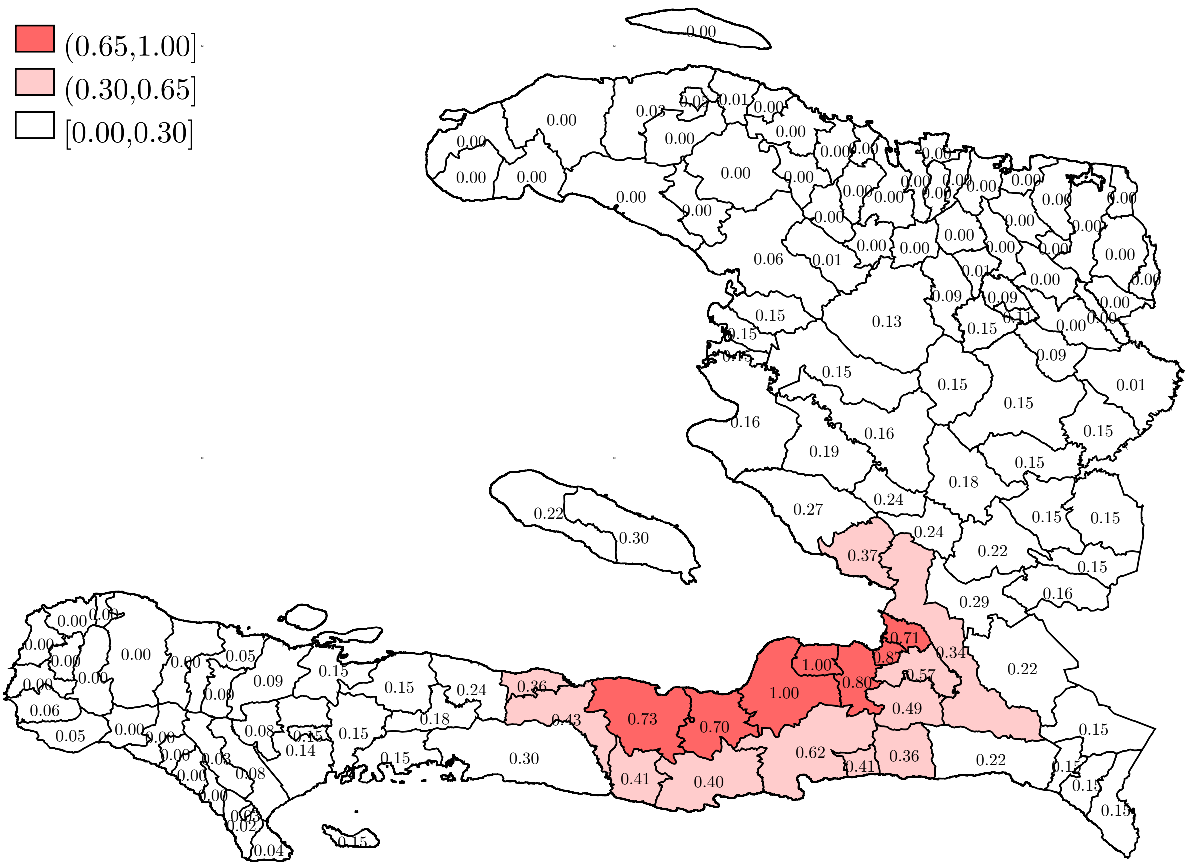

Figure 4.2.: (Scaled) Earthquake intensity (PGA), in communes. Darker shaded areas experienced stronger exposure and destruction.

4.4. Theoretical framework

The theoretical foundation for the analysis builds upon the model by Krusell et al. (2000). focusing on aggregate production and abstracting from individual decision making, the production function takes the form of $Y = G(K_s, K_H, L, H)$, with Y being output, K_s being structures capital (e.g. residential and nonresidential buildings), K_H being high-skill specific capital or equipment capital (e.g. machinery, computers, or medical tools), L being low-skilled workers, and H being high-skilled workers. In particular, the production function is assumed to be of the form $Y = K_s^\alpha Q^{1-\alpha}$, i.e. Cobb-Douglas over structures capital and the human capital part $Q(K_H, L, H)$, which in itself is a constant elasticity of substitution function of the remaining inputs. Assuming equal input shares for simplicity, the full production function is $Y = K_s^\alpha \left\{L^\delta + \left[K_H^\gamma + H^\gamma\right]^{\frac{\delta}{\gamma}}\right\}^{\frac{(1-\alpha)}{\delta}}$.

γ, δ are governing the elasticities of substitution: the elasticity of substitution between K_H and H is given by $\sigma_\gamma \equiv \frac{1}{1-\gamma}$, and the elasticity of substitution between L and the H, K_H aggregate is given by $\sigma_\delta \equiv \frac{1}{1-\delta}$. $\alpha \in [0,1]$, $\gamma, \delta < 1$. Wages for low and high skilled workers are given by $w_L \equiv \frac{\partial Y}{\partial L}$ and $w_H \equiv \frac{\partial Y}{\partial H}$. The skill premium reflecting the returns to education is given by $R \equiv \frac{w_H}{w_L}$. The expected impact of the earthquake on the returns to education depends on the destruction. The 2010 earthquake was undestructive in the far North and far West, and devastating close to the epicenter in cities such as Léogâne. The majority of the high skill specific capital is likely to be situated in the capital region, Haiti's economic, financial, and political center. Hence, areas which experienced a non-devastating shock to physical capital likely experienced only a shock $\Delta K_s < 0$. In the most strongly affected areas K_H, H, and L were destructed too. H, L suffered at comparable shares such that the ratio $\frac{L}{H} \equiv \mu$ was not affected by the earthquake.

The destruction of structures capital has no effect on the returns to education if K_s remains positive. The impact of a negative shock to either of the other three factors depends on the size of the relevant elasticities of substitution. Duffy et al. (2004), Duffy and Papageorgiou (2000), Behrman (1972), and Behar (2010) suggest that in Haiti or in countries/settings comparable to Haiti the elasticity of substitution between high and low skilled labor σ_δ equals 2 ($\delta = \frac{1}{2}$), and the elasticity of substitution between high-skilled human capital and skill-specific physical capital being smaller, i.e. $\sigma_\delta > \sigma_\gamma$, such that $\delta > \gamma$ and with γ being below one and maybe smaller than zero. Hence σ_γ may be below 1 which would imply capital skill complementarity, i.e. high skill specific capital and high skill human capital are rather complements whereas the inputs of low-skill workers vs. the high-skill aggregate may be relative substitutes.

If true, isolated negative shocks to K_H or L will decrease the returns to education whereas an isolated negative shock to H will increase them. In the most affected region, physical and human capital suffer destruction. The combined shock to both types of human capital tends to increase the returns to education, if (i) μ remains constant and greater than one, and if (ii) $1 > \delta > \gamma$, which is assumed given previous literature. Hence, a combined μ ratio-preserving shock to both types of human capital is expected to increase the returns to education, i.e. $\frac{\partial log(R)}{\partial H} + \frac{\partial log(R)}{\partial L} < 0$. The negative simultaneous shock to K_H, L, H, as observed in the most affected regions, will decrease the returns, given the previous assumptions as well as assuming $H < K_H$. For the opposite to hold and for returns to increase, it has to be that $\delta \leq \gamma$, which is not supported by previous literature. Hence, $dR \equiv \frac{\partial log(R)}{\partial K_H} + \frac{\partial log(R)}{\partial H} + \frac{\partial log(R)}{\partial L} > 0$. This also implies that the shock to physical capital dominates the effects of the shock to human capital. Appendix (C.1) provides relevant derivations. The empirical section reviews the theoretical derivations.

4.5. Quantitative framework

This section introduces the data as well as methodology employed in this paper.

4.5.1. Data

The study focuses primarily on two cross sectional surveys. First, the 2001 living conditions survey in Haiti (Enquête sur les Conditions de Vie en Haïti, ECVH) covering 7,186 households and 33,007 individuals. Second, the 2013 post-earthquake survey (L'Enquête sur les Conditions de Vie des Ménages Après Séisme, ECVMAS II). It was the second ECVMAS round, including 2,282 households and 10,887 individuals.[5]

The strength of the earthquake is given by PGA which is the largest acceleration/increase in velocity recorded on the ground during an earthquake, measured in $0.04g$ intervals (Worden and Wald, 2016). PGA is an indicator for the seismic energy released at the hypocenter of the earthquake as well as how strong the earth shook at locations away from the epicenter. The data are obtained from U.S. Geological Survey (2010) and are mapped onto the commune level, reflecting the average exposure of the individuals in each commune. Exposure severity is weighted by commune area.[6] The severity indicators are rescaled onto the interval from 0 (least exposed) to 1 (most exposed), in line with the underlying difference in difference (DID) setting.[7]

The income data are created based on CEDLAS and World Bank (2014) and CEDLAS (2017). Income is measured on a monthly basis based on annual earnings. Data limitations from ECVMAS II do not allow for the calculation of hourly wages or similar. The income information includes autoconsumption of the households, assigned to the head of the household. The data are adjusted for inflation (World Bank, 2017) and urban-rural differences following CEDLAS and World Bank (2014). Information pertaining educational attainment and control variables is available from the ECVH and ECVMAS surveys. Four waves of Demographic and Health Surveys (DHS) (1994, 2000, 2005, 2012) supplement the analysis (ICF, 2018; Cayemittes et al., 1995, 2001, 2007, 2013).

Table (4.1) provides summary statistics of the principal data at hand from the ECVH and ECVMAS II. Appendix (C.2.2) presents all data. The data are limited to people of working age (15-65) in 2001 and 2013. Average labor income increased over time but fewer people report an income. Educational attainment is low but increased over time, in years of schooling as well as degree attainment. The average age and gender-ratio remain constant at about 33 years and 48% males, the degree of urbanization increased.

[5]Unfortunately, the income modules and data from ECVMAS I and II are not comparable.

[6]Exposure could also be weighted by population density but an exogenous measure is preferred.

[7]MMI is an alternative treatment indicator. However, MMI may be endogenous to income since it is a function of pre-shock vulnerability. PGA is strictly exogenous and hence the preferred measure.

Table 4.1.: Summary statistics

	Year 2001		Year 2013	
	PGA<.37 mean (SD)	PGA≥.37 mean (SD)	PGA<.37 mean (SD)	PGA≥.37 mean (SD)
Labor income¶	6.21	6.84	7.31	8.00
	(1.45)	(1.50)	(1.52)	(1.39)
1=Labor income¶ (%)	51.15	46.78	50.99	44.68
	(49.99)	(49.90)	(50.00)	(49.72)
Years of Education	4.65	7.14	5.42	8.00
	(4.93)	(5.67)	(4.31)	(4.38)
1=No degree attained (%)	68.40	49.13	55.77	30.49
	(46.49)	(50.00)	(49.67)	(46.04)
1=Primary education (%)	16.78	17.79	20.74	19.51
	(37.37)	(38.25)	(40.55)	(39.64)
1=Secondary education (%)	9.68	15.50	19.40	33.57
	(29.56)	(36.19)	(39.55)	(47.23)
1=Tertiary education (%)	5.14	17.58	4.09	16.43
	(22.09)	(38.07)	(19.80)	(37.06)
Potential labor market experience	23.18	19.54	22.23	19.13
	(17.12)	(16.50)	(16.58)	(15.13)
Age	33.80	32.65	33.65	32.89
	(14.46)	(13.60)	(14.21)	(12.87)
1=Male (%)	48.47	46.89	49.50	46.29
	(49.98)	(49.91)	(50.00)	(49.87)
1=Metropolital area (%)	0.73	52.61	0.63	80.88
	(8.49)	(49.94)	(7.93)	(39.33)
1=Other urban area (%)	27.96	6.29	31.52	3.99
	(44.88)	(24.28)	(46.47)	(19.58)
1=Rural area (%)	71.31	41.10	67.85	15.13
	(45.23)	(49.21)	(46.71)	(35.84)
Observations	12,685	5,423	3,636	2,856

Each cell shows the mean of the relevant variable, in a given year, in an exposed on non-exposed region. Standard deviations (SD) in parentheses. ¶ Labor income measured monthly, in 2013 prices.

4.5.2. Difference in differences methodology

The empirical analysis employs a Mincerian equation in a DID framework:

$$\begin{aligned} log(y_{ict}) = \beta_0 + \beta_1 S_{ict} + \zeta_1 \omega_c + \zeta_2 T_t + \zeta_3(\omega_c \times T_t) + \zeta_4(\omega_c \times S_{ict}) + \zeta_5(T_t \times S_{ict}) \\ + \tau(S_{ict} \times \omega_c \times T_t) + \beta_2 L_{ict} + \beta_3 L_{ict}^2 + Z_{ict}'\gamma + \varepsilon_{ict} \end{aligned} \tag{4.1}$$

with y_{ict} being log labor income of individual i living in commune c at time t, S_{ict} educational attainment, L_{ict} potential labor market experience (age - 6 - years of education),

ω_c treatment/shock (intensity), T_t time defined as dummy (1=2013), Z_{ict} a vector of personal and environmental controls, and ε_{ict} idiosyncratic error term. τ is the parameter of interest measuring how the returns change over time and disaster exposure. $\hat{\tau}$ yields the intention to treatment effect. Setting ω and T equal zero reduces equation (4.1) to a standard Mincerian equation with β_1 measuring the returns to education.[8]

Treatment is included at the commune level. All of Haiti is at risk of being exposed to an earthquake and has experienced several such shocks in the past. However, earthquakes can hit at virtually any time, with the precise moment, location, and intensity being unpredictable. As such treatment exposure ("assignment") is exogenous. The treatment indicator enters equation (4.1) as the random spatial and temporal variation in PGA.[9]

For baseline specifications Z_{ict} will remain empty. The extended approach includes individual characteristics: gender, a dummy for being an employee (vs. self-employed) due to the specific form of self-employment often encountered in less affluent societies, and the degree of urbanization classified as rural, urban, and metropolitan. When considering the result's robustness to confounding shocks and migration, Z_{ict} will be expanded further.

Following Abadie et al. (2017), the data are clustered based on data sampling design (IHSI, 2013; IHSI, 2003) and experimental design due to the geographically aggregated treatment assignment. Cameron and Miller (2013) advice to cluster at the highest level until which enough clusters are available; for the application at hand the commune level which coincides with the level of treatment, accounting for possible intra-cluster correlation. Since all sampling units are of lower geographical hierarchy the sampling design clustering is implicitly taken care off. The estimations yield over 100 clusters and hence more than the suggested benchmarks of 5 to 30 or 50 requiring additional error corrections (Cameron et al., 2008; Donald and Lang, 2007; Gábor Kézdi, 2004).[10] Higher levels of administrative aggregation have fewer than 50 groups.

Fixed effects control for structural differences across administrative regions. Relevant differences to the observed outcomes can be anticipated on different levels. Departments are relevant from a quality-of-governance and legislative perspective since administrative decentralization happens mainly at the department level. The remainder of policy is conducted on the national level, e.g. wage policies. In addition, department level fixed effects can control for structural differences for aspects such as disaster exposure since most of the disasters are floods or storms which are related to being located at the costal area or having mountainous terrain. Further, these disasters tend to affect more than a single commune which also holds for epidemics and earthquakes. Commune fixed effects

[8] β_1 approximates the education premium for wages, ignoring other costs; see e.g. Meghir and Rivkin (2011), Heckman et al. (2006), and Schultz (2004a) for discussions.

[9] See Kirchberger (2017), Behrman and Weitzman (2016), Gignoux and Menéndez (2016), Saint-Macary and Zanuso (2015), and Baez and Santos (2008) for similar approaches.

[10] See Nichols and Schaffer (2007) for additional discussion.

would be relevant for more detailed aspects, e.g. concerning intra-department differences in soil quality which might affect autoconsumption and labor income, or pertaining to intra-department differences of risk of disaster exposure.[11]

Regional specific time trends are required if the unobserved, structural factors are not constant over time, e.g. if one region has (unobserved) policy in place affecting income (e.g. access to credit), which it also changes at a different speed over time vis-à-vis the other regions (e.g. further liberalization of access to credit). Whereas department level trends are intuitive due to the policy-making role, commune level trends may not be needed from an economics perspective: Risk of disaster exposure is related to geographic location and hence constant, and factors related to productivity or proximity to markets is captured by covariates such as urbanization and migration, c.f. World Bank (2014a).

Empirically, clustering the standard errors limits the rank of the covariance matrix to the number of clusters. Hence, when applying fixed effects at the same level as clusters, model specific tests cannot be executed as the number of constraints exceeds the number of degrees of freedom. Further, t statistics for the fixed effects estimators may be inflated due to underestimated standard errors (Nichols and Schaffer, 2007). Hence, the necessity to include fixed effects at the same level as clusters is statistically not testable.[12]

Hence, to account for parts of possible unobserved heterogeneity within and between treatment groups, inter-department correlation is being modeled by including department level fixed effects in Z_{ict}. The fixed effects are interacted with time to account for possible department specific trends. This approach is in line with previous literature dealing with labor market outcomes in Haiti (e.g. income, productivity, returns to education), using either no or department level fixed effects (Scot and Rodella, 2016; Montenegro and Patrinos, 2014; World Bank, 2014a; Verner, 2008). The substance of the results is unchanged when including commune level fixed effects in the robustness checks.

The analysis applies survey weights on an individual level. The weights from the ECVH and ECVMAS surveys run on different scales. The weights are normalized to the $[1, 2]$ interval to increase inter-temporal comparability. Two alternative approaches exist: ignoring weighting all together or applying the original expansion weights. The robustness checks show that either option does not change the substance of the results.

[11] See World Bank (2014a, appendix O) for maps depicting disaster risks in different regions.

[12] This problem is further worsened when allowing for commune level specific trends. While it might be possible to still do inference on the estimators which are not the fixed effects (Nichols and Schaffer, 2007), the estimates of the coefficients and standard errors of the elements in the X matrix are rendered unreasonable when including commune level specific trends. For example, applying cluster level fixed effects and allowing for cluster level regional trends suggests that in 2013, moving from the least to the most affected regions implied an increase in income by approximately $1,186,970\%$.

4.6. Results

Below section presents the main findings of the paper, addresses threats to identification, and explores heterogeneity of the observed results.

4.6.1. Main results

The returns to education decrease for individuals who live in communes which experienced a strong exposure to the earthquake. Table (4.2) shows the main results, focusing on the coefficient $\hat{\tau}$ from equation (4.1), i.e. the coefficient of the double interaction effect of education × treatment × year. Table (C.2) in the appendix presents the full result.[13]

Table 4.2.: Main results: changing Mincerian returns to education

	Years of education				Levels of education	
	(1a)	(1b)	(2a)	(2b)	(3)	(4)
S (Years) × $\omega_{0/1}$ × T	−0.05**	−0.03**				
	(0.02)	(0.02)				
S (Years) × ω_c × T			−0.07**	−0.05**		
			(0.03)	(0.03)		
Primary × $\omega_{0/1}$ × T					−0.34**	
					(0.16)	
Secondary × $\omega_{0/1}$ × T					−0.24	
					(0.25)	
Tertiary × $\omega_{0/1}$ × T					−0.59**	
					(0.27)	
Primary × ω_c × T						−0.47**
						(0.22)
Secondary × ω_c × T						−0.39
						(0.40)
Tertiary × ω_c × T						−0.76*
						(0.40)
Controls	No	Yes	No	Yes	Yes	Yes
Department FE	No	Yes	No	Yes	Yes	Yes
Dep. FE × T	No	Yes	No	Yes	Yes	Yes
Observations	11,539	8,989	11,539	8,989	8,910	8,910
R^2	0.25	0.32	0.25	0.32	0.32	0.32
F	105.49	89.75	102.89	98.82	107.04	94.29

The dependent variable in all models is *log* labor income including autoconsumption. Controls in Z: gender (male=1, female=0); employment (employee=1, self-employed=0); urban (urban (metropolitan area)=2, urban (other)=1, rural=0; included as two dummies with rural as comparison group); department FE; department FE × Time. Eicker–Huber–White robust standard errors, clustered at commune level, in parentheses. Significance: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

[13] The point estimates for the return to an additional year of schooling for monthly income are between 10.3% and 11.3%. These estimates exceed previous results by Montenegro and Patrinos (2014), Verner (2008), and Scot and Rodella (2016). This may be because this paper uses gross (not net) returns, a different specification (e.g. a less extensive set of control variables), and a partially different sample (e.g. including self-employed individuals).

Columns (1a) and (1b) in table (4.2) present the estimation with education measured in years of schooling and treatment defined as dummy with cutoff at scaled PGA 0.37 (1=exposure value larger than 0.37). Column (1a) yields the Mincerian equation with the relevant interaction terms of treatment, time, and education. Column (1b) extends (1a) and includes the full matrix of controls and fixed effects. The estimate is negative and statistically as well as economically significant. In the baseline specification, the returns to education for the individuals living in the exposed communes decreased by over 32% from ≈15.7% to ≈10.6%. Controlling for other factors in column (1b), the corresponding value lowers to 25%. The chosen cutoff value which assigns treatment status in columns (1a) and (1b) is necessarily arbitrary. Figure (4.3) considers all possible cutoff values of the continuous exposure distribution. The graph shows that the impact of the shock varies over the exposure distribution, displaying a slight negative slope. For low values of the treatment dummy the estimate is statistically insignificant. For values of scaled PGA>.3 the estimates are consistently negative and statistically significant at the 10% level.

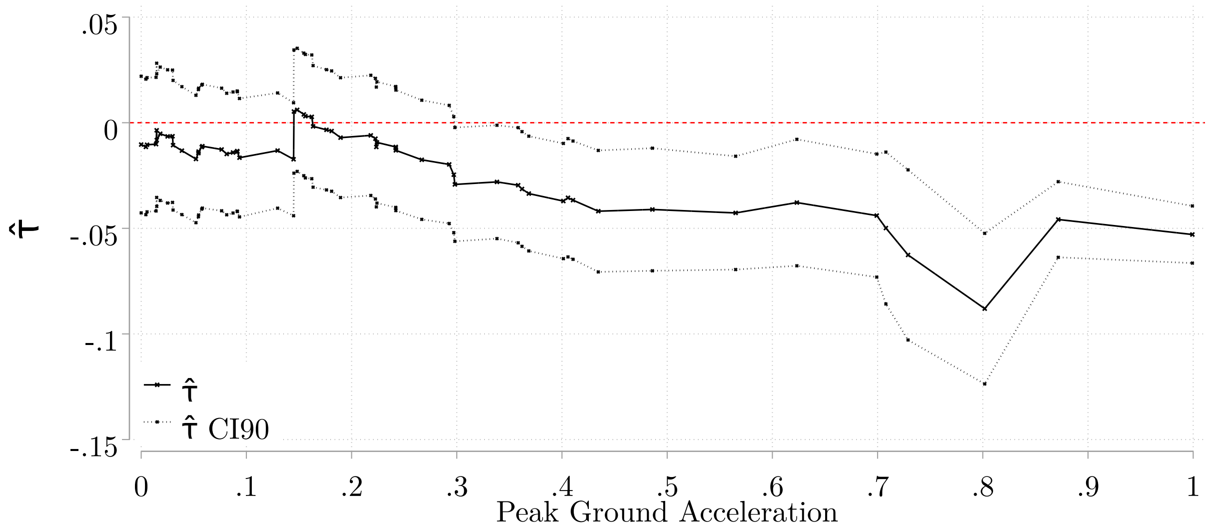

Figure 4.3.: Each dot represents the $\hat{\tau}$ coefficient from estimations for all treatment dummies $\omega_{0/1}$, following specification (1b) in table (4.2).

Columns (2a) and (2b) mimic columns (1a) and (1b), respectively, but replace the treatment dummy with the corresponding continuous treatment indicator. The estimates for the interaction term increase in magnitude, reflecting a unit change in the treatment indicator which runs from zero to one and hence from the least to the worst affected areas. The returns for the individuals living in the exposed communes decreased by 45% in the baseline specification, and by over 37% in the extended setting, from ≈13.7% to ≈8.5%. The results are in line with the theoretical findings. They imply a negative effect of the earthquake-induced negative shocks on returns to education.[14] Further, the results hint to heterogenous results across the types of shocks as also found above.

[14]The analysis ignores possible general equilibrium effects. A possible concern is that in response to the earthquake funds of the central government as well as international aid were solely focused on the

Columns (3) and (4) build up on columns (1b) and (2b), treating education in discontinuous levels for completed primary, secondary, and tertiary schooling. The estimates are relative to no/less than completed primary schooling. The results show non-linearities in the effects across the attainment distribution. Living in an exposed commune significantly lowers the value of primary and tertiary education. The estimate for secondary schooling is statistically insignificant. In column (4), the returns to primary education decrease by ≈ 37%, for tertiary education by ≈ 53%. Extending figure (4.3), figure (C.2) applies the specification in column (3) to all values of the exposure distribution. The returns to primary education decrease statistically significantly for values of scaled PGA greater than .2. For high values of exposure, the returns may not change, i.e. the estimate is statistically insignificant. In contrast, the returns to completed secondary schooling are not affected significantly in areas which experienced only lower values of exposure though for high values of scaled PGA>.7 the change in returns is negative. The returns to tertiary education suffer a decrease for values of scaled PGA>.3.[15,16]

4.6.2. Parallel trends and absence of pre-existing trends

Identification in a DID setting crucially rests on the assumption that in absence of the intervention the treatment and control group(s) would have moved in a parallel fashion such that any observed diversion from the parallelity can be assigned to the intervention. Given that the earthquake had a substantial impact in the metropolitan area, the assumption may be questioned.

4.6.2.1. Income

One concern may be that income developed relatively weaker in the treated regions over time, implying decreasing returns to education vis-à-vis the control region, all else equal. Data limitations prevent direct testing for trend stability before and after 2001. However, secondary data suggest relatively stable poverty rates for Port-au-Prince and the rest of

disaster region, exposing the relatively weaker affected areas to a negative financial shock. The latter is expected to slow economic activity, which might affect local returns to education. The interactions of education and time in table (C.2) suggest that the returns in untreated areas did not change or decreased, suggesting that the observed estimates for the double interaction may be a lower bound.

[15] Including a dummy for being an employee might be considered a "bad control", c.f. e.g. a discussion in Angrist and Pischke (2009). However, rerunning the analysis under exclusion of the potentially bad control variable does not alter the results, as also suggested by the reported specifications bar of any control variables in columns (1a) and (2a).

[16] Following disasters labor saving technologies may enter the labor market, possibly yielding an upgrade of the capital stock. However, all else equal, this technology is likely to substitute workers with relatively lower educational attainment with machines which in turn would be operated by those with relatively higher education. This would imply a decrease of the wages in the lower end of the education distribution and hence increase the returns to education, ceteris paribus, which is not being observed and hence evidence against such influx.

the country between 1986 and 2000 (World Bank, 2006).[17] Understanding poverty rates as a proxy for income, this speaks against the presence of pre-existing trends biasing the results. In addition, in 2001 in the metropolitan area (and few other communes), perceived income tended to increase or remain stable compared to the previous year. In turn, perceived income worsened in virtually the entire rest of the country. Table (C.3) in the appendix confirms a negative correlation between earthquake exposure and worsened perceived income. For the time after 2001, Scot and Rodella (2016) review changes in income between 2007 and 2012. They find that income in urban areas, in particular the metropolitan area, developed relatively better compared to the rest of the country. Herrera et al. (2014) derive a similar conclusion for the same timeframe. They corroborate this finding by highlighting that the perceived income and labor situation in 2012 had worsened at the same rate or relatively less in the metropolitan area compared to other regions, compared to just before the earthquake.[18]

Taken together, income appears to have developed the same or relatively better in the treatment area compared to the control group, i.e. returns may have increased in the metropolitan area, all else equal. Given a negative observed treatment effect, the obtained result would be a lower bound estimate. A similar argument can be made if income in the control groups decreased while it remained stable in the treatment group.

4.6.2.2. Education

Differential trends in educational attainment may affect and bias the observed change in returns. If education increases, returns decrease, ceteris paribus. Hence, DHS data from 1994 to 2012 is used to test whether educational attainment in the treatment areas expanded faster relative to non-exposed regions. Figure (C.3) in the appendix shows that educational attainment grew relatively less in the metropolitan area compared to each other area of Haiti. The relatively lower expansion of educational attainment in the treatment area pushes the measured returns upwards, compared to the control regions. This implies that the observed negative treatment effect is a lower bound estimate.[19]

[17] Lundahl (1996) discusses income data from 1970 and 1976, shedding doubt on their accuracy. Another problem is inter-temporal non-comparability of data, e.g. three expenditure/income surveys for the years 1986, 1999, and 2001 (World Bank, 2006), the ECVMAS waves I and II, or the ECVH and survey on employment and the informal economy (Enquête sur l'Emploi et l'Economie Informelle).

[18] Alternatively, trends in wealth may be explored, e.g. using DHS data 1994 to 2012 with an inter-temporarily comparable wealth index, cf. Rutstein and Staveteig (2014). However, the inter-temporal changes in wealth are a weak and statistically insignificant predictor of changes in income from the primary data. This may be due to different factors other than income driving wealth, e.g. remittances, donations, aid projects providing assets, or theft. As such this avenue is not pursued.

[19] The substance of this result is unchanged when using degree attainment instead of years of education.

4.6.3. Heterogeneity of results over types of shock

The exogenous spatial variation allows to test the differential impact of different types of shocks to which the individuals were exposed: (A) dual shock to both human and physical capital, (B) physical (structures) capital shock, and (C) no/negligible shock. To compare (A) and (B) is to compare people who live in the most exposed communes to those who experienced a milder yet still destructive shock. For each given treatment value $\omega_k, k \in]0,1[$, communes who experienced a weaker earthquake exposure $\omega_j, j < k, j \in [0,1[$ are subsequently cut from the sample. j, k both run along the scaled PGA distribution. Figure (C.4) displays the results for all comparisons, given $\omega_j = .29$.[20] The estimates for $\hat{\tau}$ are negative and statistically significant, i.e. people living in region (A) experience a sharper decrease in their returns to education compared to individuals in region (B), and the physical capital shock (weakly) dominates the human capital shock. To compare (B) and (C) is to compare people who live in exposed communes to those who experienced no or a negligible shock. The analysis follows the reverse logic from the comparison of (A) and (B), i.e. $j > k, j \in]0,1]$. Figure (C.5) displays the results, given $\omega_j = .63$. The estimates for $\hat{\tau}$ are largely statistically insignificant when eliminating the most exposed regions. The significant estimates for large ω_k recover the comparison of (A) vs. (B).[21]

The comparisons confirm the theoretical findings. (A) vs. (B): the simultaneous decrease in all factors yields decreasing returns, i.e. $dR > 0$. The shock to equipment capital dominates the shock to human capital. $dR > 0$ requires $\sigma_\delta > \sigma_\gamma$ to hold and hence confirms findings by Duffy et al. (2004), Duffy and Papageorgiou (2000), and Behrman (1972) pertaining the capital-skill complementarity in poor settings. (B) vs. (C): the shock to structures capital appears not to affect the returns to education whereas a shock to equipment and human capital affects the returns.

4.7. Robustness checks

Between 2001 and 2013 the population of Haiti has experienced several idiosyncratic and covariate shocks with varying impacts on physical and human capital. Table (C.4) in the appendix shows that Haiti was struck by floods, storms, and an epidemic in this time span which may have induced the observed effects. In addition, Haiti experienced migration, e.g. from rural to urban areas or in response to the disaster. Haiti also experienced a short episode of political unrest. Beyond shocks and migration, this section also considers an alternative approach for defining the control groups in the DID setting, assesses an

[20]This follows the extended specification, c.f. figure (4.3) and table (4.2) column (1b).
[21]Appendix (C.3.3) extends the comparisons to all possible combinations of ω_j and ω_k.

omitted variable bias, and explores alternative functional forms to measure the relevant relationships. The substance of the results is unchanged, for all robustness checks.

4.7.1. Exposure to other shocks and migration

The section discusses other shocks, migration patterns, results of the robustness checks.

4.7.1.1. Other natural disasters

The 2010 earthquake was by far the most severe shock striking Haiti in terms of total number of people deceased/injured/affected as well as in terms of total economic/monetary damage. The earthquake is responsible for 95% of the deaths and over 90% of the economic damages in the time between 1980 and 2010 (World Bank, 2014a; Guha-Sapir et al., 2015). Other major shocks before the 2010 earthquake were the 2004 Caribbean flood and the Cyclone Jeanne.[22] Unfortunately, the impact of these shocks cannot be tested with the data at hand. However, Échevin (2014) investigates the relative and absolute impact of idiosyncratic and covariate shocks in Haiti using data from 2007.[23] The surveyed population perceived the disease, casualty, or death of a household member as the shocks with the worst impact on income and/or assets. These self-declared perceptions are corroborated in further analysis. He concludes that individual level shocks yield a greater (negative) impact on assets and/or income than covariate shocks such as floods and storms. Further, Échevin reports a low correlation between the exposure to covariate shocks and experiencing idiosyncratic health shocks which in turn have the strongest (negative) impact on income and assets. In addition to the relatively low importance of covariate shocks in 2007, any negative effect emanating specifically from these shocks may have worn off by 2013 or be overshadowed by the impact of the 2010 earthquake. Otherwise, the effects would be expected to smoothen the observed treatment effect distribution. The discussed events occurred in areas which were affected relatively weakly by the 2010 earthquake, i.e. in areas which are in the control group for most specifications. Hence, a decrease in income would imply decreasing returns relative to the other areas, all else equal, leaving the observed treatment effects as lower bound estimates.

The most notable shocks occurring between the 2010 earthquake and the data collection are Hurricane Sandy in 2012 and the 2010 Cholera outbreak which carried on ever since at varying degrees. Both shocks are slow-onset events, in contrast to earthquakes which are rapid-onset events. People may be able to adjust and prepare for possible exposure, e.g.

[22] The Caribbean flood affected mainly the South-East border with the Dominican Republic, in particular Fonds-Verrettes and Mapou, Cyclone Jeanne mainly the North-East coastal area, especially Gonaïves.

[23] The vulnerability and food security survey was conducted in October and November 2007, covering about 3,000 households living in 228 rural communities, cf. Échevin (2014).

by taking extra sanitary precautions in the case of an epidemic, or by reinforcing/securing physical assets and staying out of the hurricane's harm's way. The ECVMAS waves contain household level information concerning the exposure to both events.[24]

Figure (C.8) in the appendix shows that Hurricane Sandy struck Haiti mainly in the South and the South-West as well as in the most Northern part of the country. The storm was the greatest natural disaster in terms of damage in the of year 2012 (Zorn, 2018). Figure (C.9) in the appendix shows that the Cholera outbreak in Haiti was most intense in the departments Arbonite and Centre.[25]

Exposure to Hurricane Sandy and exposure to the Cholera epidemic hardly correlate which each other (Pearson's $\rho \approx -0.06$; level of significance 0.56). Similarly, earthquake exposure tends to correlate negatively with exposure to Hurricane Sandy (Pearson's $\rho = -0.07$; 0.48) and the Cholera epidemic (Pearson's $\rho = -0.19$; 0.05). The expected bias from Hurricane Sandy is to smooth the observed treatment effect by lowering income for exposed individuals since the hurricane affected regions which were exposed relatively less by the earthquake. Following theory, the Cholera exposure (a human capital shock) may strengthen the observed treatment effect.[26] However, the epidemic was still ongoing at the time of the ECVMAS II survey, i.e. incomes may be affected rather negatively since the active epidemic may hamper market activity and induce people to stay home for reasons of safety or health. This would imply again a smoothing effect.

4.7.1.2. Domestic migration

Two sources of migration may drive the observed treatment effect: (1) migration in anticipation of the earthquake and (2) migration in response to the earthquake. Regarding (1): migration in anticipation of the earthquake may be a concern if individuals non-randomly select into living in (perceived-to-be) safer areas. If this unobservable self-selection would correlate with outcomes such as/related to income, the measure of disaster exposure may capture unobserved differences in wages rather than the effects of the earthquake. However, little had been done to prepare for a possible earthquake and the major cities are all located close by tectonic fault lines, i.e. all of Haiti is at moderate to high risk of experiencing an earthquake (CHRR and CIESIN, 2005). Figure (C.10) in the appendix shows that in response to the earthquake people hardly undertook moving as adjust-

[24] The relevant Cholera data are given by ECVMAS I. The information carries over to ECVMAS II thanks to the panel dimension between the two surveys. The Cholera infection and fatality rates remained relatively stable across the time for the two surveys (UNICEF Haiti, 2013).

[25] The epidemic emerged in October 2010 from a camp of the United Nations Stabilization Mission in Haiti, upstream of the Arbonite river near the city Mirebalais in the Arbonite department, and from there throughout the country (Smallman-Raynor et al., 2015).

[26] This assumes that the high and low skilled individuals suffered exposure in equal shares.

ment mechanism, even shortly after the earthquake. Hence, it is unlikely that individuals undertook pre-earthquake migration by moving to safer areas.[27]

Regarding (2): migration in response to the shock may be a concern since table (C.5) panel A shows that those who migrate tend to have higher levels of education.[28] This may bias the estimated results, possibly increasing them in receiving areas and lowering them in originating areas.[29] Migration in response to the earthquake was mainly of short duration and happened largely in close spatial proximity if leaving the commune (Hauge et al., 2015; Herrera et al., 2014; Schwartz et al., 2011; Kolbe et al., 2010), though at the time of the ECVMAS II, between 147,000 and 172,000 people were still displaced in different parts of the country (IOM, 2014). However, the distribution was uneven. Many displaced individuals were located in Port-au-Prince and adjacent communes (Smallman-Raynor et al., 2015; Bengtsson et al., 2011). Figure (C.11) in the appendix presents the share of the population who lived in another commune during the 2010 earthquake. It shows that the number of migrated/displaced individuals due to the earthquake represent a low share in relative terms vis-à-vis the overall local population in the communes.[30]

Another migration pattern stems from individuals who do not live in their commune of birth without additional information on why or when they moved. The share of "internal migrants" is high, at about 30% of the 2013 sample. The metropolitan area is the principal recipient. However, the share of internal migrants remained relatively steady between 2001 and 2012 (World Bank, 2014a). Similar to earthquake-related migration, table (C.5) panel B shows that those who do not live in their commune of birth tend to have higher educational attainment.

4.7.1.3. Political change

Haiti experienced a brief period of political violence in 2004 related to the coup d'état ousting then-President Aristide. It was mainly prevalent in Port-au-Prince and north of it. As such, the impact may have a smoothing or strengthening character on the treatment distribution. However, the conflict was on a low scale, both in comparison to the natural disasters as well as per ranking as level 1, i.e. "weak civil violent conflict"

[27] Similar patterns of not adjusting ex ante have been observed in other settings, e.g. for earthquakes in Indonesia (Kirchberger, 2017) and for hurricanes in Mexico (Rodríguez-Oreggia, 2013).

[28] Displaced women, men, girls, and boys were also exposed to violence and rape (Hauge, 2018; Duramy, 2012; Amnesty International, 2011; Kolbe et al., 2010), which affects labor market outcomes.

[29] This holds if the individuals who migrate would command at least the same income after the displacement. This may be questioned since the migrants can either move to rural areas which are characterized by low incomes and low demand for higher education, or migrants move to urban areas with labor markets which have limited capacity to absorb the post-earthquake migration influx. If this assumption does not hold, the bias would smoothen the observed effect.

[30] Most post-earthquake migrants originate from the communes very close to the epicenter, indeed suggesting that the migration was triggered by the earthquake.

(Center for Systemic Peace, 2017). These magnitude scores are "*a scaled indicator of the destructive impact of the violent episode on the directly-affected society or societies on a scale of 1 (smallest) to 10 (greatest). Magnitude scores reflect multiple factors including state capabilities, interactive intensity (means and goals), area and scope of death and destruction, population displacement, and episode duration*" (Sundberg and Melander, 2013; Croicu and Sundberg, 2017). Given the low intensity and therewith related assumed low relative impact, this pre-earthquake event is not investigated further.

Lastly, in 2009 the Haitian government changed its laws on minimum wage, lifting it from 70 to 200 Haitian Gourdes per day (Herrera et al., 2014, annex B). The policy shift may bias the estimates. For example, if the minimum wage recipients were concentrated in the earthquake affected region, the policy change may be the driver of the observed change in returns to education as an increase in the minimum wage flattens the returns to education curve, all else equal. Unfortunately, the data do not allow for a dedicated test since the ECVH and ECVMAS II only report annual income. However, the great majority of Haitians hold an informal job, severely limiting the impact such labor market policy has (Herrera et al., 2014), and as such limiting any bias from the reform.

4.7.1.4. Results considering exposure to other shocks and domestic migration

To assess the potential impact of the shocks as well as migration, three additional specifications are being employed. (A) A matrix C of confounding shock indicators s is being defined and included as control variables in the Z matrix of equation 4.1. For the covariate shocks of the Cholera epidemic and Hurricane Sandy, C contains two elements: (i) a dummy reflecting whether the individual's household indicated to have been exposed to shock s and (ii) the percentage of the population exposed to the shock in a given commune in 2013. Hence, (i) captures individual level effects and (ii) regional/macro level effects. In addition, C contains a dummy indicating whether the individual lives in a temporary shelter ("camp") which is related to migration in response to the earthquake. The shelter may be based in the same commune where the person lived before the earthquake.[31] (B) repeats (A) under the inclusion of indicators for individuals who migrated to another commune following the earthquakes. Again, with (i) an individual dummy and (ii) commune level averages. Finally, (C) is like (B) but also focuses on all individuals who were born in a different commune than the one they are living in now.

[31] A potential drawback of the approach emanates from the fact that epidemics and hurricanes are slow-onset events. Hence the (degree of) impact may be a function of income. Therefore, by addressing the potential omitted variable bias a new bias might be introduced by including endogenous control variables. However, the shock exposures correlate only weakly/not at all with exposure to the earthquake. Hence, this estimation approach is taken as the lesser evil.

Table (C.6) in appendix (C.4.2) presents the results. Columns (Aa), (Ba), and (Ca) employ education as years of schooling and extend the original model from table (4.2) column (2b), using above specifications A, B, and C, respectively. The results carry over to columns (Aa), (Ba), and (Ca). Only exposure to Hurricane Sandy yields a statistically significant (negative) effect on income. Other shock or migration indicators do not affect income. Figure (C.12) repeats figure (4.3) for these samples, confirming the robustness of the results. Concerning heterogeneity across the attainment distribution, table (C.6) columns (Ab), (Bb), and (Cb) presents the results for education in levels. Again, only exposure to Hurricane Sandy yields a statistically significant (negative) effect. The original results hold when including the controls for Hurricane Sandy, the Cholera epidemic, and living in a camp. When controlling for individuals who migrated after the earthquake or since birth in columns (Bb) and (Cb), the previously observed change in the returns to tertiary education are rendered (borderline) insignificant.

The results from table (C.6) indicate three things. First, the previously observed change in the returns is at least partly driven by domestic migration. For individuals who moved since birth or in response to the earthquake the returns to education change stronger and for those who lived their entire life in the same commune the value of education changes less, and the returns to tertiary education possibly not at all. This could be due to the fact that "locals" (people who live in their commune of birth) can rely on more solid/better established networks in times of a crisis whereas those who came later do not enjoy the same local backing. All other coefficients are essentially unchanged, e.g. education has a similar importance for individuals irrespectively of their migration background. It is just in response to the shock that the returns change.

Second, the results from table (C.6) suggest that migration may not equal migration. Shock-related or ad-hoc forced migration has different implications than (planned) long term migration. It can be expected that both locals and non-locals alike are exposed to post-earthquake migration, explaining the unchanged results. This is confirmed by employing an alternative approach, which excludes all individuals who migrated in response to the earthquake and since birth. The disadvantage of this approach is a significant reduction of the sample size. Notwithstanding this limitation, when excluding individuals who migrated in response to the earthquake, the original model and column (Aa) carry fully through. However when excluding internal migrants, also the change in the level of primary education is rendered insignificant which bolsters the finding that internal migrants appear to (partially) drive the main results. The results are not affected by including or excluding the commune averages of migration.[32]

The third implication from table (C.6) is that years of schooling and degree attainment ultimately measure different things. The value of having been longer in school, regardless

[32]Results omitted for brevity but available upon request.

of degree attainment, decreases for all people after the shock. This is in contrast to real degrees whose value appears to decrease mainly for persons who migrated since birth. This implies an additional value for a more efficient schooling system which succeeds in providing students with degrees, particularly in a society with high internal migration and relatively high risk of being exposed to a negative covariate shock.

The results give confidence that the non-testable shocks such as the 2004 flood and cyclone do not affect the derived conclusions. Both floods and storms are mainly physical capital shocks, as was Hurricane Sandy. And, also as Hurricane Sandy, these shocks took place in areas which were relatively less exposed to the 2010 earthquake. Hence one would expect lower returns in these areas, if anything, which would oppose the results. Hence, the observed effects will serve as lower bound estimates of the effects.

4.7.1.5. Out-of-country migration

Out-of-country migration may affect the results too.[33] It is on the tendency higher educated individuals who leave the country ("brain drain"). Compared to individuals living in Haiti, a lower share of the Haitian diaspora in the USA (first and second generation) have no/low school attainment, and a higher share have higher educational attainment, e.g. university degrees (Migration Policy Institute, 2014). Unfortunately, with the data at hand the impact of this type of migration is untestable. However, given the attainment pattern among those who leave the country, it may be possible to approximate a test by exploring movements in the coefficients of interest when excluding individuals who attained a tertiary degree. Revisiting the main specifications, the $\hat{\tau}$ estimate for years of schooling turns insignificant, the estimates for levels are virtually unchanged.[34] Hence, in the absence of out-of-country migration, more high skilled individuals would have been affected, such that the observed treatment effect can be expected to have been more prominent. This is suggestive evidence that the observed effects are lower bound estimates of the effects.

4.7.2. Alternative control group

Overlaying figures (4.2) and (C.1) shows that the exposed regions contain densely and less densely populated areas which hints to different local capital structures.[35] Hence, the treatment and control groups may live in structurally different settings. To foster comparability, the sample is split into three groups: individuals living in (i) rural settings

[33]About two thirds of the Haitian diaspora is based in the USA and another quarter in the Dominican Republic and the rest of Latin America (Jadotte, 2012; Migration Policy Institute, 2014).
[34]Results available upon request.
[35]While the individuals who experienced the strongest exposure live in population-scarce rural settings, the exposed regions tend to have a higher degree of urbanization compared to the non-exposed areas.

with mean scaled PGA $\nu \approx .19$ ($SD \approx .24$), (ii) non-metropolitan urban environment with $\nu \approx .13$ ($SD \approx .19$), and (iii) metropolitan settings with $\nu \approx .75$ ($SD \approx .11$).

Table (C.7) revisits the main results for the three subsamples which confirm the main results and theory. The two types of urban areas appear to drive the results. The specifications display significant coefficient estimates which exceed those in the main specification. For rural areas no statistically significant effects can be observed. Rural areas are expected to be equipment capital scarce and great majority of human casualties occurred in the capital region. Hence in rural areas the earthquake will have destroyed predominantly structures capital. Similarly, the significant estimates for the urban specifications reflect the earthquake's negative impact on equipment capital which implies decreasing returns to education. The magnitude of the estimated effect is larger for the third subsample as it amounts to comparing strongly and very strongly affected areas.

These results suggest that the main results are lower bound estimates due to the different effects the earthquake has in different regions of the country. For the urban specifications also the returns to secondary education decrease. This suggests that the lack of statistical significance observed in the full sample is driven by developments in the rural areas.

4.7.3. Omitted variable bias: endogeneity of education

Estimates for the returns to education may suffer from a bias due to omitted unobservable variables, e.g. related to excluding a measure of individual ability.[36] This so-called (innate) ability bias may yield an upward bias of the estimated returns since individuals with higher ability may command higher wages for other, unobserved, reasons which correlate with education. Given the DID structure of equation (4.1), the absolute value of the estimated treatment effect $\hat{\tau}$ is not affected by a bias due to omitted unobservable variables, given that the underlying distribution of the unobservable variables did not change over time. In the prominent case of ability this assumption is supported by the fact that the casualties of the earthquake do not differ significantly by educational attainment. However, the derived percentage change may be affected if the underlying estimates for the returns to education are biased. Table (C.8) in appendix (C.4.4) presents the results from testing the relative importance of omitted unobservable variables following Oster (2017). Selection on unobservables is not a major concern in the application at hand. The conclusions concerning the estimated percentage change prevail.[37]

[36]See Becker (1962), Heckman et al. (2006), and Belzil and Hansen (2007) for discussions.
[37]A similar result was also found in other relatively less affluent settings (Gundersen, 2016).

4.7.4. Alternative specifications

4.7.4.1. Alternative specifications for heterogeneity across intensity and types of shock

Appendix (C.4.5) provides alternative approaches to assess the heterogeneity of the treatment effects. First, using multiple treatment dummies. Equation (4.1) includes one treatment dummy $0 < \omega_{0/1} < 1$. Extending this approach, the specification applied in the appendix yields two treatment dummies $0 < \omega^l_{0/1} < \omega^u_{0/1} < 1$ which vary along the exposure distribution. Figures (C.13) and (C.14) present the results graphically. Second, employing higher order polynomials of the continuous treatment variable. Table (C.9) presents the results. The substance of the conclusions is not affected.

4.7.4.2. Fixed effects

Table (C.10) in appendix (C.4.6) revisits the main results by modifying the specification of the fixed effects in matrix Z following above discussions. The substance of the results is unchanged when including only department fixed effects, replacing department fixed effects with commune fixed effects, or including commune fixed effects, department fixed effects, and year specific department fixed effects.

4.7.4.3. Weights

The estimations apply scaled survey weights on a scale from $[1, 2]$. Appendix (C.4.7), table (C.11) shows results for two alternative approaches: not applying any weights and applying the unscaled weights. The essence of the results is unchanged.

4.7.4.4. Measure of education

The variable for years of education has two problems. First, it understates the actual time students spent in education. Due to data limitations years of education is imputed from the last year a person has completed in school, irrespective of grade repetition. E.g. an individual who states that the last class she completed is sixth grade is coded with six years of schooling (see Adelman et al. (2014) and World Bank (2014a) for a similar approach). However, Haiti has an inherently weak educational system. People are likely to have spent more time in education than the variable indicates and students tend to be too old for their current grade (Adelman et al., 2014, 2017). The second problems stems from the same underlying approximation: years of education is coded as $[0..13, 17]$. The jump from 13 to 17 is due to lack of information to infer the number of years of tertiary studies.

This mapping of the education outcomes yields an upwards bias in the returns vis-à-vis the actual time students spent in school. The observed bias is stronger for individuals who have completed higher levels of education as they tend to have repeated more classes. Hence, the estimate for years of schooling, and as such also the estimated returns to education, is more accurate for lower levels of education.

Two avenues are being pursued to address these concerns. On the one hand, all reported results include estimations for the effects for different levels of education. This measure is more robust and not driven by coding-induced bias. On the other hand, table (C.12) in appendix (C.4.8) revisits the main results from table (4.2), capping years of education at 13 years. This harmonizes the years of education measure across the surveys and induces the same bias across the two waves. The alternative coding does not affect the results.

4.7.4.5. Poverty

Given Haiti's persistently high rates of various levels of poverty (World Bank, 2014a), as alternative outcome is to assess the impact of the earthquake on the returns to education for being in poverty. Various income-bases poverty thresholds are being used in the literature, for example \$1.25, \$1.90, \$2.00, \$2.50, \$4.00, and \$10.00 in purchasing power parity (PPP) indicating degrees of extreme or moderate poverty as well as being vulnerable to poverty (Cruz et al., 2015; World Bank, 2014a). To derive the indicators, the income data is being adjusted for PPP using World Bank (2020).[38] Employing the main specifications with the continuous treatment indicator and defining education as years of schooling or as discontinuous levels of education again yields a negative treatment effect, largely bolstering conclusions.[39]

4.8. Discussion and channels for changes in returns

The changes in the returns to education are driven by post-earthquake labor market dynamics given the new environment with an altered stock of human and physical capital.[40,41] Three avenues are being explored to explain the observed changes. First, whether

[38] The suggested PPP exchange rate is 20 Haitian Gourdes to 1 US\$ in PPP instead of 40:1 (World Bank, 2020). In the absence of data for 2013 prices, the rates for 2011 are used. The nominal exchange rate remained relatively constant at 40:1 between 2011 and 2013.

[39] Results available upon request.

[40] The results are not driven by higher or lower rates human capital accumulation in exposed areas (e.g. due to destroyed or rebuild schools) because (1) the data are limited to individuals in the labor market, (2) the follow up survey took place about three years after the earthquake, not leaving enough time for any such effects to kick in, and (3) school re-construction was slow, in line with overall sluggish reconstruction as also evidenced by data from the Haiti 2012 school census.

[41] Labor market outcomes are not driven by post-disaster aid, which was short-lived, with limited impact, and in parts not used at all (Herrera et al., 2014; Kirsch et al., 2012; Saint-Macary and Zanuso, 2015).

adjustments in the type of labor market affiliation cause the altered returns. In particular if certain education groups drop out of the labor market, stop earning income all together, or if the type of employment drives the effects. Second, in how far the sector of activity matters, i.e. if the observed changes in are driven by changes within or across sectors. Individuals changing sectors could explain the changes in their income and hence returns. Lastly, if the changes relate to individual characteristics.[42,43]

4.8.1. Labor market affiliation

Columns (1) in table (C.13) shows that the observed effects are not caused by individuals reporting no more income. More educated individuals are more likely to have a positive income. This relationship is not affected by earthquake exposure. Similarly, columns (2) show that the effects are not driven by working-status.[44] The share of people working decreased over time though the returns to education concerning being in work do not change in response to the disaster. Following columns (3), this also holds for having in secondary job. Relatively more educated individuals are neither more nor less likely to take up an additional employment in response to the shock.

Following columns (4) in table (C.13), living in areas exposed to the earthquake affects the value of education for being an employee. On average, people with a relatively higher level of education are more likely to be an employee. The share of employees did not change over time, neither overall, nor in shock-affected areas. However, people with primary and secondary school degrees become more likely to be an employee relative to those without education. In contrast, the returns to tertiary education do not change and is hence losing relatively to the other degrees. This indicates two things. First, in Haiti, as in many poor countries, self-employment is related to precarious and low income. Employees command higher income, both across time and the earthquake exposure distribution. Since lower levels of education become relatively more valuable for being an employee, this provides insights into the decreasing returns to higher levels of education. A reason for the relative decline in the returns to tertiary education could be that low- and medium-skill

[42]The analyses concerning sector-specific outcomes omit department specific trends due to data limitations. Including regional trends does not affect the substance of the results with exception of education and health sector specific returns to education. Estimates in these regressions appear volatile and in parts unreasonable, suggesting extreme changes in the returns.

[43]Unfortunately, the DHS surveys were not suitable to supplement the analysis of labor market trends over time. The relevant variables were either incomplete (e.g. making it impossible to match job categories over time), or missing in surveys before/after the earthquake (e.g. employee status or seasonal working patterns), or were highly volatile in five year-on-year patterns (e.g. -50%, +30%, +10%, 15% for the share of individuals working in agriculture or changes exceeding 1,000% for skilled and unskilled manual labor). This may stem from different definitions of job categories rendering inter-temporal comparisons impossible. The data appear applicable to analyze an individual's working status (now/in the last 12 months). The results coincide with the results from the primary data.

[44]"Working" excludes family aids due to a data inconsistency between the ECVH and ECVMAS II.

focused sectors absorb those with primary education. In turn, following the destruction of employment opportunities in the high-skilled sector, only some find employment, and some are driven into self-employment. Second, it underscores the additional value of obtaining at least primary education, given its increasing value and given that the value of years of education is being unchanged.

4.8.2. Sector of activity

Looking at the sector of activity, two aspects can be distinguished. First, the intra-sector returns to education may change following the disaster. Second, individuals may adjust and change the sector of activity, e.g. because of destroyed jobs, which may change income too. The analysis focuses on the sectors (1) agriculture/fishery, (2) construction, (3) industry, (4) trade, (5) transportation, (6) education, (7) health, and (8): other services.[45] The industry and health sectors may be relatively more equipment capital intensive. Agriculture/fishery, construction, trade, transportation, education, and other services may be relatively more structures or human capital intensive. The largest share of individuals works in agriculture/fishery, followed by the trade sector. The third largest sector is industry, though substantially smaller, c.f. table (C.1) in the appendix. Individuals holding tertiary education work in all sectors but with a concentration in education and trade as well as a high relative share in public administration which drops considerably over time.

4.8.2.1. Changing sector-specific monetary returns to education

Table (C.14) presents the results concerning changing intra-sector returns. In the education sector the returns to secondary schooling increased, possibly due to human capital scarcity. In turn, for people working in industry the returns to years of schooling and all levels of attainment decreased. The change is expected given the sector's relative equipment capital intensiveness which is complementarity to human capital. However, the returns in the health sector probably do not change. This may be due to imprecise estimates or a higher relative importance of high skilled human capital.[46]

The returns in the construction sector decreased for tertiary education. The reconstruction efforts may imply an increased demand for relatively low skilled workers and a lower demand for those with higher educational attainment. Also in transportation the returns to education decreased, for primary and tertiary education, whereas for workers in agriculture/fishery and trade no changes were observed.

[45]The analysis omits the public sector due to relatively few observations and hence imprecise estimates.

[46]In some year-sector-attainment cells the number of observations is low: construction and tertiary, education/health and no/primary education. This may affect the precision of the results.

4.8.2.2. Changing returns to education for working in a specific sector

Figure (4.4) shows the heterogeneous development of income across sectors, time, and earthquake exposure. The graph depicts sectorial discrepancies which remain relatively stable over time and disaster exposure, i.e. sectors realizing above average income in 2001 also tended to do so in 2013. Income in the sectors of agriculture/fishery and trade were always below their (regional) averages. Similarly for industry, with the exception of the most affected areas. The social sectors (education, health) displayed above average wages in both years.[47] In transportation income was always above average, with the exception of the medium affected area in 2013. Lastly, people in construction experienced above average wages in 2001 in all regions, but below mean income in all regions in 2013. The entire sector saw on the tendency declining wages over time, relative to the other sectors.[48]

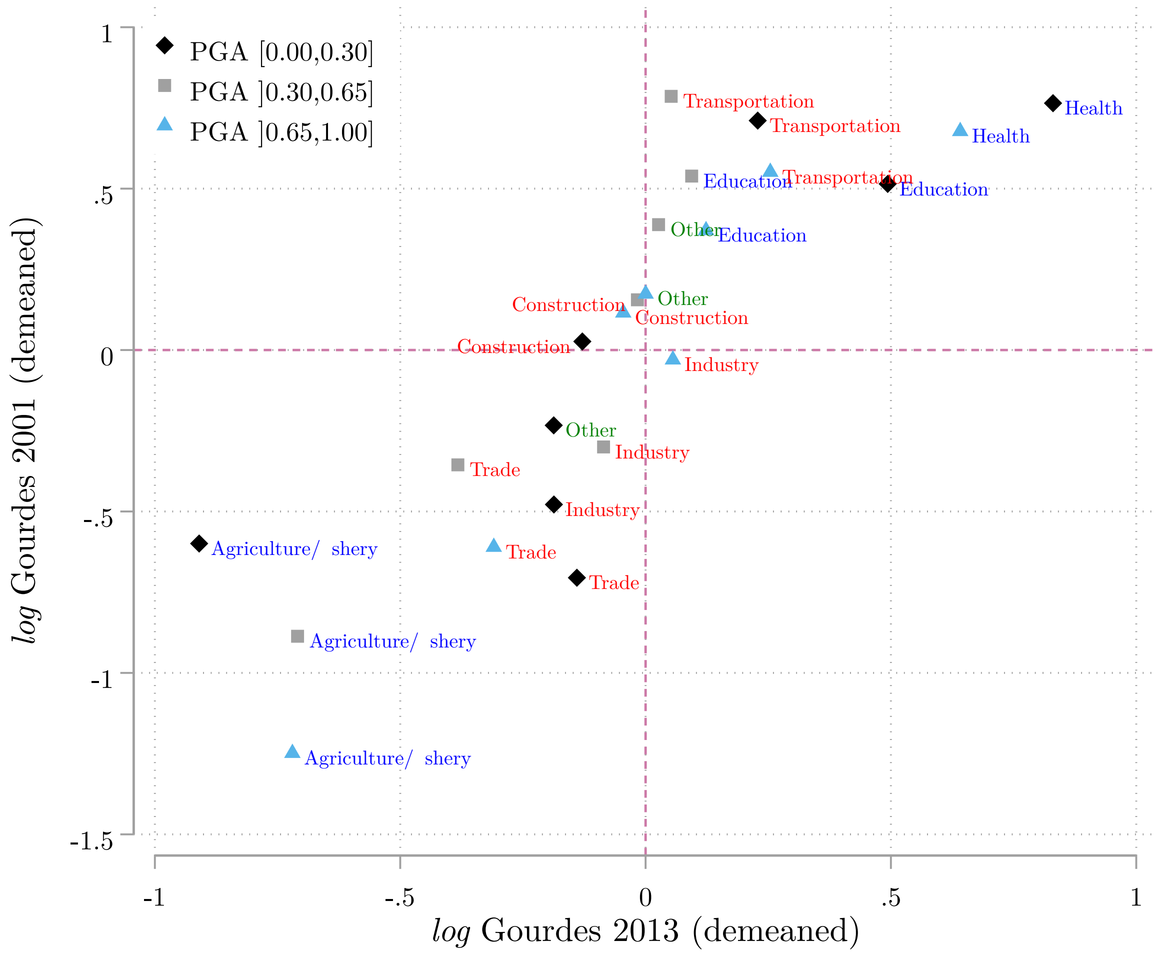

Figure 4.4.: Labor income over time, sectors, and regions, demeaned based on year-earthquake area mean. Labels: Blue: (Higher) education tends to become more valuable. Red: less valuable. Green: neither more nor less valuable.

[47] The health sector realization is omitted for PGA $\in$].3, .65] due to limited number of observations.
[48] World Bank (2014a) found similar sector wage patterns in 2012.

Table (C.15) explores the (changing) returns to education for working in a specific sector, figure (4.4) visualizes the results. Columns (1) in the tables show that the returns to education for working in agriculture/fishery increase. In the exposed regions, fewer people work in agriculture. But, educated individuals with more years of schooling and those with completed primary and secondary degrees are becoming more likely to work in agriculture after the disaster. The effect for tertiary education is (borderline) insignificant. Individuals with no education appear to switch to other, possibly higher paying jobs. One opportunity may be construction as seen following the 2006 earthquakes in Indonesia (Kirchberger, 2017). For educated people, moving into the agriculture sector is not beneficial in terms of income as it displays the lowest average wage of all sectors.

Columns (6) in table (C.15) show that the returns for an additional year of schooling for working in education increased too. Those with secondary education appear to drive the effect, which yields an insight why the returns for those with secondary education appear to be less affected by the earthquake given the high average wages in the sector. For health in columns (7), people with tertiary education become more likely to work in the sector; the effect for years of education is only borderline insignificant. However, individuals with primary education appear to move out of the sector. This may be due to specific skills for jobs which can be expected to be in high demand following a disaster. The observed patterns from the social sectors partly work against the decrease in the returns to education following exposure to the earthquake. It further suggests that the type of locally available jobs is important to explain the observed effects.

In contrast to agriculture/fishery and social sectors, columns (2) in the table show that the more educated become less likely to work in construction. The sector is expanding in affected regions as also seen following other disasters (Kirchberger, 2017; Rodríguez-Oreggia, 2013). It appears that on the tendency those with fewer years of schooling take up these jobs, possibly moving out of lower income positions, e.g. agriculture/fishery. This may contribute to a decrease in the average returns to education. The observed effects in this sector are expected to be lower than those shortly after the earthquake. World Bank (2014a) found a higher relative wage position of the construction sector in 2012, even though 86% of the reconstruction programs were stopped by May 2012 (Herrera et al., 2014) and despite the fact that post-earthquake reconstruction was only limited (UNISDR, 2015; Barone and Mocetti, 2014).

The trade sector also expanded. Columns (4) show that individuals with lower levels of educational attainment are more likely to take up these openings, which offers an opportunity to improve the income situation. Following columns (5), the value of education for working in the transportation sector also declined, for years of schooling and secondary degree attainment. However, those with tertiary education do not appear to move out of the sector which is not beneficial given the declining monetary returns in the area for the highest degree attainment, cf. table (C.14). Columns (3) show a decrease of the returns for working in the declining industry sector, in particular for primary education, i.e. those with completed secondary and tertiary education appear not to move out of the sector. Similarly to the area of transport, also in industry the monetary returns decline which makes staying in the sector not beneficial. The sector of not further specified other services did not experience any changes.

4.8.3. Individual characteristics

Table (C.16) focuses on individual characteristics as an additional dimension concerning the observed changes in returns to education. The measured effects differ across age classes. Columns (1) and (2) show that for recent labor market entrees aged 15-24 the returns do not suffer, in contrast to individuals in the middle or end of their working life. Columns (3) and (4) show that gender matters too. The effects appear to be driven primarily by males. This difference may be due to different adjustment patterns for females and males. In affected areas women appear to adjust in parts from relatively lower paying jobs such as agriculture/fishery into relatively higher paying jobs such as industry. In medium affected regions females expand in the social sector. These positive developments may counterbalance possible negative wage effects in other sectors.

4.9. Conclusion

This paper investigates the impact of the 2010 Haiti earthquake on individual monetary returns to education. The earthquake caused major destruction of human and physical capital. The exogenous shock gives rise to a natural experiment which is analyzed using a Mincerian equation in a DID framework. Exposure to the earthquake decreases the returns on average by 37%. The impact increases in shock intensity and is stronger for people in urban regions. Compared to rural areas, urban regions tend to host high-skill specific physical capital which is complementary to human capital. The destruction of this type of physical capital has a negative effect on the returns to education in the labor market as also suggested by the theoretical model. The paper assesses the situation over two years after the earthquake. The labor market and the Haitian population had time to adjust, local and international assistance programs had time to kick in. The fact that the negative effect is still measurable suggests that the short-term effects have been worse.

The changes in the returns to education are driven by two avenues. On the one hand, the earthquake-induced shake up of the labor market leads to changing returns to education in some sectors, such as industry, which are relatively equipment capital intensive. On the other hand, the earthquake changed the type and quantity of labor demanded in certain sectors, inducing individuals to adjust. People with lower educational attainment appear to be able to improve their income situation, for example by leaving the agricultural/fishery sector and moving into relatively better paying areas of trade or construction. In turn, higher educated people become in parts more likely to work in low paying agriculture/fishery and do not adjust out of non-beneficial settings as faced in transportation. Further, relatively lower educated people adjust into an employee relationship which tends to yield higher income which hence decreases overall returns to education. The effects do not appear to be driven by changes in the labor market affiliation.

The results are heterogeneous across the attainment distribution, the returns to higher degrees depreciate relatively more as individuals with higher education may be more affected by high-skill complementary physical capital destruction. Surprisingly, the value of secondary education does not change in the aggregate and is only negatively affected in the most exposed regions and in urban settings. In rural areas the returns to secondary schooling do not seem to decrease. The share of individuals working in "other" strongly increased over time, which is noteworthy as this economic sector did neither suffer nor gain from the shock. Another reason may be the expansion of secondary educated in the relatively less exposed regions. They may struggle to find suitable work, depressing the returns in the control region too. This may add to noisy estimates which can be due to a high variance of realizations for people with secondary education or data quality.

Individuals may observe a decrease of the monetary return to their education which the individuals might translate into an increase in the uncertainty of future expected returns given that physical capital may be destroyed by future natural disasters.[49] It is possible that this affects household level decisions concerning future investment into education and hence yield an additional dimension of hidden costs of intergenerational shock transmission. The uncertainty may also serve as migration push factor, further incentivizing Haitians to move abroad where the returns to education may be less at risky. While individuals going abroad may send remittances,[50] the net-effect of migration may be particularly negative for most remaining Haitians as well as aggregate welfare (Biavaschi et al., 2020).

This yields an additional incentive to provide individuals with perspectives at the place where they live and bears two policy implications. First, disaster relief may focus on (re)building resilient structures to limit volatility in the returns, induced by changes in the stock of physical capital. Second, public policy should still focus on expanding educational attainment but may have to overcome individual reservations. Despite the

[49] A similar argument can be made by Jensen's inequality for the expected income of educated individuals (w_H) as a function of equipment capital (K_H).

[50] About 45% of Haitians receive remittances. 90% of the remittances originate from Haiti's diaspora in North America (Jadotte, 2012) which received many earthquake related migrants.

decline, the skill premia in Haiti remains high and the social returns may even increase in response to the disaster.

5. Conclusion

The dissertation aims to contribute to the ongoing research at the nexus of individual educational attainment, labor market outcomes, and exposure to natural disasters. The thesis progresses in three steps, considering first aspects concerning access to education, then elements influencing (high) school degree completion, and lastly factors impacting the monetary returns to obtained education. The spatial focal points of the work are relatively less affluent settings due the persistent scarcity of knowledge of these regions, their greater ongoing challenges in the education and labor markets, and their greater exposure to natural disasters and the effects of climate change.

Chapter (2) considers the first step and investigates the impact of a tuition waver program on supply and demand for education in relatively less affluent settings within Haiti. The program was administered as a randomized controlled trial. It provided public funds to nonpublic schools which they could use on a predefined set of items such as teacher salaries or infrastructure investments. In return, schools had to wave tuition. The results of the evaluation display a strong increase in enrollment after lowering the costs of schooling for low income households in Haiti. This hints to a substantial local demand for education which is unmet given prohibitively high tuition fees at the locally available schools. Moreover, the study shows that the program also reduced grade repetition, the share of students who are too old for their level of schooling, and the number of staff such that the student teacher ratio increased only slightly.

Chapter (3) explores the second step by concentrating on educational degree attainment. Using data from Mexico, the chapter investigates how adolescents' completion rates of upper secondary education are affected if the individuals are exposed to different classes of natural disasters, in particular geological disasters (for example earthquakes), climatic disasters which affect only living capital (for example droughts), and climatic disasters which can also affect physical capital such as infrastructure (for example floods or storms). The empirical findings indicate a substantial decrease in the completion rates in response to exposure to all classes of natural disasters. Comparing the different classes, climatic disasters which affect exclusively living capital appear to have the largest negative effect, followed by climatic disasters which can also affect infrastructure. The impact of geological disasters is lower but still economically meaningful. While the effects are not heterogenous across the gender of the individuals, they vary over the locality of where people live in the form of urbanization. Climatic shocks have their strongest effect in urban areas, geological events rather in rural regions. In the given case, the observed drop in attainment is driven by demand side effects such as dropping out of school while not entering the labor market and increasing fertility, especially for young women. A negative parental labor market response to the disasters may influence the outcomes. Supply side effects appear to be driven solely by infrastructure-destructive climatic shocks, via the destruction of infrastructure and a decrease in the municipal share of teachers.

Chapter (4) is on the last element and considers the labor market experience of individuals, in particular their individual monetary returns to education. Following testable predictions of economic theory, this chapter exploits the exogenous variation of destruction of human and physical capital caused by the 2010 Haiti earthquake to disentangle the differential impact on local individual monetary returns to education. The quantitative analysis finds that the returns decreased by an economically significant degree. The changes are driven by changes in the local labor market. The effects are especially strong in areas where the physical capital tends to be relatively more complementary to (high) skilled human capital. In the given case higher educated individuals tend to adjust into low-paying self-employment or jobs in the agriculture culture, the lowest paying sector of the country. The returns are particularly shock-sensitive for urban residents, internal migrants, males, and people over the age of 25.

The chapters in this thesis are ultimately a form of case studies, providing experimental or quasi-experimental evidence from one particular program, disaster, or country. This implies that the results' external validity and general policy implications must remain indicative and elements within larger holistic approaches. Against this backdrop, the results suggest that removing access hurdles such as (financial) costs can be an important element to foster initial and sustained school enrollment in places with particularly high need of educational expansion. Indeed, programs focused on eliminating (parts of) the financial constraints appear as if they may also be relatively cost-effective to increase educational attainment. While not tested here, it may be particularly valuable to combine different effective approaches, for example information on the returns to education and elimination of access hurdles. This may further increase demand while avoiding that the demand remains unmet. Evidently, also the supply side has to adjust if demand grows.

Programs to increase and sustain school enrollment and educational attainment can be particularly important in the advent of adverse covariate shocks such as natural disasters. Of course, how households and individuals respond to such socks is not always merely a choice. However, in situations where people can decide, policy has a role to intervene in responding to such events to (re-)encourage kids to attend education. Interventions may address negative changes in the perceived value of education or the over-discounting of potential future 'bads' such as selecting into being NEET or teenage parenthood. Policy making has to be mindful of the context, such as the type of disaster, implied destruction, and local economic and societal context.

Last but not least, measures to improve resilience have to take local labor market structures into account. If disasters threaten the avenues for the population to translate their (formal) education into making a living, a key incentive for obtaining such education may be jeopardized. A means to lower the shock-sensitivity of the value of education is to reduce the vulnerability of certain factors of production, especially those which are complementary to skilled human capital. Overcoming such structural local vulnerabilities may yield high potential benefits in terms of foregone reconstruction costs, the potential need for interventions to foster continuation of educational attainment or (re-)integration in post-disaster labor markets, and dampening migration push factors.

All three aspects of the thesis are relatively complex in the sense that they affect many aspects of life and or are affected by many factors. As such, more evidence is needed, from other settings as well as from other academic disciplines, in particular other social sciences aside from economics. To further add to the literature I pursue other ongoing research, for example concerning factors of late entry to schooling, how kinship ties affect educational attainment, and how this relationship is affected by external shocks, or how exposure to violent conflict during childhood impacts labor market outcomes later in life, subject to certain political environments. However, these and all similar research projects stand and fall with the quality of data. Notwithstanding the enormous progress which has been made over recent decades to improve data from less affluent countries as well as natural and man-made disaster prevalence, much more can and should be done. Given the increased technical possibilities it is merely a matter of coordination, means, and implementation to make more – and more detailed – data readily available to allow for investigating the topics of this thesis. Several regional or global databases are a substantial step in this direction which future efforts could build up on.

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A. Appendix Chapter 1 Increasing Access by Waiving Tuition: Evidence from Haiti

A.1. Analysis of Tuition Waiver Program Assignment Randomization

The World Bank tuition waiver program expanded to five new departments in the 2008–9 school year: Centre, Grand-Anse, Nord-Est, Nord-Ouest, and Nord. Lists were created in each department, including schools that were interested in participation and met the criteria of being nonpublic and having some level of Ministry of Education recognition. Tuition waiver recipient schools were then randomly selected from each province's list.

In order to confirm that randomization had resulted in groups of schools that were equal in expectation at baseline, we compared selected and nonselected schools in the 2002 national school census, which was the most recent year available. Given the time lapse between 2002 and the initial list development in 2008, the rapid turnover of private schools in Haiti, and the fact that the 2008 school lists did not include census identifiers, not all 2008 schools were present or identifiable in the 2002 census. Percentages of applicant schools identified in the 2002 census are as follows: 61 percent in Centre, 64 percent in Grand-Anse, 59 percent in Nord-Est, 78 percent in Nord-Ouest, and 64 percent in Nord. Overall, 64 percent of selected schools were identified, and 68 percent of nonselected schools were identified.

Next we compared the identified selected and nonselected schools within departments on 19 variables in the 2002 school census, using chi-square tests and t-tests as appropriate. Across the 95 individual statistical tests, just five within-department comparisons showed statistically significant differences (see the bold comparisons in table A.1). The five differences were all on different variables. The total number of statistically significant differences represent approximately 5 percent of all tests conducted, which would be expected. Therefore, the within-department randomization appears to have been correctly done, producing groups of schools that were equivalent in 2002.

Future evaluation work comparing selected and nonselected schools will focus on school size, and it is therefore important to note that there were no statistically significant differences between selected and nonselected schools in the number of children enrolled in first grade, overall or by gender.

Table A.1.: Comparing selected schools to nonselected applicant schools in the 2002 census

Variable	Nonselected Schools (Mean or Percentage)	Selected Schools (Mean or Percentage)	p-Value (χ^2 or t-Test)
		Centre	
Electricity	85%	84%	.887
Toilet	90%	83%	.362
Courtyard	14%	21%	.411
Desks	2.00	2.29	.615
Desks–good condition	1.18	2.21	.057
Chairs	4.53	4.25	.660
Chairs–good condition	3.24	3.60	.580
Kitchen	70%	75%	.659
Library	90%	95%	.369
License	7%	3%	.396
Year founded	1982	1986	.265
Management committee	49%	63%	.142
Parent committee	72%	85%	.109
Any committee	74%	88%	.067
Urban	23%	18%	.541
Private	84%	87%	.676
1st-grade enrollment	48.49	44.10	.592
1st-grade boys	23.65	22.00	.692
1st-grade girls	24.84	22.10	.515
		Grand-Anse	
Electricity	92%	89%	.637
Toilet	97%	96%	.791
Courtyard	33%	52%	.065
Desks	2.81	2.21	.288
Desks–good condition	2.35	1.39	.112
Chairs	5.42	4.53	.228
Chairs–good condition	3.77	3.61	.806
Kitchen	90%	88%	.817
Library	94%	90%	.449
License	6%	4%	.734
Year founded	1982	1988	.022

(*Continued*)

Management committee	64%	49%	.151
Parent committee	75%	70%	.576
Any committee	81%	78%	.766
Urban	31%	30%	.964
Private	97%	99%	.606
1st-grade enrollment	52.61	45.07	.381
1st-grade boys	27.69	24.12	.398
1st-grade girls	24.92	20.95	.380
		Nord-Est	
Electricity	83%	82%	.946
Toilet	74%	88%	.031
Courtyard	14%	14%	.921
Desks	3.90	3.04	.207
Desks–good condition	2.82	2.56	.692
Chairs	5.58	5.43	.793
Chairs–good condition	4.50	4.79	.656
Kitchen	51%	57%	.493
Library	83%	89%	.271
License	5%	4%	.672
Year founded	1986	1988	.241
Management committee	78%	85%	.253
Parent committee	92%	93%	.719
Any committee	92%	95%	.484
Urban	53%	59%	.521
Private	99%	100%	.309
1st-grade enrollment	47.77	45.16	.606
1st-grade boys	24.49	23.12	.618
1st-grade girls	23.27	22.04	.621
		Nord-Ouest	
Electricity	84%	88%	.420
Toilet	87%	91%	.450
Courtyard	10%	14%	.371
Desks	3.05	3.19	.787
Desks–good condition	2.07	2.39	.467
Chairs	6.07	5.45	.191
Chairs–good condition	5.00	4.28	.145
Kitchen	59%	62%	.724
Library	79%	90%	.048
License	5%	10%	.273
Year founded	1982	1983	.407

(*Continued*)

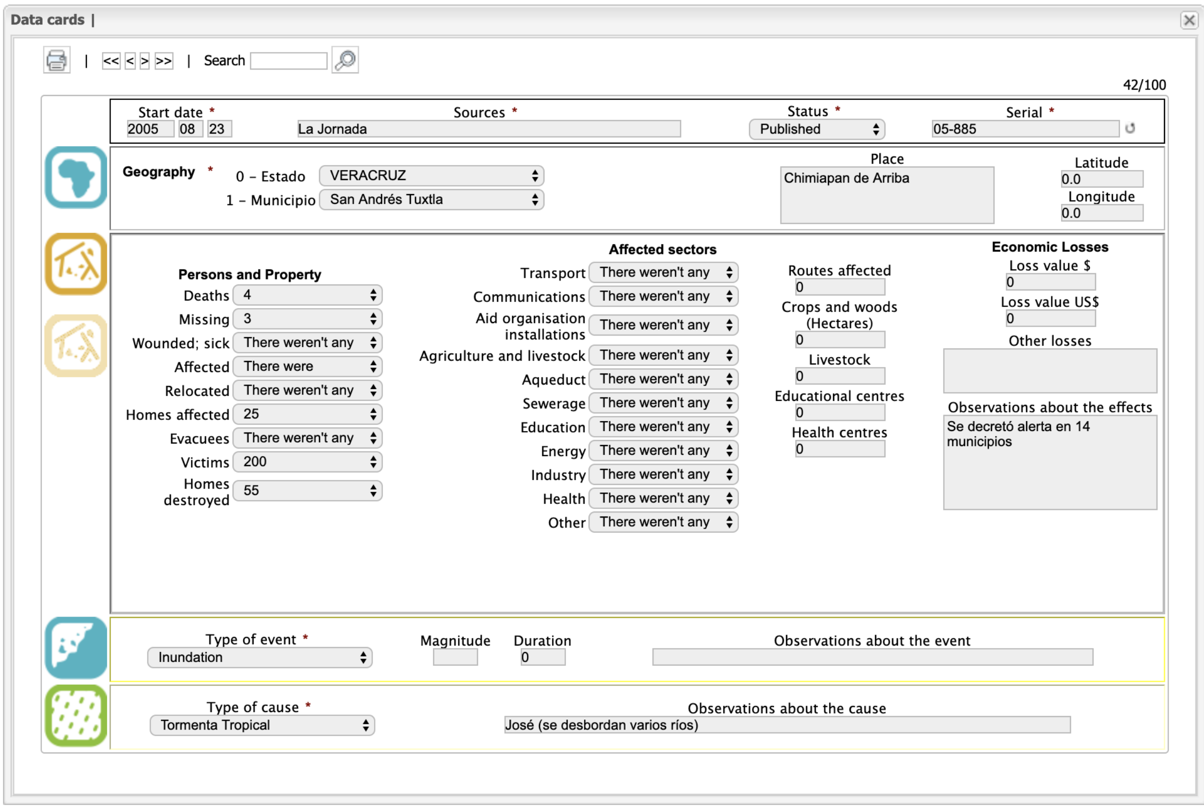
Management committee	87%	89%	.691
Parent committee	83%	88%	.349
Any committee	93%	99%	.046
Urban	22%	30%	.242
Private	100%	100%	n/a
1st-grade enrollment	50.41	48.08	.613
1st-grade boys	25.69	25.11	.809
1st-grade girls	24.72	22.98	.450
		Nord	
Electricity	67%	69%	.767
Toilet	68%	77%	.247
Courtyard	19%	38%	.013
Desks	4.40	3.61	.230
Desks–good condition	3.34	2.90	.498
Chairs	6.19	5.89	.636
Chairs–good condition	5.40	4.86	.438
Kitchen	51%	49%	.855
Library	85%	87%	.709
License	10%	4%	.163
Year founded	1982	1983	.726
Management committee	70%	75%	.552
Parent committee	69%	77%	.326
Any committee	81%	88%	.346
Urban	46%	45%	.905
Private	99%	100%	.404
1st-grade enrollment	45.73	51.05	.478
1st-grade boys	24.62	25.96	.753
1st-grade girls	21.11	25.09	.276

B. Appendix Chapter 2 Natural disasters and educational attainment

B.1. Data

B.1.1. Data sources

Figure (B.1) shows an example data card from the DesInventar data, available online (DesInventar, 2019a). The information on the card is not complete. For example, while the card indicates that 80 homes where affected or destroyed, the card also reports zero economic losses.


Data cards |
<< < > >> | Search
42/100
Start date * 2005 08 23 | Sources * La Jornada | Status * Published | Serial * 05-885
Geography * 0 – Estado VERACRUZ
1 – Municipio San Andrés Tuxtla
Place Chimiapan de Arriba
Latitude 0.0
Longitude 0.0

Persons and Property
Deaths 4
Missing 3
Wounded; sick There weren't any
Affected There were
Relocated There weren't any
Homes affected 25
Evacuees There weren't any
Victims 200
Homes destroyed 55

Affected sectors
Transport There weren't any
Communications There weren't any
Aid organisation installations There weren't any
Agriculture and livestock There weren't any
Aqueduct There weren't any
Sewerage There weren't any
Education There weren't any
Energy There weren't any
Industry There weren't any
Health There weren't any
Other There weren't any

Routes affected 0
Crops and woods (Hectares) 0
Livestock 0
Educational centres 0
Health centres 0

Economic Losses
Loss value $ 0
Loss value US$ 0
Other losses
Observations about the effects
Se decretó alerta en 14 municipios

Type of event * Inundation | Magnitude | Duration 0 | Observations about the event
Type of cause * Tormenta Tropical | Observations about the cause José (se desbordan varios ríos)

Figure B.1.: Example DesInventar data card, serial 05-885 (DesInventar, 2019a).

B.1.2. Overview of disasters

Figure (B.2) shows the frequencies of impactful disasters, over time, in Mexico based on data from DesInventar (2019a) and EMDAT (Guha-Sapir et al., 2015). The graph shows an upward trend over time, i.e. the number of recorded disasters increased. The graph further distinguishes the by disaster classes. Evidently, for all disaster classes the number of events increased over time. Figure (B.3) presents the spatial distribution of the all disasters, aggregated over time. Darker shaded areas imply a higher prevalence of disasters. Figure (B.4) refines the map, considering only the disaster indicator as applied in the main specification, for the year 2010.

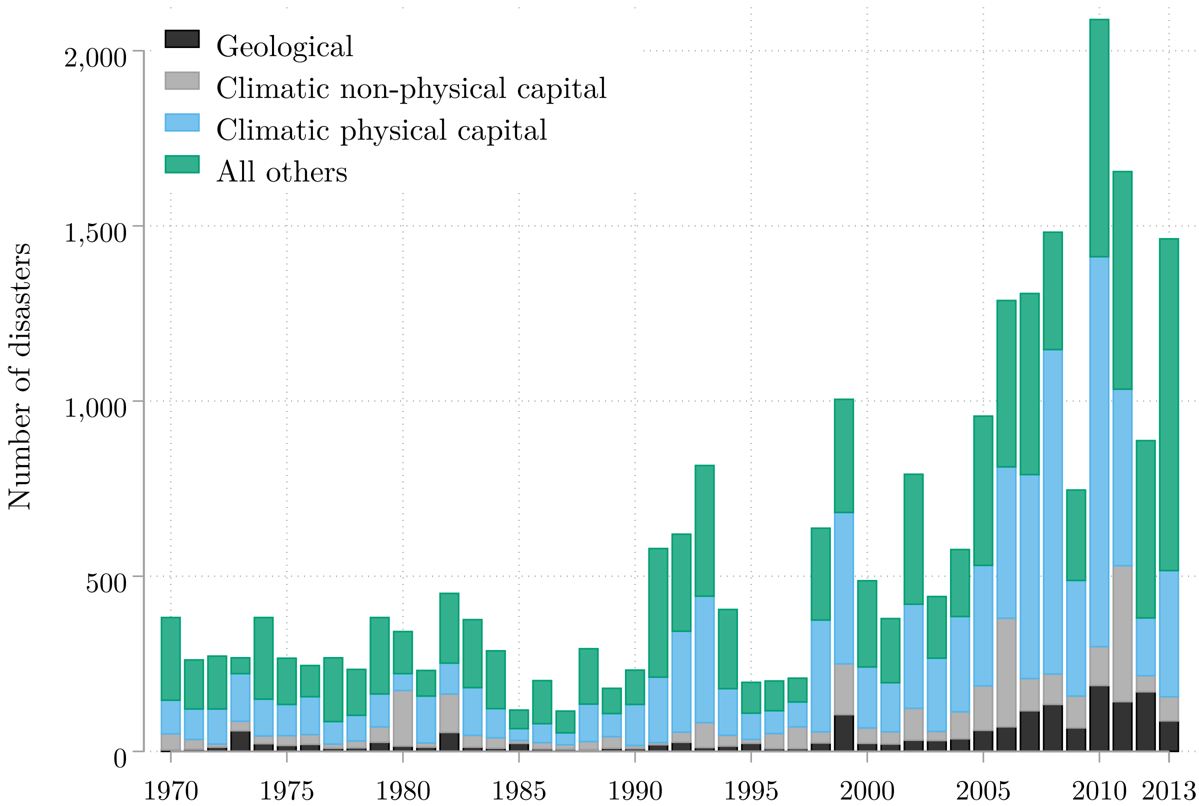


Figure B.2.: Frequencies of impactful disasters, over time, in classes.

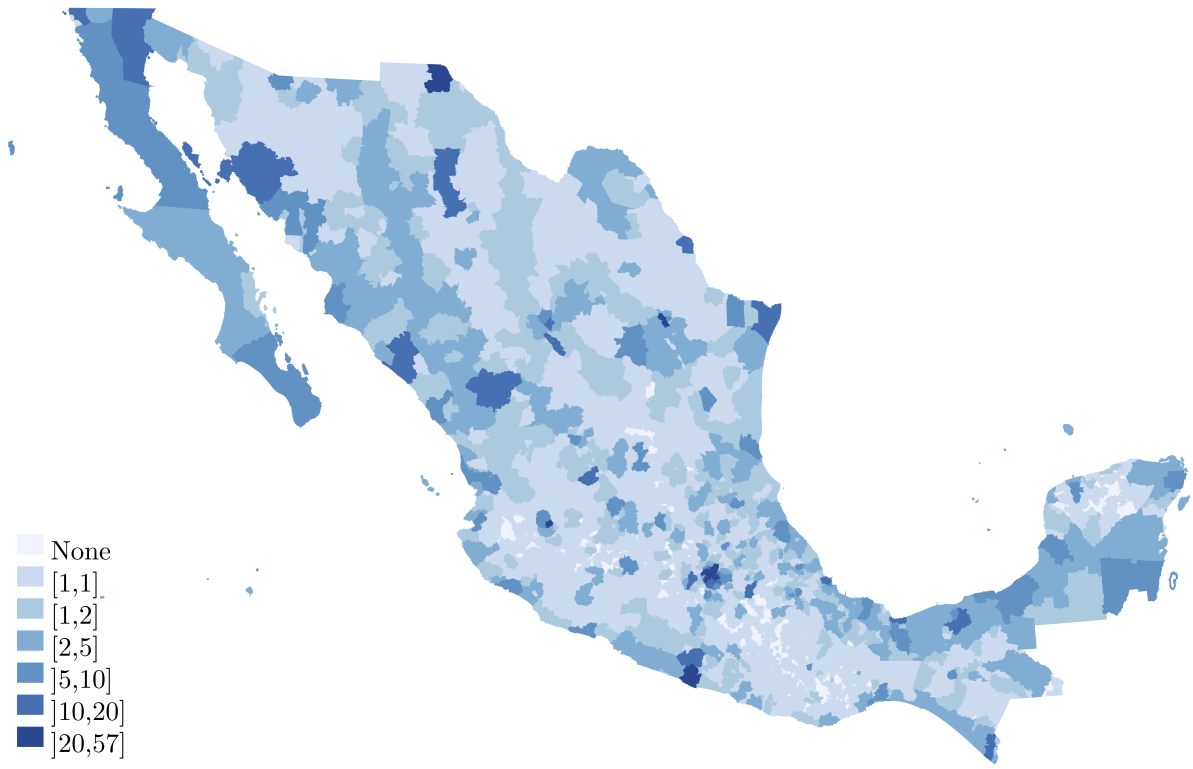


Figure B.3.: Annual average number of impactful disasters in municipalities, 1980-2013.

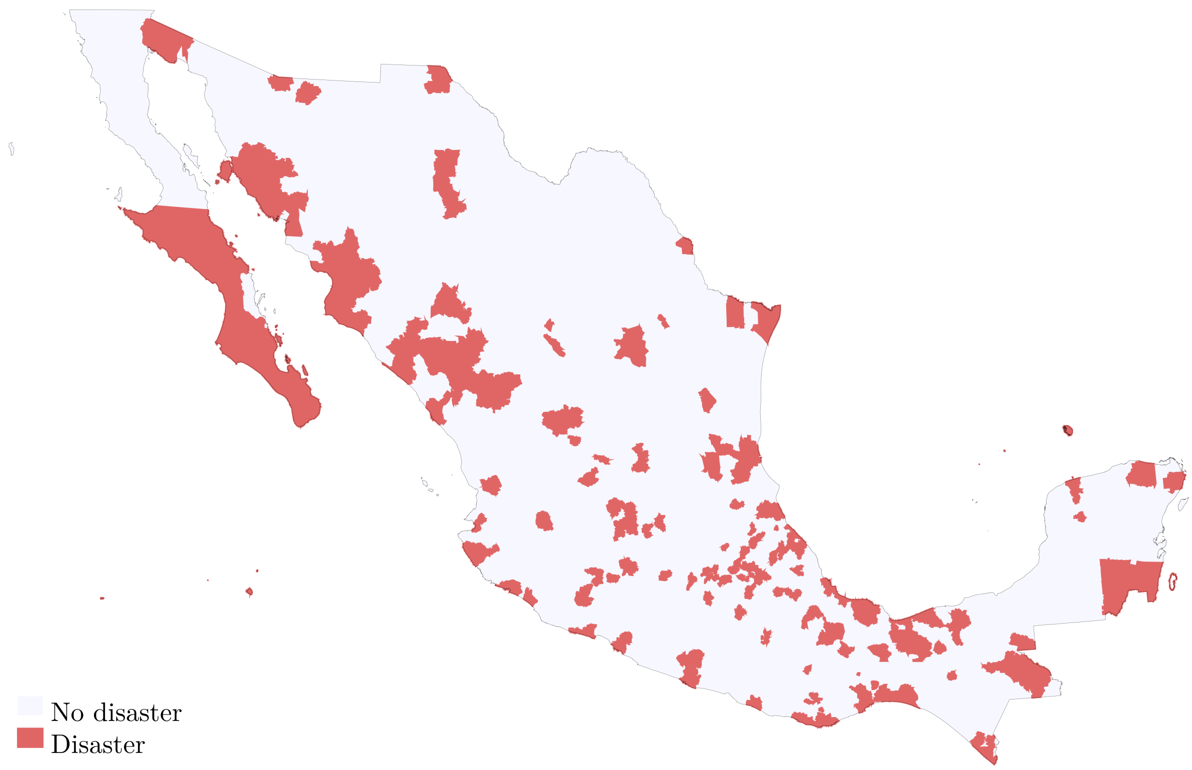


Figure B.4.: Municipalities whose number of recorded impactful disasters in 2005-2010 exceeded 2 SD of the average number of recorded impactful disasters in their relevant state in 2000-2005.

B.1.3. Classification of disasters

The disasters are classified as follows: (i) exogenous/geological events ("GEO"), (ii) climate-change-related events which cannot be expected to destroy infrastructure but may affect human capital, livestock, or harvests ("LIV"), and (iii) climate-change-related events which can be expected to damage infrastructure ("PHY").

Table (B.1) presents an overview of the three disaster classes applied in this study as well as the number of cases for each type of disaster. For example, in the period from 2005-2010 the data yield 443 GEO disasters. 4 of those 443 events were earthquakes.

Table B.1.: Disaster overview 2005-2010

Disaster	GEO	(N)	LIV	(N)	PHY	(N)
Aluvion	✓	(9)	×		×	
Avalanche	✓	(1)	×		×	
Coastline change	✓	(2)	×		×	
Earthquake	✓	(4)	×		×	
Failure	✓	(78)	×		×	
Geotechnical fault	✓	(3)	×		×	
Liquefaction	✓	(1)	×		×	
Slip	✓	(187)	×		×	
Torrential wave	✓	(158)	×		×	
Volcanic activity	✓	(0)	×		×	
Sum		443				
Cold wave	×		✓	(225)	×	
Drought	×		✓	(36)	×	
Frost	×		✓	(358)	×	
Heat wave	×		✓	(108)	×	
Sum				727		
Electrical storm	×		×		✓	(41)
Flood	×		×		✓	(1,990)
Gale	×		×		✓	(192)
Hailstorm	×		×		✓	(132)
Storm	×		×		✓	(282)
Storm surge	×		×		✓	(23)
Tornado	×		×		✓	(4)
Sum						2,664

N indicates the number of observations.

B.1.4. Complete summary statistics

Table B.2.: Complete summary statistics

A: Municipality level means/shares for 17-18-year-olds				
	Year 2000		Year 2010	
	No disaster mean (SD)	Disaster mean (SD)	No disaster mean (SD)	Disaster mean (SD)
% completed upper secondary attainment[a]	7.09	13.65	10.52	16.16
	(5.52)	(6.65)	(5.86)	(6.88)
% completed lower secondary attainment[a]	47.94	64.12	70.78	79.54
	(18.20)	(16.42)	(13.32)	(10.76)
% enrolled in school[a]	25.33	40.28	40.58	51.88
	(12.25)	(13.49)	(12.75)	(11.98)
% NEETs[a]	35.22	25.27	30.09	24.23
	(12.04)	(9.73)	(9.18)	(7.32)
% employed[a]	43.69	41.56	33.02	29.22
	(10.98)	(9.36)	(8.52)	(7.83)
% working and schooling[a]	1.85	2.31	2.25	2.32
	(2.08)	(1.51)	(1.91)	(1.54)
% pursuing housework[a]	0.17	0.16	0.49	0.46
	(0.49)	(0.29)	(0.61)	(0.44)
Hours worked per week[a]	44.09	44.87	41.04	41.82
	(5.35)	(3.52)	(5.06)	(4.23)
Number of own children[a]	0.10	0.09	0.09	0.09
	(0.09)	(0.04)	(0.04)	(0.03)
% living in urban area[a]	46.02	74.02	47.81	74.51
	(33.15)	(28.42)	(33.31)	(27.32)
Age[a]	17.50	17.50	17.50	17.50
	(0.06)	(0.04)	(0.04)	(0.03)
Gender (% female)[a]	48.70	49.09	49.76	50.48
	(6.24)	(3.88)	(4.45)	(3.42)
% state level migrants[a]	2.15	4.89	2.08	3.43
	(2.50)	(4.59)	(2.09)	(2.62)
% municipality level migrants[a]	2.35	3.44	2.16	2.82
	(3.18)	(3.25)	(3.07)	(2.92)
Income aggregate of parents[†,‡,a,b]	3.10	6.80	3.99	7.61
	(3.33)	(4.04)	(2.37)	(3.60)
% of mothers employed[a]	23.78	33.20	29.00	41.93
	(11.81)	(11.26)	(12.50)	(13.19)
% of fathers employed[a]	84.63	88.05	86.14	89.43
	(11.07)	(7.02)	(8.33)	(5.32)
Hours worked per week of mother[a]	37.52	38.41	36.35	38.54
	(5.64)	(3.17)	(4.18)	(2.74)
Hours worked per week of fathers[a]	46.28	49.34	45.72	49.09
	(4.69)	(3.54)	(4.92)	(4.06)

(*Continued*)

B: Municipality level disaster realizations in period 2005-2010

	Year 2000		Year 2010	
	No disaster mean (SD)	Disaster mean (SD)	No disaster mean (SD)	Disaster mean (SD)
% hit by any impactful disaster[c]	0.00	100.00	0.00	100.00
	(0.00)	(0.00)	(0.00)	(0.00)
% hit by GEO disaster[c]	0.00	44.86	0.00	44.86
	(0.00)	(49.76)	(0.00)	(49.76)
% hit by LIV disaster[c]	0.00	56.11	0.00	56.11
	(0.00)	(49.65)	(0.00)	(49.65)
% hit by PHY disaster[c]	0.00	86.85	0.00	86.85
	(0.00)	(33.81)	(0.00)	(33.81)

C: Municipality level means/shares for entire population

	Year 2000		Year 2010	
	No disaster mean (SD)	Disaster mean (SD)	No disaster mean (SD)	Disaster mean (SD)
log per capita revenue[‡,b,d]	1.53	5.19	4.97	6.55
	(3.55)	(2.74)	(2.77)	(1.32)
Population, in 10,000[d]	2.82	34.43	3.14	39.16
	(3.00)	(40.51)	(3.11)	(43.74)
% working in education[a]	1.81	2.82	2.04	2.94
	(1.42)	(1.33)	(1.49)	(1.25)
% working in armed forces[a]	0.09	0.20	0.12	0.21
	(0.26)	(0.35)	(0.35)	(0.36)
% working in agriculture[a]	18.04	8.79	15.43	7.31
	(11.02)	(10.21)	(8.54)	(8.29)
% working in construction[a]	4.15	4.14	4.45	4.35
	(2.52)	(1.57)	(2.59)	(1.45)
% living in urban area[a]	45.91	73.87	48.18	75.16
	(33.12)	(28.43)	(33.08)	(27.08)
0/1 deaths violent conflict[e]	0.93	2.34	0.71	27.76
	(9.62)	(15.13)	(8.41)	(44.80)
% PAN victory 2007-2010[f]	19.23	24.42	19.23	24.42
	(39.44)	(42.98)	(39.44)	(42.98)
% PRI victory 2007-2010[f]	56.55	56.92	56.55	56.92
	(49.60)	(49.54)	(49.60)	(49.54)
Observations	1,082	1,182	1,082	1,182

Each cell shows the mean of the relevant variable, in a given period, in an exposed on non-exposed municipality. Standard deviations (SD) in parentheses. Population weights for the number of 17 to 18-year-old individuals applied. † Monthly income, in 1,000 MEX \$. ‡ In 2015 prices. Source(s) of variables: a: Minnesota Population Center (2019c), b: World Bank (2019), c: DesInventar (2019a), d: Instituto Nacional de Estadística, Geografía e Informática (1990, 2000, 2010a,b, 2019), e: Uppsala Conflict Data Program (2019); Sundberg and Melander (2013); Högbladh (2019), f: Centro de Investigación para el Desarollo A.C. (2011b).

B.2. Additional Results

Table (B.3) presents the results of the tests for heteroskedasticity. Table (B.4) builds up on table (3.2) and shows the complete main results. Tables (B.7) and (B.8) yield the placebo tests for comparing 1990 *versus* 2000 and 1990 *versus* 2000 *versus* 2010, respectively. Table (B.5) assess the results when exploring the intensive margin. Table (B.6) shows the results when limiting the sample to municipalities with an urbanization rate of at least 80% and 95%, respectively.

Table B.3.: Test for heteroskedasticity

	(1990)	(2000)	(2010)
Age	2.902***	9.647***	4.558*
	(0.610)	(1.558)	(2.327)
Gender (% female)	−0.009	0.026*	0.066***
	(0.006)	(0.014)	(0.021)
% state level migrants	−0.003	0.117***	0.016
	(0.016)	(0.035)	(0.044)
% living in urban area	0.028***	0.041***	0.017***
	(0.003)	(0.004)	(0.004)
log per capita revenue‡	0.077*	0.173***	0.022
	(0.040)	(0.046)	(0.045)
Population, in 10,000	0.029**	0.049***	0.037***
	(0.012)	(0.013)	(0.013)
0/1 deaths violent conflict	−1.322	1.168	0.993
	(3.685)	(1.501)	(0.772)
% working in armed forces	0.257*	0.193	0.540*
	(0.144)	(0.312)	(0.305)
% working in agriculture	−0.111***	−0.172***	−0.248***
	(0.010)	(0.013)	(0.018)
% working in construction	−0.081*	−0.170***	−0.304***
	(0.048)	(0.056)	(0.064)
State FE	Yes	Yes	Yes
Observations	2,241	2,248	2,243
R^2	0.29	0.38	0.38
χ^2	360.18	12.89	710.75
p	0.00	0.00	0.00

The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. Standard errors in parentheses.

Table B.4.: Complete main results: upper secondary education, age [17,18]

	(1)	(2)	(3)	(4)
Year 2010 × $Any_{0/1}$	−1.363**			
	(0.535)			
Year 2010 × $GEO_{0/1}$		−0.901*		
		(0.472)		
Year 2010 × $LIV_{0/1}$			−1.613***	
			(0.521)	
Year 2010 × $PHY_{0/1}$				−1.187**
				(0.521)
Year 2010	3.945***	3.882***	4.027***	3.897***
	(0.941)	(0.949)	(0.922)	(0.962)
% living in urban area	0.045**	0.047**	0.045**	0.044**
	(0.020)	(0.021)	(0.020)	(0.020)
Age	9.356***	9.794***	10.125***	8.982***
	(2.912)	(2.919)	(2.896)	(2.902)
Gender (% female)	0.022	0.021	0.015	0.022
	(0.027)	(0.027)	(0.027)	(0.027)
% state level migrants	−0.119**	−0.105*	−0.132**	−0.119**
	(0.059)	(0.055)	(0.059)	(0.058)
% municipality level migrants	−0.081	−0.064	−0.073	−0.073
	(0.060)	(0.062)	(0.060)	(0.061)
0/1 deaths violent conflict	0.867	0.555	0.671	0.763
	(0.632)	(0.611)	(0.598)	(0.626)
log per capita revenue‡	0.211***	0.238***	0.217***	0.225***
	(0.053)	(0.055)	(0.053)	(0.055)
Population, in 10,000	0.101***	0.085***	0.103***	0.094***
	(0.030)	(0.030)	(0.029)	(0.030)
% working in armed forces	−0.139	−0.057	−0.084	−0.092
	(0.583)	(0.567)	(0.584)	(0.585)
% working in agriculture	−0.162***	−0.172***	−0.164***	−0.164***
	(0.030)	(0.030)	(0.030)	(0.030)
% working in construction	−0.288***	−0.254**	−0.272**	−0.303***
	(0.109)	(0.109)	(0.109)	(0.110)
State specific trends	Yes	Yes	Yes	Yes
Observations	4,489	4,489	4,489	4,489
R^2	0.43	0.43	0.44	0.43

The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Table B.5.: Intensive margin

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	-1.757^{***}			
	(0.677)			
Year 2010 × Any_{c}	0.022			
	(0.025)			
Year 2010 × $\text{GEO}_{0/1}$		-1.127^{*}		
		(0.657)		
Year 2010 × GEO_{c}		0.113		
		(0.255)		
Year 2010 × $\text{LIV}_{0/1}$			-2.132^{***}	
			(0.532)	
Year 2010 × LIV_{c}			0.133^{***}	
			(0.048)	
Year 2010 × $\text{PHY}_{0/1}$				-0.890
				(0.591)
Year 2010 × PHY_{c}				-0.041
				(0.045)
Year 2010	3.973^{***}	3.884^{***}	4.154^{***}	3.906^{***}
	(0.933)	(0.950)	(0.901)	(0.964)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	4,489	4,489	4,489	4,489
R^2	0.43	0.43	0.44	0.43

The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. For example, $\text{GEO}_{0/1}$ in row 3 indicates the extensive margin, i.e. d^e from equation (3.2), and GEO_c in row 4 indicates the intensive margin, i.e. d^i from equation (3.2). Similar for Any, LIV, and PHY. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Table B.6.: Urbanization

A: Urbanization rate ≥ 80%

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	−1.680**			
	(0.754)			
Year 2010 × $\text{GEO}_{0/1}$		−0.172		
		(0.729)		
Year 2010 × $\text{LIV}_{0/1}$			−1.958***	
			(0.671)	
Year 2010 × $\text{PHY}_{0/1}$				−1.418*
				(0.792)
Year 2010	−6.725***	−6.362***	−5.772***	−6.481***
	(1.176)	(1.202)	(1.047)	(1.157)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	755	755	755	755
R^2	0.64	0.63	0.65	0.64

B: Urbanization rate ≥ 95%

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	−2.291*			
	(1.166)			
Year 2010 × $\text{GEO}_{0/1}$		−0.830		
		(0.844)		
Year 2010 × $\text{LIV}_{0/1}$			−2.707**	
			(1.296)	
Year 2010 × $\text{PHY}_{0/1}$				−1.989*
				(1.126)
Year 2010	0.138	−1.117	−0.649	−0.181
	(1.450)	(0.995)	(1.096)	(1.474)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	297	297	297	297
R^2	0.76	0.74	0.76	0.75

The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

B.3. Robustness checks

B.3.1. Placebo tests

Table B.7.: Robustness check: placebo test 1990-2000

	(1)	(2)	(3)	(4)
Year 2000 × $\text{Any}_{0/1}$	2.593^{***}			
	(0.461)			
Year 2000 × $\text{GEO}_{0/1}$		1.277^{***}		
		(0.462)		
Year 2000 × $\text{LIV}_{0/1}$			1.875^{***}	
			(0.515)	
Year 2000 × $\text{PHY}_{0/1}$				2.107^{***}
				(0.467)
Year 2000	1.844^{***}	1.947^{***}	2.049^{***}	1.926^{***}
	(0.434)	(0.472)	(0.470)	(0.435)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	4,487	4,487	4,487	4,487
R^2	0.78	0.77	0.78	0.78

The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Table B.8.: Robustness check: placebo test 1990-2000-2010

	(1)	(2)	(3)	(4)
Year 2000 × $\text{Any}_{0/1}$	2.194***			
	(0.421)			
Year 2010 × $\text{Any}_{0/1}$	0.849*			
	(0.497)			
Year 2000 × $\text{GEO}_{0/1}$		1.345***		
		(0.452)		
Year 2010 × $\text{GEO}_{0/1}$		0.257		
		(0.455)		
Year 2000 × $\text{LIV}_{0/1}$			1.639***	
			(0.493)	
Year 2010 × $\text{LIV}_{0/1}$			−0.110	
			(0.478)	
Year 2000 × $\text{PHY}_{0/1}$				1.954***
				(0.395)
Year 2010 × $\text{PHY}_{0/1}$				0.657
				(0.450)
Year 2000	1.461***	1.522***	1.581***	1.511***
	(0.444)	(0.452)	(0.459)	(0.441)
Year 2010	6.750***	6.942***	7.066***	6.801***
	(0.751)	(0.783)	(0.770)	(0.749)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	6,728	6,728	6,728	6,728
R^2	0.76	0.76	0.76	0.76

The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Table B.9.: Robustness check: placebo test older cohorts

Cohort a	$\hat{\tau}_{\text{Any}}$	$\hat{\tau}_{\text{GEO}}$	$\hat{\tau}_{\text{LIV}}$	$\hat{\tau}_{\text{PHY}}$
a = 18	−1.970***	−1.233*	−1.990***	−2.205***
	(0.716)	(0.698)	(0.729)	(0.678)
	[4,487]	[4,487]	[4,487]	[4,487]
a = 22	−0.773	−0.066	−1.049	−0.516
	(0.765)	(0.663)	(0.817)	(0.754)
	[4,488]	[4,488]	[4,488]	[4,488]
a = 24	−0.554	0.672	−1.446*	−0.394
	(0.914)	(0.789)	(0.863)	(0.935)
	[4,491]	[4,491]	[4,491]	[4,491]
a = 25	−0.589	1.712*	−0.433	−0.536
	(0.901)	(0.917)	(0.901)	(0.959)
	[4,489]	[4,489]	[4,489]	[4,489]

The dependent variable in all models is the municipal share of 25-year-olds who completed upper secondary education. Controls in Z: for 25-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses. The number of observations in brackets.

B.3.2. Duration and weights

Table (B.10) repeats the main analysis but adjusts the disaster data with information pertaining the duration of events. Table (B.11) repeats the main analysis without population weights.

Table B.10.: Robustness check: adding duration data

	(1)	(2)	(3)	(4)
Year 2010 × $Any_{0/1}$	-1.320^{**}			
	(0.537)			
Year 2010 × $GEO_{0/1}$		-0.871^{*}		
		(0.479)		
Year 2010 × $LIV_{0/1}$			-1.781^{***}	
			(0.562)	
Year 2010 × $PHY_{0/1}$				-1.124^{**}
				(0.536)
Year 2010	3.904^{***}	3.876^{***}	4.071^{***}	3.884^{***}
	(0.942)	(0.951)	(0.919)	(0.967)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	4,489	4,489	4,489	4,489
R^2	0.43	0.43	0.44	0.43

The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Table B.11.: Robustness check: no population weights

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	-1.073^{**}			
	(0.514)			
Year 2010 × $\text{GEO}_{0/1}$		-1.119^{***}		
		(0.409)		
Year 2010 × $\text{LIV}_{0/1}$			-1.395^{***}	
			(0.482)	
Year 2010 × $\text{PHY}_{0/1}$				-1.002^{**}
				(0.473)
Year 2010	4.853^{***}	5.014^{***}	5.093^{***}	4.846^{***}
	(1.282)	(1.172)	(1.150)	(1.284)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	4,491	4,491	4,491	4,491
R^2	0.28	0.28	0.28	0.28

The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. No population weights applied. Eicker–Huber–White robust standard errors in parentheses.

B.3.3. Validity of disaster indicators

Table (B.12) revisits the specification of the main results but adjusts the threshold used for the disaster indicator. Table (B.13) adds additional conditions concerning past disaster realizations on the estimation sample. Table (B.14) alters the underlying control group applied in the estimations.

Table B.12.: Robustness check: variation of threshold

Adjusted threshold	$\hat{\tau}_{\text{Any}}$	$\hat{\tau}_{\text{GEO}}$	$\hat{\tau}_{\text{LIV}}$	$\hat{\tau}_{\text{PHY}}$
All disasters	−1.064***	−1.220***	−1.382***	−0.807*
	(0.400)	(0.456)	(0.455)	(0.432)
Lagged state mean	−0.942***	−1.220***	−1.382***	−0.875**
	(0.354)	(0.456)	(0.455)	(0.397)
Lagged state mean +1 *SD*	−0.786*	−1.018**	−1.292***	−1.059**
	(0.438)	(0.483)	(0.463)	(0.436)
Main; lagged state mean +2 *SD*	−1.363**	−0.901*	−1.613***	−1.187**
	(0.535)	(0.472)	(0.521)	(0.521)
Lagged state mean +3 *SD*	−2.101***	−1.072**	−1.546***	−1.655***
	(0.583)	(0.481)	(0.552)	(0.586)
Lagged state mean +4 *SD*	−1.480**	−1.599***	−1.907***	−1.898***
	(0.663)	(0.470)	(0.652)	(0.633)

Treatment indicator defined as in main specification but with adjusted thresholds. Each cell represents a separate regression, reporting $\hat{\tau}$ from equation (3.1). For reference, row 4 reproduces the main results from table (3.2). All regressions follow the structure of the extended specifications in table (3.2) and include control variables as well as state specific trends. The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Table B.13.: Robustness check: variation of sample conditions

Sample condition	$\hat{\tau}_{\text{Any}}$	$\hat{\tau}_{\text{GEO}}$	$\hat{\tau}_{\text{LIV}}$	$\hat{\tau}_{\text{PHY}}$
Five years no disasters in municipality	−1.194	−1.245**	−1.514**	−1.538*
	(0.959)	(0.561)	(0.638)	(0.833)
	[2,810]	[4,274]	[4,205]	[3,495]
Ten years no disasters in municipality	−0.872	−1.477**	−1.384*	−2.101**
	(1.336)	(0.598)	(0.706)	(0.967)
	[2,346]	[4,116]	[4,057]	[3,166]

Treatment indicator defined as in main specification but with adjusted sample conditions. Each cell represents a separate regression, reporting $\hat{\tau}$ from equation (3.1). All regressions follow the structure of the extended specifications in table (3.2) and include control variables as well as state specific trends. The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses. The number of observations in brackets.

Table B.14.: Robustness check: limitation of control group

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	−1.358**			
	(0.631)			
Year 2010 × $\text{GEO}_{0/1}$		−1.038**		
		(0.473)		
Year 2010 × $\text{LIV}_{0/1}$			−1.555***	
			(0.504)	
Year 2010 × $\text{PHY}_{0/1}$				−1.154*
				(0.616)
Year 2010	−3.198***	3.956***	0.273	4.642*
	(0.634)	(0.967)	(0.672)	(2.815)
Controls	Yes	Yes	Yes	Yes
State specific trends	No	Yes	Yes	Yes
Observations	2,492	4,402	4,225	3,328
R^2	0.55	0.44	0.45	0.48

Treatment indicator defined as in main specification with threshold of 2 *SD* but under exclusion of weak disasters. The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

B.3.4. Multiple disasters

Table B.15.: Overview multiple disasters 2005-2010

	N	%
No disaster	1,707	75.33
$GEO_{0/1}$	138	6.09
$LIV_{0/1}$	155	6.84
$PHY_{0/1}$	144	6.35
$GEO_{0/1}$ and $LIV_{0/1}$	22	0.97
$GEO_{0/1}$ and $PHY_{0/1}$	43	1.90
$LIV_{0/1}$ and $PHY_{0/1}$	36	1.59
$GEO_{0/1}$ and $LIV_{0/1}$ and $PHY_{0/1}$	21	0.93
Sum	2,266	100.00

Table B.16.: Robustness check: multiple disasters

	(1)
Year 2010 × ($GEO_{0/1}$)	−0.512
	(0.651)
Year 2010 × ($LIV_{0/1}$)	−1.195*
	(0.683)
Year 2010 × ($PHY_{0/1}$)	−0.205
	(0.688)
Year 2010 × ($GEO_{0/1}$ and $LIV_{0/1}$)	−2.420*
	(1.236)
Year 2010 × ($GEO_{0/1}$ and $PHY_{0/1}$)	−1.802**
	(0.856)
Year 2010 × ($LIV_{0/1}$ and $PHY_{0/1}$)	−2.490***
	(0.918)
Year 2010 × ($GEO_{0/1}$ and $LIV_{0/1}$ and $PHY_{0/1}$)	−2.057***
	(0.742)
Year 2010	4.156***
	(0.894)
Controls	Yes
State specific trends	Yes
Observations	4,489
R^2	0.44

The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. The reference category is no disasters at all. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

B.4. Channels

B.4.1. Demand for education

Table (B.17) considers the municipal shares of enrolled 17-18-year-olds as dependent variable, while table (B.18) considers the municipal share of 17-18-year-old that are neither employed, nor in education or training (NEET). Table (B.19) and (B.20) display the labor market response of the parents of the 17-18-year-olds, namely the parental income as well as the employment shares of mothers and fathers, respectively. Table (B.21) examines disaster effect on fertility rate. Table (B.22) goes further and examines gender-specific fertility effects. Table (B.23) analyzes exposure to dual shocks, i.e. natural disasters and increased violence.

Table B.17.: Channel: school enrollment, age [17,18]

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	-3.908^{***}			
	(0.807)			
Year 2010 × $\text{GEO}_{0/1}$		-1.917^{**}		
		(0.783)		
Year 2010 × $\text{LIV}_{0/1}$			-2.487^{***}	
			(0.937)	
Year 2010 × $\text{PHY}_{0/1}$				-3.471^{***}
				(0.818)
Year 2010	15.331^{***}	15.071^{***}	15.232^{***}	15.199^{***}
	(0.885)	(0.816)	(0.775)	(0.860)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	4,489	4,489	4,489	4,489
R^2	0.79	0.78	0.78	0.79

The dependent variable in all models is the municipal share of 17-18-year-olds who enrolled in school. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Table B.18.: Channel: NEET, age [17,18]

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	3.282***			
	(0.619)			
Year 2010 × $\text{GEO}_{0/1}$		1.305**		
		(0.612)		
Year 2010 × $\text{LIV}_{0/1}$			1.267*	
			(0.738)	
Year 2010 × $\text{PHY}_{0/1}$				2.986***
				(0.631)
Year 2010	3.568	3.822	3.779	3.671
	(2.555)	(2.508)	(2.525)	(2.529)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	4,489	4,489	4,489	4,489
R^2	0.36	0.34	0.34	0.36

The dependent variable in all models is the municipal share of 17-18-year-olds who neither in employment nor in education and training (NEET). Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Table B.19.: Channel: income parents (mother + father)

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	−0.579**			
	(0.283)			
Year 2010 × $\text{GEO}_{0/1}$		0.108		
		(0.253)		
Year 2010 × $\text{LIV}_{0/1}$			−0.690**	
			(0.296)	
Year 2010 × $\text{PHY}_{0/1}$				−0.757***
				(0.271)
Year 2010	1.727***	1.642***	1.762***	1.732***
	(0.400)	(0.406)	(0.384)	(0.401)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	4,489	4,489	4,489	4,489
R^2	0.26	0.26	0.26	0.27

The dependent variable in all models is the municipal average aggregate income of the parents of 17-18-year-olds. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Table B.20.: Channel: parental employment

A: Employment mothers

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	0.352			
	(0.657)			
Year 2010 × $\text{GEO}_{0/1}$		0.004		
		(0.638)		
Year 2010 × $\text{LIV}_{0/1}$			-1.408^{*}	
			(0.752)	
Year 2010 × $\text{PHY}_{0/1}$				0.269
				(0.633)
Year 2010	17.295^{***}	17.338^{***}	17.558^{***}	17.311^{***}
	(2.816)	(2.801)	(2.740)	(2.809)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	4,489	4,489	4,489	4,489
R^2	0.58	0.58	0.58	0.58

B: Employment fathers

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	-1.753^{***}			
	(0.468)			
Year 2010 × $\text{GEO}_{0/1}$		-1.428^{***}		
		(0.450)		
Year 2010 × $\text{LIV}_{0/1}$			-1.628^{***}	
			(0.482)	
Year 2010 × $\text{PHY}_{0/1}$				-1.939^{***}
				(0.462)
Year 2010	3.933	3.884	3.969	3.913
	(2.396)	(2.395)	(2.424)	(2.390)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	4,489	4,489	4,489	4,489
R^2	0.30	0.30	0.30	0.30

In panel (A) the dependent variable in all models is the municipal share of employed mothers of 17-18-year-olds. In panel (B) the dependent variable in all models is the municipal share of employed fathers of 17-18-year-olds. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Table B.21.: Channel: number of own children, age [17,18]

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	0.008**			
	(0.004)			
Year 2010 × $\text{GEO}_{0/1}$		0.002		
		(0.004)		
Year 2010 × $\text{LIV}_{0/1}$			0.010**	
			(0.004)	
Year 2010 × $\text{PHY}_{0/1}$				0.008**
				(0.004)
Year 2010	0.020	0.021	0.020	0.020
	(0.019)	(0.018)	(0.018)	(0.018)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	4,489	4,489	4,489	4,489
R^2	0.14	0.14	0.14	0.14

The dependent variable in all models is the municipal average number of own children by 17-18-year-olds. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Table B.22.: Channel: number of own children, gender specific, age [17,18]

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	0.019^{***}			
	(0.006)			
Year 2010 × $\text{GEO}_{0/1}$		0.005		
		(0.006)		
Year 2010 × $\text{LIV}_{0/1}$			0.019^{***}	
			(0.006)	
Year 2010 × $\text{PHY}_{0/1}$				0.015^{**}
				(0.006)
Year 2010 × $\text{Any}_{0/1}$ × Male	-0.017^{***}			
	(0.006)			
Year 2010 × $\text{GEO}_{0/1}$ × Male		−0.006		
		(0.007)		
Year 2010 × $\text{LIV}_{0/1}$ × Male			-0.017^{**}	
			(0.006)	
Year 2010 × $\text{PHY}_{0/1}$ × Male				−0.009
				(0.006)
Year 2010	0.010	0.013	0.010	0.011
	(0.018)	(0.018)	(0.018)	(0.018)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	8,971	8,971	8,971	8,971
R^2	0.09	0.09	0.09	0.09

The dependent variable in all models is the municipal average number of own children by 17-18-year-olds. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Table B.23.: Channel: violent conflict

	All elections				Close elections	
	(1)	(2)	(3)	(4)	(5)	(6)
Year 2010 × $\text{Any}_{0/1}$	−1.300**				1.935	2.393
	(0.522)				(1.608)	(1.632)
Year 2010 × $\text{Any}_{0/1}$ × PAN	0.130				−1.100	
	(1.026)				(2.064)	
Year 2010 × $\text{Any}_{0/1}$ × PRI						−1.984
						(1.786)
Year 2010 × $\text{GEO}_{0/1}$		−1.223**				
		(0.529)				
Year 2010 × $\text{GEO}_{0/1}$ × PAN		1.607				
		(0.982)				
Year 2010 × $\text{LIV}_{0/1}$			−1.494***			
			(0.541)			
Year 2010 × $\text{LIV}_{0/1}$ × PAN			−0.195			
			(0.993)			
Year 2010 × $\text{PHY}_{0/1}$				−1.029**		
				(0.513)		
Year 2010 × $\text{PHY}_{0/1}$ × PAN				−0.284		
				(1.064)		
Year 2010	4.083***	4.217***	4.100***	3.965***	2.634	3.583**
	(0.973)	(1.000)	(0.958)	(0.996)	(1.894)	(1.404)
Year 2010 × PAN	−0.474	−0.969*	−0.284	−0.303	1.104	
	(0.486)	(0.556)	(0.497)	(0.484)	(1.147)	
Year 2010 × PRI						−0.997
						(0.876)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,678	3,678	3,678	3,678	493	769
R^2	0.44	0.44	0.44	0.44	0.49	0.49

The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. No population weights applied. Eicker–Huber–White robust standard errors in parentheses.

B.4.2. Supply for education

Table B.24.: Channel: Physical and human capital destruction

	Physical capital			Human capital			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year 2010 × $\text{Any}_{0/1}$ (Houses)	−1.589***						
	(0.608)						
Year 2010 × $\text{GEO}_{0/1}$ (Houses)		0.739					
		(1.504)					
Year 2010 × $\text{PHY}_{0/1}$ (Houses)			−1.089*				
			(0.656)				
Year 2010 × $\text{Any}_{0/1}$ (Casualties)				−1.510***			
				(0.569)			
Year 2010 × $\text{GEO}_{0/1}$ (Casualties)					−1.355**		
					(0.541)		
Year 2010 × $\text{LIV}_{0/1}$ (Casualties)						−2.383***	
						(0.632)	
Year 2010 × $\text{PHY}_{0/1}$ (Casualties)							−1.145
							(0.710)
Year 2010	4.369***	3.775***	4.166***	3.843***	3.868***	3.933***	4.167***
	(1.008)	(0.992)	(0.997)	(0.966)	(0.941)	(0.965)	(1.000)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,489	4,489	4,489	4,489	4,489	4,489	4,489
R^2	0.43	0.43	0.43	0.43	0.43	0.44	0.43

The dependent variable in all models is the municipal share of 17-18-year-olds who completed upper secondary education. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

Table (B.24) changes the treatment variables and considers physical capital and human capital destruction. Table (B.25) considers the share of teachers in the municipalities.

Table B.25.: Channel: share of teachers in population

	(1)	(2)	(3)	(4)
Year 2010 × $\text{Any}_{0/1}$	−0.131**			
	(0.066)			
Year 2010 × $\text{GEO}_{0/1}$		−0.068		
		(0.060)		
Year 2010 × $\text{LIV}_{0/1}$			−0.037	
			(0.074)	
Year 2010 × $\text{PHY}_{0/1}$				−0.159**
				(0.064)
Year 2010	0.395	0.387	0.385	0.395
	(0.332)	(0.333)	(0.333)	(0.333)
Controls	Yes	Yes	Yes	Yes
State specific trends	Yes	Yes	Yes	Yes
Observations	4,489	4,489	4,489	4,489
R^2	0.12	0.11	0.11	0.12

The dependent variable in all models is the municipal share of teachers. Controls in Z: for 17-18-year-olds: share living in urban areas, average age, gender ratio, and shares who migrated to current municipality in last five years from another state and from another municipality; 0/1 if any deaths due to violent conflict; *log* of per capita revenue of last five years; municipality population size; municipality population shares of people working in armed forces, agriculture, and construction. State specific trends. Population weights applied. Eicker–Huber–White robust standard errors in parentheses.

C. Appendix Chapter 3 Natural Disasters and Returns to Education

C.1. Derivations of theoretical framework

The derivations of the expected impact of the earthquake on the returns to education follow from the aggregate production function in equation (C.1).

$$Y = K_s^{\alpha}\left\{L^{\delta} + \left[K_H^{\gamma} + H^{\gamma}\right]^{\frac{\delta}{\gamma}}\right\}^{\frac{(1-\alpha)}{\delta}} \tag{C.1}$$

First, derive the skill premium $R \equiv \frac{w_H}{w_L}$ in equation (C.2a) and transform into logs in equation (C.2b):

$$R = \frac{H^{\gamma-1}[K_H^{\gamma} + H^{\gamma}]^{\frac{\delta-\gamma}{\gamma}}}{L^{\delta-1}} \tag{C.2a}$$

$$log(R) = (\gamma - 1)log(H) + \frac{\delta - \gamma}{\gamma}log(K_H^{\gamma} + H^{\gamma}) - (\delta - 1)log(L) \tag{C.2b}$$

This facilitates the derivation of the partial derivatives for the isolated shock impacts:

$$\frac{\partial log(R)}{\partial K_s} = 0 \tag{C.3a}$$

$$\frac{\partial log(R)}{\partial K_H} = \frac{(\delta - \gamma)K_H^{\gamma}}{K_H(K_H^{\gamma} + H^{\gamma})} \quad > 0 \text{ given } \delta > \gamma \tag{C.3b}$$

$$\frac{\partial log(R)}{\partial H} = (\gamma - 1)\frac{1}{H} + (\delta - \gamma)\frac{H^{\gamma-1}}{K_H^{\gamma} + H^{\gamma}} \quad < 0 \text{ given } \gamma, \delta < 1 \tag{C.3c}$$

$$\frac{\partial log(R)}{\partial L} = -(\delta - 1)\frac{1}{L} \quad > 0 \text{ given } \delta < 1 \tag{C.3d}$$

The impact of the simultaneous shock to both types of human capital depends on the ratio $\frac{L}{H} \equiv \mu$. Using 2005 data from the Demographic and Health Surveys (DHS) for Haiti for the capital region, and understanding high-skilled workers as those who completed at least secondary education, $\mu \approx 5.6 \gg 1$ which reflects the overall low attainment rates. Using other DHS years or the 2001 living conditions survey in Haiti (Enquête sur les Conditions de Vie en Haïti, ECVH) data does not alter the substance of this finding, i.e. $\mu > 1$. This yields:

$$\frac{\partial log(R)}{\partial H} + \frac{\partial log(R)}{\partial L} = (\gamma - 1)\frac{1}{H} + (\delta - \gamma)\frac{H^{\gamma-1}}{K_H^{\gamma} + H^{\gamma}} - (\delta - 1)\frac{1}{L} \tag{C.4a}$$

$$\frac{\partial log(R)}{\partial H} + \frac{\partial log(R)}{\partial L} \lesseqgtr 0 \tag{C.4b}$$

Multiply with $H, (K_H^{\gamma} + H^{\gamma}) > 0$, substitute $L = \mu H$, and collect terms:

$$\frac{\partial log(R)}{\partial H} + \frac{\partial log(R)}{\partial L} = K_H^{\gamma}\left((\gamma - 1) - \frac{1}{\mu}(\delta - 1)\right) + H_H^{\gamma}\left((\delta - 1) - \frac{1}{\mu}(\delta - 1)\right) \tag{C.4c}$$

$$< 0 \tag{C.4d}$$

The last inequality holds if (i) $\mu > 1$ and $\delta < 1$, such that $(\delta-1)-\frac{1}{\mu}(\delta-1) = (1-\frac{1}{\mu})(\delta-1) < 0$, and (ii) $\delta > \gamma$, such that $0 > (\delta - 1) - \frac{1}{\mu}(\delta - 1) > (\gamma - 1) - \frac{1}{\mu}(\delta - 1)$. Hence a combined ratio-preserving shock to both types of human capital is expected to increase the returns to education given the assumptions. The impact of the earthquake in the most affected area is given by $dR \equiv \frac{\partial log(R)}{\partial K_H} + \frac{\partial log(R)}{\partial H} + \frac{\partial log(R)}{\partial L}$:

$$dR = \frac{(\delta - \gamma)K_H^{\gamma-1}}{K_H^{\gamma} + H^{\gamma}} + (\gamma - 1)\frac{1}{H} + (\delta - \gamma)\frac{H^{\gamma-1}}{K_H^{\gamma} + H^{\gamma}} - (\delta - 1)\frac{1}{L} \tag{C.5a}$$

Add and subtract $\delta\frac{1}{H}$ on the RHS, substitute $L = \mu H$, and collect terms:

$$dR = (\delta - \gamma)\frac{K_H^{\gamma}(\frac{H}{K_H} - 1)}{H(K_H^{\gamma} + H^{\gamma})} + \frac{1}{H}(1 - \frac{1}{\mu})(\delta - 1) \tag{C.5b}$$

$$dR \lesseqgtr 0 \tag{C.5c}$$

The sign of dR is an empirical question. If (i) $\mu > 1$ and $\delta < 1$, the second term $\frac{1}{H}(1 - \frac{1}{\mu})(\delta - 1) < 0$. If (ii) $\delta > \gamma$ and assuming that $H < K_H$, then dR may be positive. Else, $\delta \leq \gamma$, implies $dR < 0$.

The empirical results of this paper yield $dR > 0$, i.e. the value of education decreases in the most affected region. Further, 2005 DHS data give $\mu \gg 1$ and $\delta < 1$ is given. Equations (C.5) shows that for $dR < 0$ to hold it must be $\delta > \gamma$. Also, an isolated simultaneous ratio-preserving shock to both types of human capital implies increasing returns to education. Further, it confirms that the effects of the shock to physical capital dominate the effects of the shock to human capital which can also be shown algebraically. The last inequality in equation (C.6) holds since $\mu > 1 > \delta > \gamma$.

$$\frac{\partial log(R)}{\partial K_H} + \frac{\partial log(R)}{\partial K_s} > \frac{\partial log(R)}{\partial H} + \frac{\partial log(R)}{\partial L} \tag{C.6a}$$

$$\frac{(\delta - \gamma)K_H^{\gamma-1}}{K_H^{\gamma} + H^{\gamma}} + 0 > (\gamma - 1)\frac{1}{H} + (\delta - \gamma)\frac{H^{\gamma-1}}{K_H^{\gamma} + H^{\gamma}} - (\delta - 1)\frac{1}{L} \tag{C.6b}$$

Following simultaneous steps as in equation (C.5) but noting the different sign of $\frac{\partial log(R)}{\partial K_H}$:

$$0 > (\delta - \gamma)\frac{-K_H^{\gamma}(\frac{H}{K_H} + 1)}{H(K_H^{\gamma} + H^{\gamma})} + \frac{1}{H}(1 - \frac{1}{\mu})(\delta - 1) \tag{C.6c}$$

C.2. Data

C.2.1. Treatment indicators

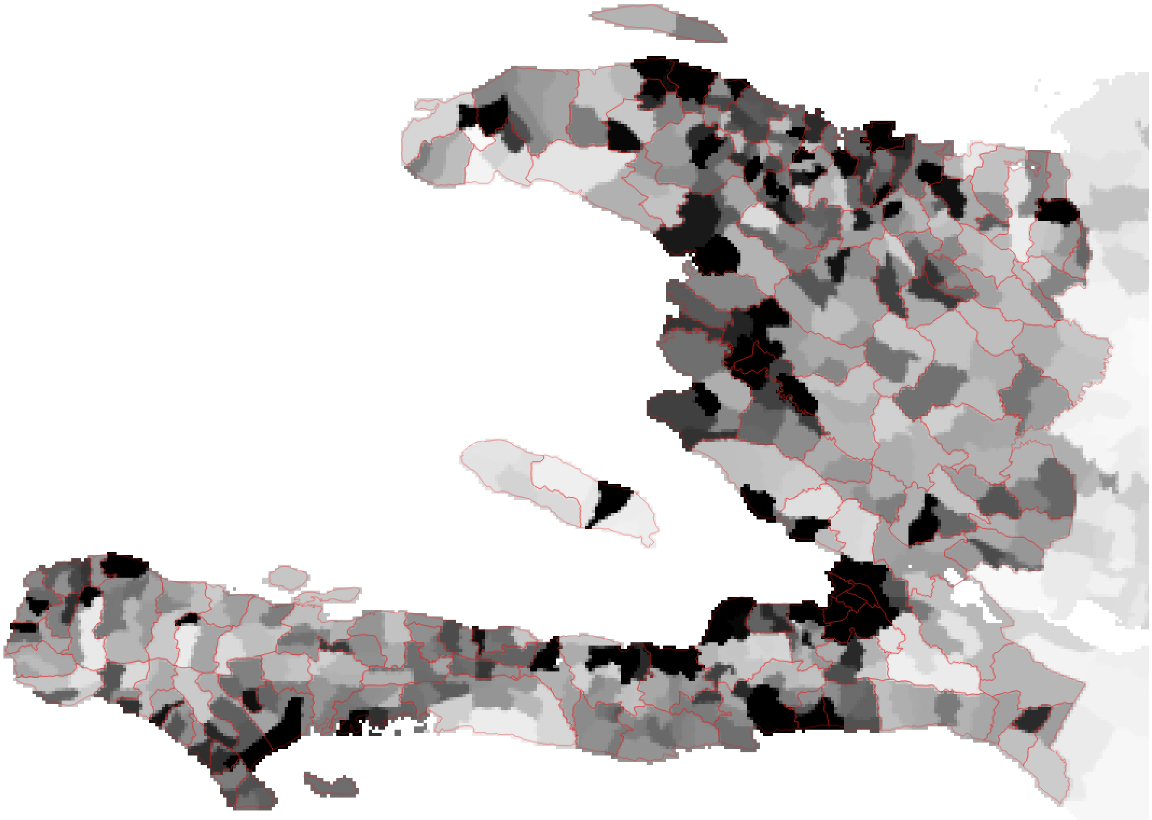


Figure C.1.: Population density in Haiti based on CIESIN (2017a), estimated based on the 2013 population census of Haiti CIESIN (2017b). Darker areas indicate higher population density. Red lines indicate administrative borders of communes of Haiti.

C.2.2. Complete summary statistics

Table C.1.: Complete summary statistics

	Year 2001		Year 2013	
	PGA<.37 mean (*SD*)	PGA≥.37 mean (*SD*)	PGA<.37 mean (*SD*)	PGA≥.37 mean (*SD*)
Labor income¶	6.21	6.84	7.31	8.00
	(1.45)	(1.50)	(1.52)	(1.39)
1=Labor income¶ (%)	51.15	46.78	50.99	44.68
	(49.99)	(49.90)	(50.00)	(49.72)
Years of Education	4.65	7.14	5.42	8.00
	(4.93)	(5.67)	(4.31)	(4.38)
1=No degree attained (%)	68.40	49.13	55.77	30.49
	(46.49)	(50.00)	(49.67)	(46.04)
1=Primary education (%)	16.78	17.79	20.74	19.51
	(37.37)	(38.25)	(40.55)	(39.64)
1=Secondary education (%)	9.68	15.50	19.40	33.57
	(29.56)	(36.19)	(39.55)	(47.23)
1=Tertiary education (%)	5.14	17.58	4.09	16.43
	(22.09)	(38.07)	(19.80)	(37.06)
Potential labor market experience	23.18	19.54	22.23	19.13
	(17.12)	(16.50)	(16.58)	(15.13)
Age	33.80	32.65	33.65	32.89
	(14.46)	(13.60)	(14.21)	(12.87)
1=Male (%)	48.47	46.89	49.50	46.29
	(49.98)	(49.91)	(50.00)	(49.87)
1=Metropolital area (%)	0.73	52.61	0.63	80.88
	(8.49)	(49.94)	(7.93)	(39.33)
1=Other urban area (%)	27.96	6.29	31.52	3.99
	(44.88)	(24.28)	(46.47)	(19.58)
1=Rural area (%)	71.31	41.10	67.85	15.13
	(45.23)	(49.21)	(46.71)	(35.84)
1=Working	77.18	61.60	46.78	41.07
	(41.97)	(48.64)	(49.90)	(49.20)
1=Employee (%)	15.13	29.19	15.46	34.44
	(35.84)	(45.47)	(36.16)	(47.54)
1=Work in Agriculture/fishery (%)	43.66	20.14	25.64	4.31
	(49.60)	(40.11)	(43.67)	(20.31)
1=Work in Construction (%)	1.60	2.71	2.09	3.19
	(12.53)	(16.24)	(14.31)	(17.57)
1=Work in Industry (%)	3.99	6.36	0.72	1.96
	(19.57)	(24.40)	(8.43)	(13.87)
1=Work in Trade (%)	21.74	20.06	15.19	16.74
	(41.25)	(40.05)	(35.89)	(37.34)
1=Work in Transportation (%)	1.02	2.09	1.62	2.31

(*Continued*)

	(10.04)	(14.31)	(12.64)	(15.03)
1=Work in Education (%)	2.92	2.76	1.35	1.79
	(16.83)	(16.39)	(11.53)	(13.25)
1=Work in Health (%)	0.91	0.94	0.36	1.16
	(9.49)	(9.65)	(5.97)	(10.69)
1=Work in Other (%)	4.13	8.02	4.13	9.84
	(19.89)	(27.16)	(19.89)	(29.79)
1=Post-earthquake migration (%)	0.00	0.00	3.87	3.85
	(0.00)	(0.00)	(19.30)	(19.24)
1=Living in camp (%)	0.00	0.00	3.91	11.66
	(0.00)	(0.00)	(19.37)	(32.10)
1=Migration since birth (%)	0.00	0.00	16.33	47.59
	(0.00)	(0.00)	(36.97)	(49.95)
1=Affected by Sandy (%)	0.00	0.00	59.93	21.01
	(0.00)	(0.00)	(49.01)	(40.74)
Ave. affected by Sandy¶¶ (%)	0.00	0.00	58.46	21.34
	(0.00)	(0.00)	(29.48)	(25.29)
1=Affected by Cholera (%)	0.00	0.00	17.49	7.25
	(0.00)	(0.00)	(37.99)	(25.93)
Ave. affected by Cholera¶¶ (%)	0.00	0.00	15.64	7.54
	(0.00)	(0.00)	(13.36)	(3.08)
Observations	12,685	5,423	3,636	2,856

Each cell shows the mean of the relevant variable, in a given year, in an exposed on non-exposed region. Standard deviations (*SD*) in parentheses. ¶ Labor income measured monthly, in 2013 prices. ¶¶ Averages in commune.

C.3. Additional results

C.3.1. Complete main results

Table (C.2) presents the full results from table (4.2). Figure (C.2) mirrors figure (4.3), replacing years of education with levels of educational attainment.

Table C.2.: Complete main results: changing Mincerian returns to education

	Years of education				Levels of education	
	(1a)	(1b)	(2a)	(2b)	(3)	(4)
S (Years) × $\omega_{0/1}$ × T	−0.05**	−0.03**				
	(0.02)	(0.02)				
S (Years) × ω_c × T			−0.07**	−0.05**		
			(0.03)	(0.03)		
Primary × $\omega_{0/1}$ × T					−0.34**	
					(0.16)	
Secondary × $\omega_{0/1}$ × T					−0.24	
					(0.25)	
Tertiary × $\omega_{0/1}$ × T					−0.59**	
					(0.27)	
Primary × ω_c × T						−0.47**
						(0.22)
Secondary × ω_c × T						−0.39
						(0.40)
Tertiary × ω_c × T						−0.76*
						(0.40)
S (Years)	0.11***	0.10***	0.11***	0.10***		
	(0.01)	(0.01)	(0.01)	(0.01)		
1=Primary					0.47***	0.46***
					(0.07)	(0.07)
1=Secondary					1.10***	1.14***
					(0.09)	(0.11)
1=Tertiary					1.54***	1.57***
					(0.15)	(0.17)
1=Year 2013	0.94***	0.56***	0.93***	0.54***	1.11***	0.56***
	(0.08)	(0.13)	(0.09)	(0.15)	(0.24)	(0.16)
$\omega_{0/1}$	0.21	−0.33***			−0.19*	
	(0.14)	(0.12)			(0.10)	
ω_c			0.67***	−0.60**		−0.37*
			(0.22)	(0.25)		(0.22)
S (Years) × T	0.02	−0.00	0.02*	0.00		
	(0.01)	(0.01)	(0.01)	(0.01)		
Primary × T					−0.08	−0.05
					(0.10)	(0.11)
Secondary × T					−0.26**	−0.22
					(0.13)	(0.16)
Tertiary × T					−0.11	−0.09
					(0.22)	(0.24)
S (Years) × $\omega_{0/1}$	0.05***	0.03**				
	(0.02)	(0.01)				
S (Years) × ω_c			0.04*	0.04*		
			(0.02)	(0.02)		
Primary × $\omega_{0/1}$					0.16	
					(0.11)	
Secondary × $\omega_{0/1}$					−0.15	

(*Continued*)

					(0.18)	
Tertiary × $\omega_{0/1}$					0.34	
					(0.27)	
Primary × ω_c						0.20
						(0.18)
Secondary × ω_c						−0.26
						(0.31)
Tertiary × ω_c						0.35
						(0.40)
$\omega_{0/1}$ × T	0.22	0.15			0.19	
	(0.20)	(0.19)			(0.21)	
ω_c × T			0.15	0.34		0.33
			(0.28)	(0.34)		(0.36)
Experience	0.09***	0.08***	0.09***	0.08***	0.07***	0.07***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Experience sq	−0.00***	−0.00***	−0.00***	−0.00***	−0.00***	−0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Controls	No	Yes	No	Yes	Yes	Yes
Department FE	No	Yes	No	Yes	Yes	Yes
Dep. FE × T	No	Yes	No	Yes	Yes	Yes
Observations	11,539	8,989	11,539	8,989	8,910	8,910
R^2	0.25	0.32	0.25	0.32	0.32	0.32
F	105.49	89.75	102.89	98.82	107.04	94.29

The dependent variable in all models is *log* labor income including autoconsumption. Controls in Z: gender (male=1, female=0); employment (employee=1, self-employed=0); urban (urban (metropolitan area)=2, urban (other)=1, rural=0; included as two dummies with rural as comparison group); department FE; department FE × Time. Eicker–Huber–White robust standard errors, clustered at commune level, in parentheses. Significance: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

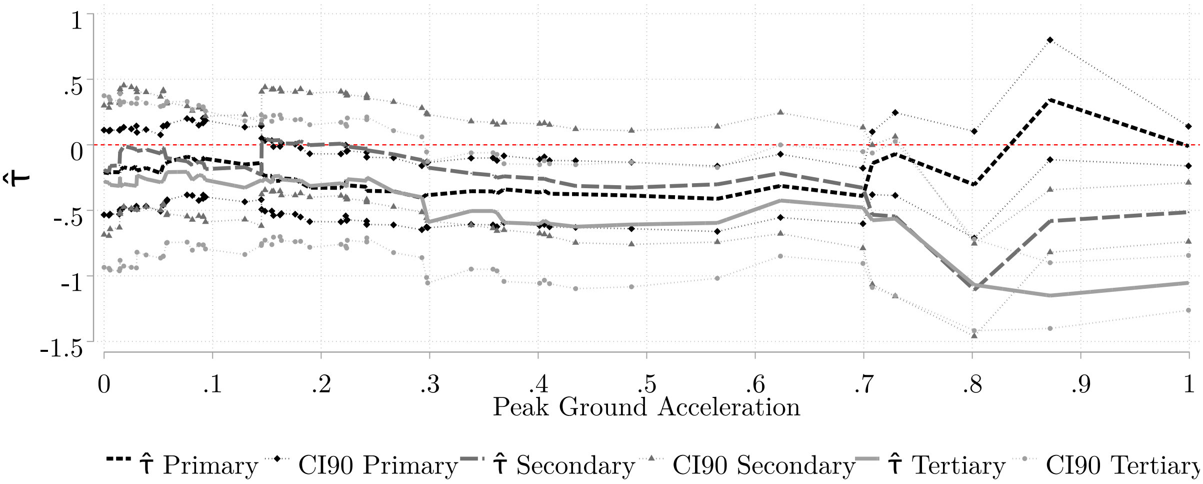


Figure C.2.: Each dot represents the $\hat{\tau}$ coefficient from estimations for all treatment dummies $\omega_{0/1}$, following specification (3) in table (4.2).

C.3.2. Trends

Table C.3.: Changes in perceived income in 2001 vs. 2000

	means		differences	
	C (PGA<.37)	T (PGA≥.37)	C-T	*SE*
Income the same or better	27.95	37.42	−9.474**	4.532
Income worse	72.05	62.58	9.474**	4.532
Observations	119			

Treated if scaled PGA>0.37. Difference = Value in control region C - value in treatment region T.

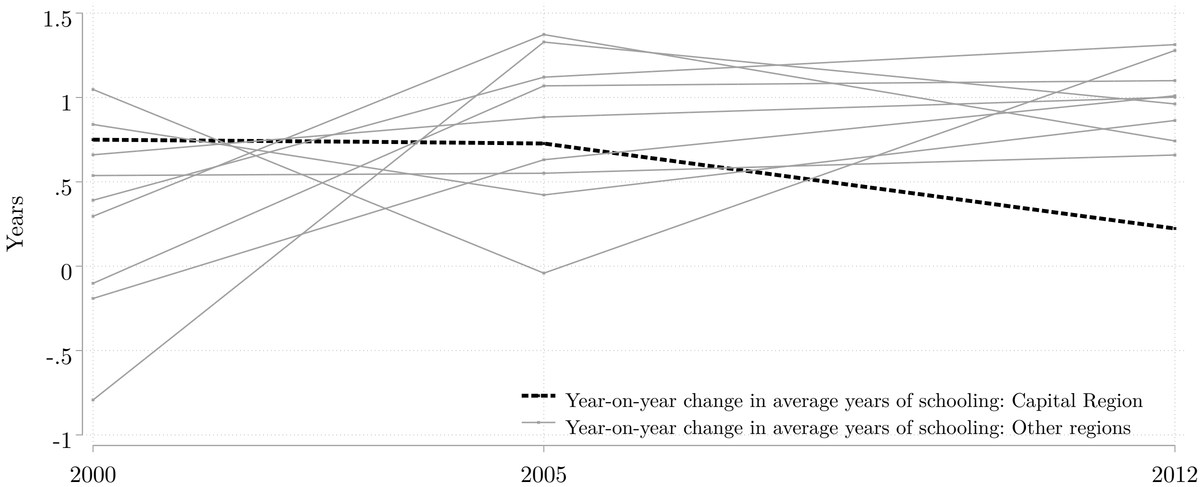


Figure C.3.: Change in average number of years of education over time in departments, based on DHS data. Baseline for inter-temporal comparison is 1994.

C.3.3. Heterogeneity across intensity and types of shock

Figure (C.4) displays $\hat{\tau}$ for comparisons between people who live in the most exposed communes to those who experienced a milder yet still destructive shock ((A) vs. (B) in section (4.6.3)). Setting $\omega_j = .29$, the figure compares the change in outcomes for people living in the relatively more exposed areas, to those individuals who experienced the earthquake with lower magnitude but greater than the value of the marker. For example, one of the comparisons would be: $\omega_j = .29$ and $\omega_k = .8$. The estimation hence compares individuals who experienced a strong exposure ($\omega_{0/1} > .8$) to those who experienced an exposure in the range of $[\omega_j = .29, \omega_k = .8]$. Individuals who experienced an exposure $< .29$ are excluded from the estimation. The scaled PGA distribution has 62 unique values. Given $0 \leq j < k < 1$, 1,830 combinations of ω_j and ω_k can be compared. Figure (C.6) extends figure (C.4), using all possible combinations of ω_j and ω_k.[1] The estimates for $\hat{\tau}$ are consistently statistically significant given $\omega_j \lesssim .3$ and $\omega_k \gtrsim .3$. For larger cutoff values (and hence lager treatment dummies) the estimates for $\hat{\tau}$ are negative but not always statistically significant. The estimate may lack power since the sample and therewith the treatment and control groups become relatively small. However, some evidence suggests that the returns to education for people who live in the most affected communes decrease even more than for those who live in the relatively less severely affected areas. Above findings are confirmed when considering the different levels of education. The returns to primary education appear not to suffer or to even increase in the most affected areas compared to the less severely affected regions. The returns to secondary and tertiary education decrease significantly.[2]

Similarly, figure (C.5) displays $\hat{\tau}$ for comparisons between people who live in exposed communes to those who experienced no or a negligible shock ((B) vs. (C) in section (4.6.3)). Setting $\omega_j = .63$, the figure compares the changes in outcomes for people living in the relatively more exposed areas, to those individuals who experienced the earthquake with lower magnitude but less than the value of the marker. Figure (C.7) extends figure (C.5), using all possible combinations of ω_j and ω_k, applying the same marker scheme as figure (C.6). The estimates for $\hat{\tau}$ are statistically insignificant for the majority of the estimations when eliminating the most exposed regions. The significant estimates for large treatment values recover the initial comparison between individuals who live in strongly affected areas with those who live in unaffected or relatively weakly affected communes. In sum, cutting off few of the most affected communes does not appear to challenge the derived result. The implication from figure (C.7) is that the returns to education appear hardly affected when the individuals are only exposed to a limited physical capital shock. Individuals with secondary education appear to fare significantly better than those with primary education.[3]

In figure (C.7), a group of communes within a relatively tight treatment band of $.14 \lesssim \omega_k \lesssim .2$ (corresponding to $5 \lesssim \text{MMI} \lesssim 6$) appears to experience increasing returns. The neighboring exposure areas do neither show a comparable nor a gradually fading pattern. Some estimates immediately to the "right" of the band in the direction of larger treatment values show significant estimates for $\hat{\tau}$. However, these may be driven by small sizes and hence inaccurate estimations, a pattern which can be observed along the base of the prescribed triangle of the displayed estimations. This observed effect is not constant across the attainment distribution. The partial increase is mainly driven by individuals

[1]The coordinates read (Treatment dummy, $\hat{\tau}$, Cutoff value). Colors/shapes indicate the levels of statistical significance: red/triangle ($p < 0.01$), Bordeaux-red/square ($p < 0.05$), turquoise/diamond ($p < 0.10$), cyan/circle ($p > 0.10$). Each circle represents $\hat{\tau}$ from one individual estimation.

[2]Results available upon request.

[3]Results available upon request.

with completed higher education. For secondary education the $\hat{\tau}$ estimates is positive and significant for $\omega_j \in [.3, .5], \omega_k \in [.15, .24]$. For tertiary education the coefficient is positive yet not significant in this range. For higher values of the treatment dummy the estimated $\hat{\tau}$ is negative and significant for primary and tertiary education but not for secondary education. The estimates for primary education are more sensitive to the shock than for tertiary.

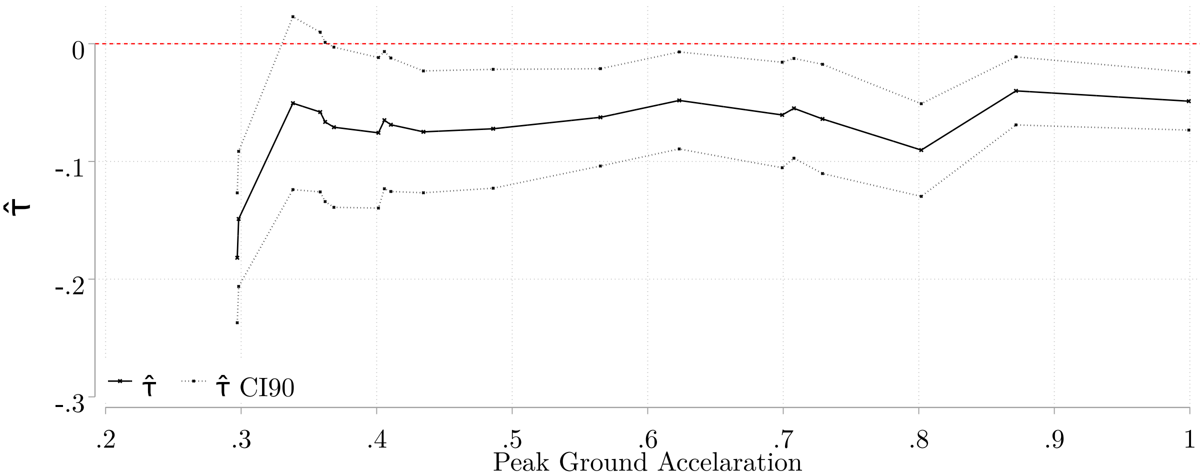


Figure C.4.: Each dot represents the $\hat{\tau}$ coefficient from estimations for all treatment dummies ω_k, given $\omega_j = .29, j < k, j \in [0, 1[$.

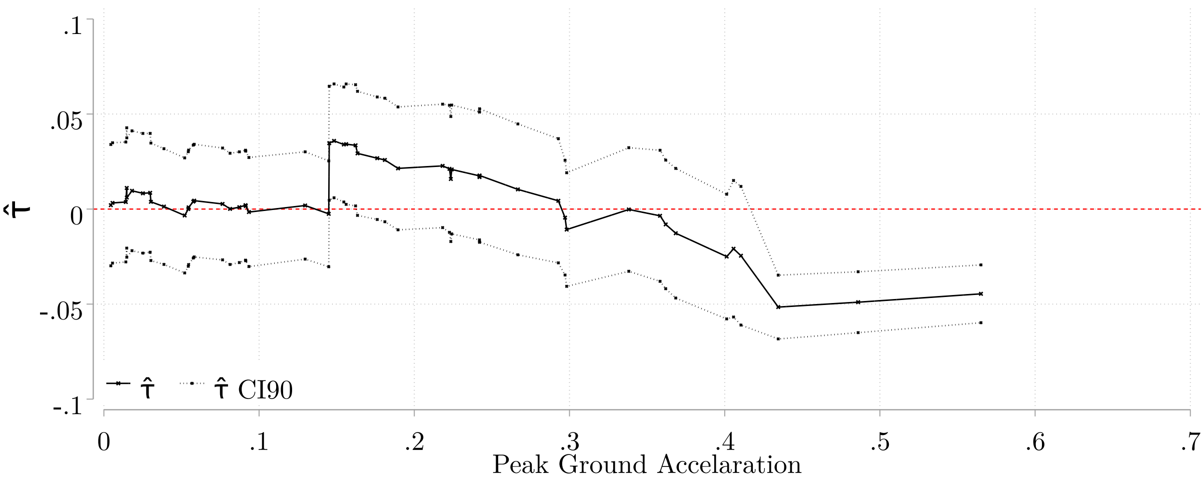


Figure C.5.: Each dot represents the $\hat{\tau}$ coefficient from estimations for all treatment dummies ω_k, given $\omega_j = .63, j > k, j \in]0, 1]$.

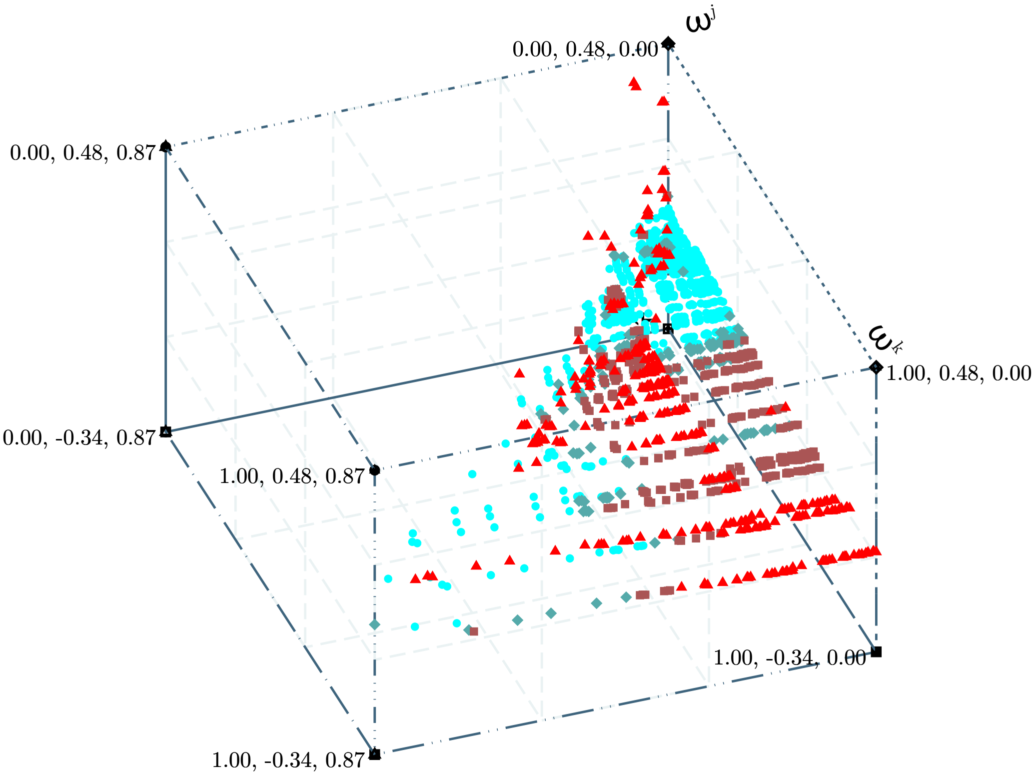


Figure C.6.: Extending figure (C.4) to all possible values of ω_j and ω_k.

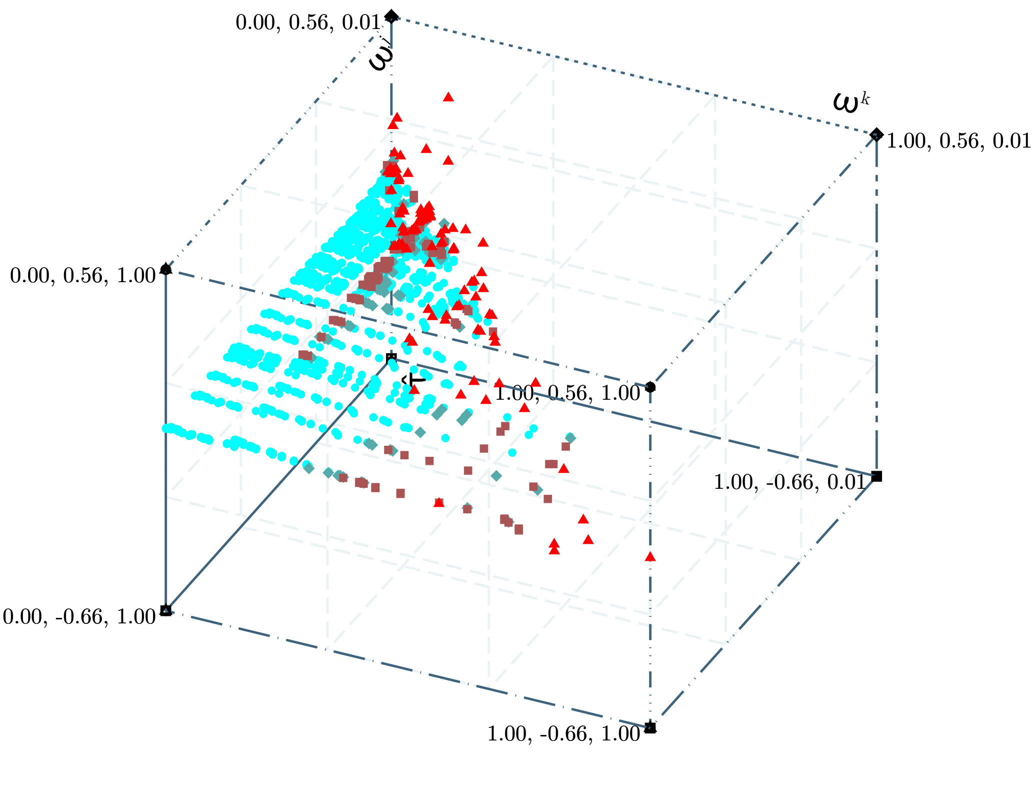


Figure C.7.: Extending figure (C.5) to all possible values of ω_j and ω_k.

C.4. Robustness checks

C.4.1. Background exposure to other shocks and migration

Table C.4.: Overview disasters Haiti based on Guha-Sapir et al. (2015)

Year	Type	Events	Deaths	Injured	Affected	Affected	Cost (10^3 US$)
2000	Flood	2	16			1,200	
2001	Flood	21	26	11	5,070	5,081	
2001	Storm	1					20
2002	Flood	1	31	4	38,335	38,339	1,000
2002	Storm	1	4		250	250	
2003	Drought	1			35,000	35,000	
2003	Epidemic	1	40		200	200	
2003	Flood	3	62	70	150,000	162,390	
2003	Storm	1	26	5		155	
2004	Flood	1	2,665	153	31,130	31,283	
2004	Storm	2	2,757	2,620	302,926	322,094	51,000
2005	Flood	3	17	5	14,078	14,083	
2005	Storm	5	71	53	27,740	27,978	50,500
2006	Flood	2	11	10	24,690	24,700	
2006	Storm	1	5		15,000	15,000	
2007	Flood	4	61	92	89,020	104,961	
2007	Storm	3	102	141	114,680	115,081	
2008	Storm	4	698	86	246,160	246,276	
2009	Flood	3	21		2,796	12,706	
2010	**Earthquake**	1	222,570	300,000	3,400,000	3,700,000	8,000,000
2010	Epidemic	1	6,908	277,451	236,546	513,997	
2010	Flood	3	44	2	22,085	22,087	
2010	Storm	2	27	67	78,075	78,142	
2011	Flood	2	36	8	4,430	4,438	
2011	Storm	2	3	4	2,500	3,044	
2012	Epidemic	2	50		5,817	5,817	
2012	Flood	3	42		26,465	26,465	
2012	Storm	2	88	27	176,500	209,857	254,000
2013	Flood	1	6			33,265	

Table C.5.: T-tests: education by migration (only 2013 data)

A: Education by having lived in another commune during 2010 earthquake†

	means		differences	
	M1 (Not migrated)	M2 (Migrated)	M1-M2	*SE*
Years of Education	5.53	7.13	−1.597	(0.522)
1=No degree attained	0.54	0.40	0.141	(0.055)
1=Primary Education	0.17	0.19	−0.018	(0.041)
1=Secondary Education	0.22	0.35	−0.124	(0.046)
1=Tertiary Education	0.07	0.07	0.001	(0.028)
Observations	2,866			

B: Education by not living department of birth‡

	means		differences	
	M3 (Not migrated)	M4 (Migrated)	M3-M4	*SE*
Years of Education	5.09	6.53	−1.441	(0.187)
1=No degree attained	0.57	0.45	0.124	(0.020)
1=Primary Education	0.15	0.20	−0.047	(0.015)
1=Secondary Education	0.21	0.27	−0.063	(0.017)
1=Tertiary Education	0.07	0.08	−0.014	(0.010)
Observations	2,859			

† Difference = Value M1 (No, live in same commune) - value M2 (Yes, live in other commune).

‡ Difference = Value M3 (No, live in department of birth)
− value M4 (Yes, live in other department).

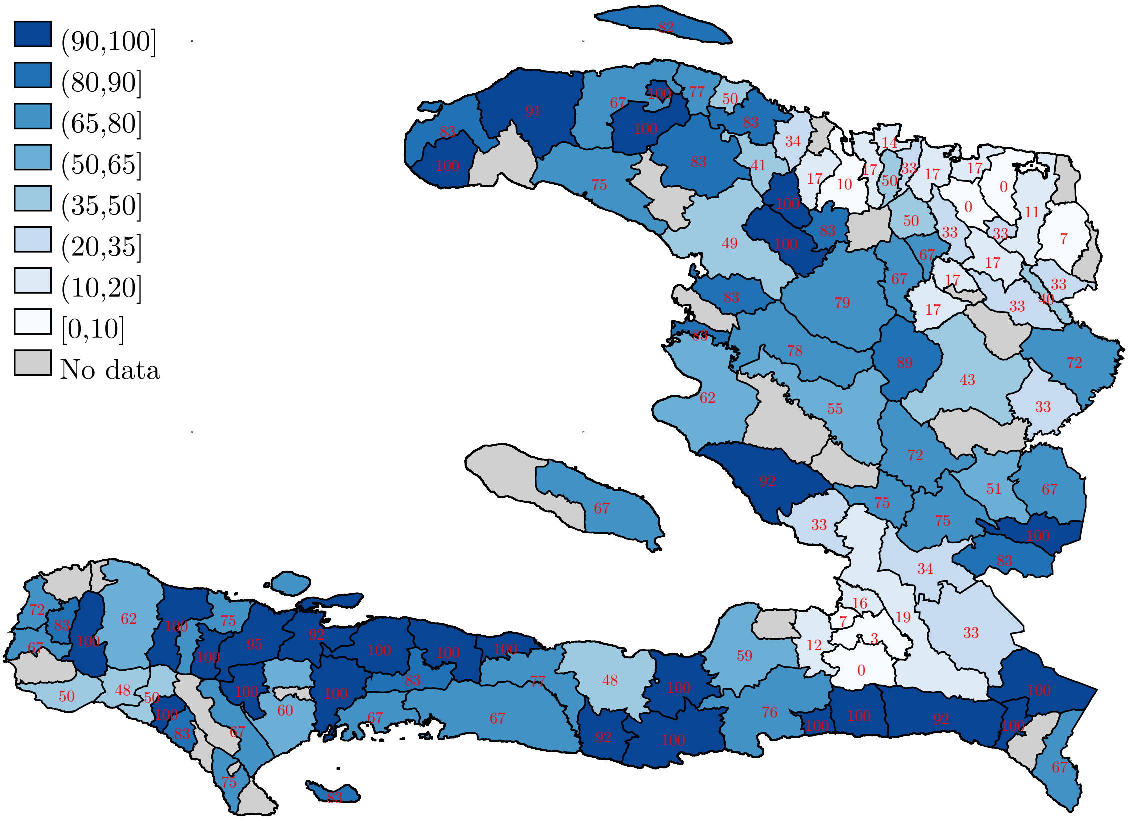


Figure C.8.: Share of households exposed to Hurricane Sandy in communes, based on ECVMAS II, including all households.

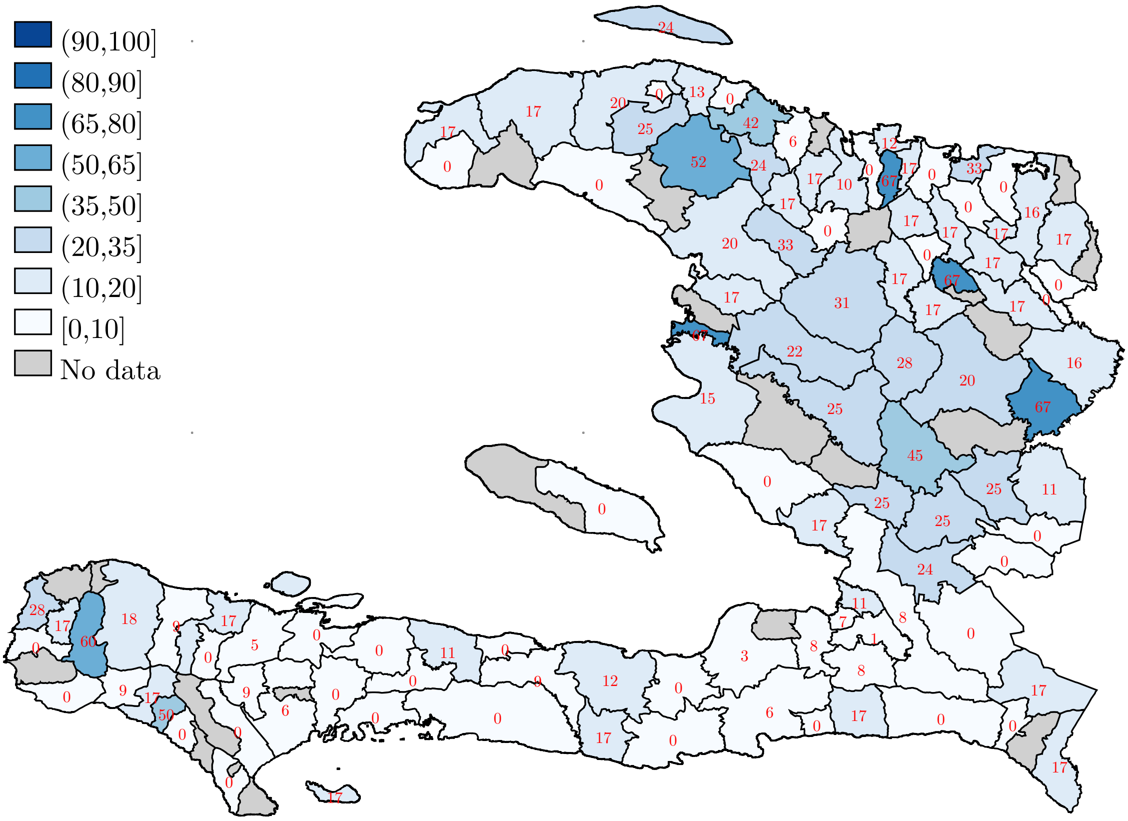


Figure C.9.: Share of households exposed to Cholera epidemic in communes, based on ECVMAS 2012/2013, including all households.

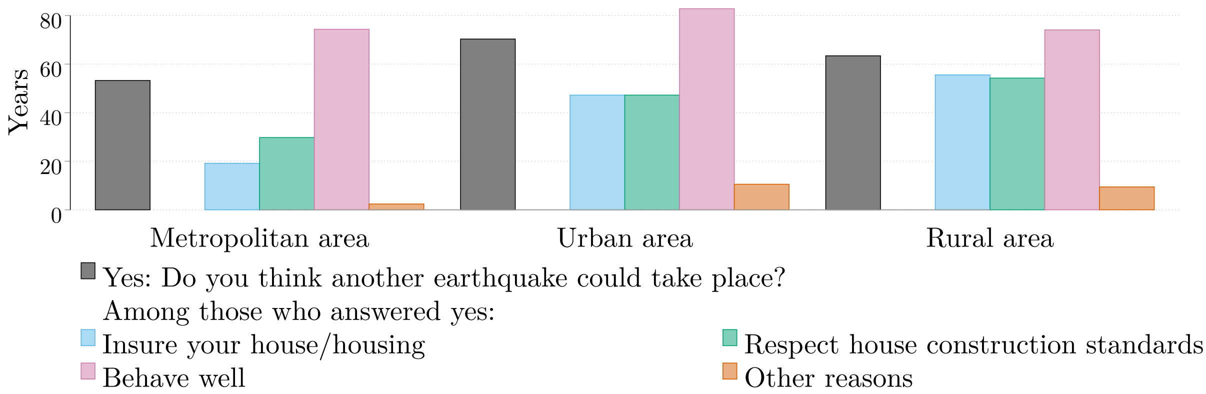


Figure C.10.: Expectations and precautions of future earthquakes, based on ECVMAS II. Moving was not an explicit option. It falls into the “other” category.

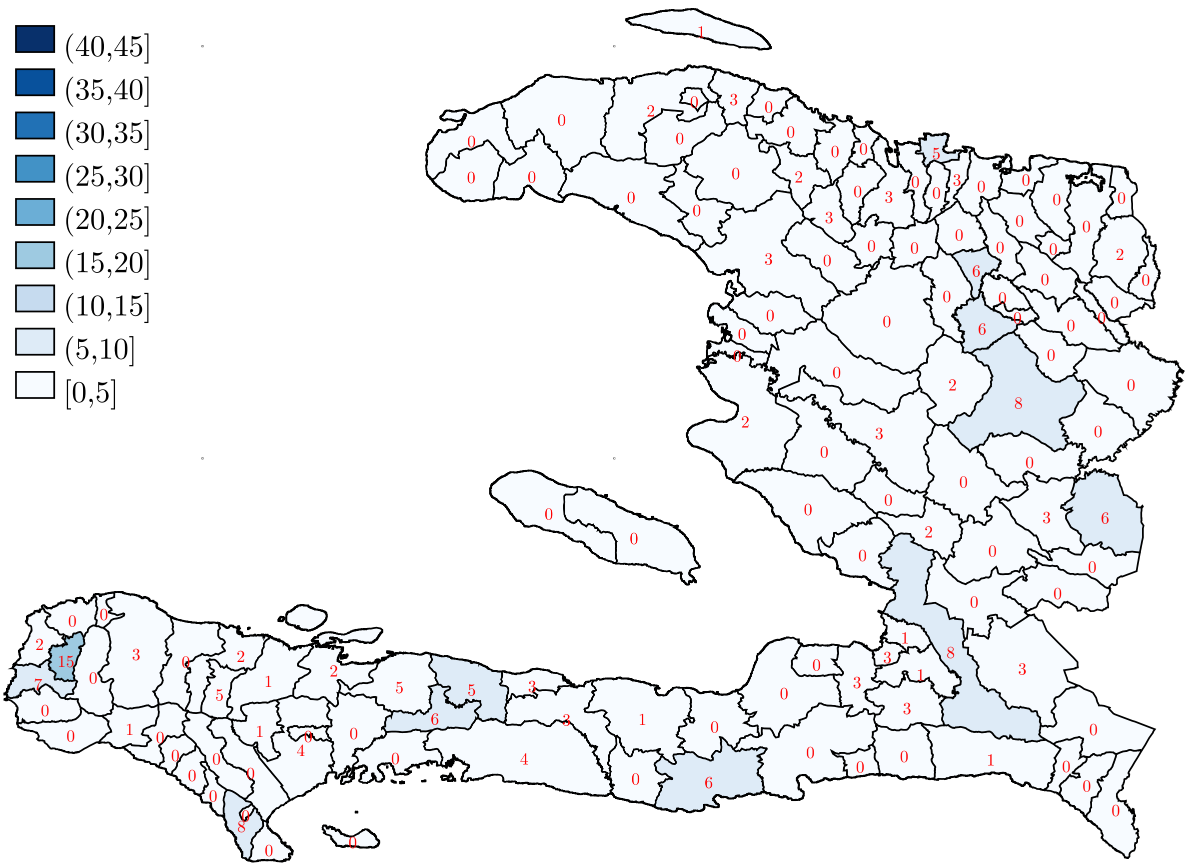


Figure C.11.: Share of population in commune who lived in another commune and not abroad, before 2010 earthquake, based on ECVMAS I, II.

C.4.2. Results

Table (C.6) revisit the main results from table (4.2), controlling for confounding shocks and migration pattern. Columns (Aa) and (Ab) repeat columns (2b) and (4) but include shock indicators for exposure to Cholera and Hurricane Sandy as well as living in a temporary shelter. Columns (Ba)/(Bb): Like (A) but controlling for earthquake-related migration. (Ca)/(Cb): Like (B) but controlling for internal migration. Figure (C.12) visualizes the results.

Table C.6.: Confounding shocks and migration

	Years of education			Levels of education		
	(Aa)	(Ba)	(Ca)	(Ab)	(Bb)	(Cb)
S (Years) × ω_c × T	−0.05*	−0.04*	−0.04*			
	(0.03)	(0.03)	(0.03)			
Primary × ω_c × T				−0.47**	−0.51**	−0.50**
				(0.22)	(0.21)	(0.21)
Secondary × ω_c × T				−0.35	−0.29	−0.28
				(0.40)	(0.41)	(0.40)
Tertiary × ω_c × T				−0.70*	−0.64	−0.63
				(0.40)	(0.40)	(0.40)
1=Cholera 2012	−0.01	−0.01	−0.01	−0.03	−0.03	−0.03
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Ave. Cholera 2012	−0.53	−0.51	−0.51	−0.48	−0.47	−0.48
	(0.34)	(0.34)	(0.34)	(0.35)	(0.35)	(0.35)
1=Sandy 2012	−0.14**	−0.14**	−0.13*	−0.12*	−0.12*	−0.12
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Ave. Sandy 2012	−0.21	−0.22	−0.21	−0.27	−0.27	−0.24
	(0.30)	(0.29)	(0.29)	(0.31)	(0.31)	(0.30)
1=living in camp	0.07	0.06	0.06	0.03	0.03	0.02
	(0.11)	(0.12)	(0.12)	(0.10)	(0.10)	(0.10)
1=migra quake 2012		−0.05	−0.05		−0.05	−0.06
		(0.21)	(0.22)		(0.20)	(0.21)
Ave. migra quake 2012		0.50	0.53		0.29	0.27
		(1.38)	(1.43)		(1.31)	(1.35)
1=migra ever 2012			0.03			0.05
			(0.09)			(0.09)
Ave. migra ever 2012			−0.03			0.03
			(0.37)			(0.35)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes	Yes	Yes
Dep. FE × T	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,989	8,643	8,643	8,910	8,564	8,564
R^2	0.32	0.33	0.33	0.32	0.33	0.33
F	129.91	132.05	146.41	173.86	171.89	169.16

The dependent variable in all models is labor income including autoconsumption, education in years of schooling. Controls in matrix Z: gender (male=1, female=0); employment (employee=1, self-employed=0); urban (urban (metropolitan area)=2, urban (other)=1, rural=0; included as two dummies with rural as comparison group); department FE; department FE × time. Eicker–Huber–White robust standard errors, clustered at commune level, in parentheses. Significance: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

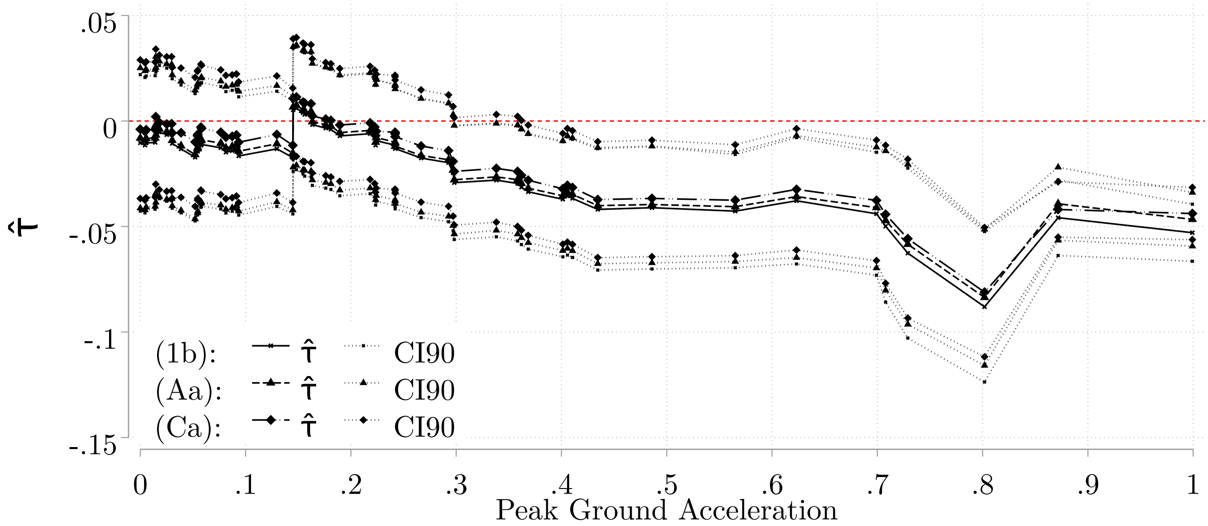


Figure C.12.: Each marker represents the $\hat{\tau}$ coefficient from estimations for all treatment dummies $\omega_{0/1}$: (1b) from table (4.2). (Aa), (Ca) from table (C.6).

C.4.3. Alternative control group

Table (C.7) revisits table (4.2), focusing on alternative control groups to foster comparability with the treatment areas. Columns (1) and (2) mimic columns (2b) and (4) from table (4.2).

Table C.7.: Main results across different regions

	Rural area		Urban area		Metropolitan area	
	(1)	(2)	(1)	(2)	(1)	(2)
S (Years) × ω_c × T	0.04		−0.11**		−0.33***	
	(0.05)		(0.05)		(0.10)	
Primary × ω_c × T		0.64		−3.25***		0.98
		(0.52)		(0.80)		(1.63)
Secondary × ω_c × T		0.47		−2.18***		−3.27**
		(0.68)		(0.75)		(1.51)
Tertiary × ω_c × T		−0.44		−2.61***		−2.82*
		(0.84)		(0.88)		(1.48)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes	Yes	Yes
Dep. FE × T	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,550	5,520	1,787	1,766	1,652	1,624
R^2	0.24	0.23	0.35	0.36	0.28	0.26
F	77.19	64.68	114.26	331.95	58.54	28.12

The dependent variable in all models is *log* labor income including autoconsumption. Controls in matrix Z: gender (male=1, female=0); employment (employee=1, self-employed=0); department FE; department FE × time. Eicker–Huber–White robust standard errors, clustered at commune level, in parentheses. Significance: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$). In specifications for the metropolitan are standard errors are not clustered at the commune level to avoid exhausting the degrees of freedom. Clustering the standard errors at the commune level at the expense of exhausting the degrees of freedom still yields significant estimates in (1) though for (2) the previously obtained significance vanishes.

C.4.4. Omitted variable bias: endogeneity of education

To assess the relative importance of omitted unobservable variables such as ability, the degree of selection on unobservables relative to selection on observables can be explored. Oster (2017) expands a previous literature to test for relative selection on unobservables in estimations. The approach compares the changes in the coefficient of interest as well as the overall fit of the model (R^2) in different specifications, relative to a theoretically attainable, unobservable, model fit for the application at hand which would be based on selection on observables and unobservables. For this maximal attainable R^2, R_{max}, the empirical selection approach suggested by Oster (2017) is being applied, which follows an empirical meta-analysis of published papers in several "top journals" (Oster, 2017, p. 2).

Table (C.8) shows the results of applying her methodology to different models. $\hat{\delta}|\beta_1 = 0$ shows for each model the estimated degree of selection on unobservables relative to selection on observables which would be required in order to attain an estimate of $\beta_1 = 0$ in the simplified version of equation (4.1), i.e. zero returns to education. The confounding shocks specification mimics column (Aa) in table (C.6) and includes household level exposure to the Cholera epidemic and Hurricane Sandy, corresponding commune averages, and a dummy for living in temporary shelter ("camp"). The migration specification mimics column (Ca) in table (C.6) and includes the indicators for the confounding shocks as well as for earthquake-related migration and internal migration and the relevant commune level averages. The results show that the selection on unobservables would have to be more than 1.8 times stronger than the selection on observables to 'explain away' the effect of years of education on income. In addition, table (C.8) shows that the estimate is relatively stable over time and across different specifications.

To assess the robustness of given estimates, Altonji et al. (2005) and Oster (2017) suggest a threshold of $\hat{\delta} = 1$, i.e. equal selection on observables and unobservables. Since the threshold is reliably exceeded, and because the potential bias appears to relatively constant over time and specification, selection on unobservables is not deemed a major concern in the application at hand, and OLS is a suitable estimation approach. This result was also found in other relatively less affluent settings (Gundersen, 2016). As such the education outcomes will not be instrumented for, circumventing challenges related to this specific application (Söderbom et al., 2015; Psacharopoulos and Patrinos, 2004; Harmon et al., 2003; Krueger and Lindahl, 2001; Heckman et al., 2006; Angrist and Krueger, 1999).

Table C.8.: Selection on unobservables following Oster (2017)

Specification	Year	$\hat{\delta}\vert\beta_1 = 0$	(SE)
Extended specification, full Z	2001	1.82	(0.17)
Add controls for confounding shocks	2001	2.03	(0.21)
Add controls for confounding shocks, migration	2001	2.04	(0.21)
Extended specification, full Z	2013	1.98	(0.40)
Add controls for confounding shocks	2013	2.01	(0.45)
Add controls for confounding shocks, migration	2013	2.07	(0.48)

The dependent variable in all models is *log* labor income including autoconsumption. Extended model: Mincerian equation with full matrix Z of controls: gender (male=1, female=0); employment (employee=1, self-employed=0); urban (urban (metropolitan area)=2, urban (other)=1, rural=0; included as two dummies with rural as comparison group); department FE. Standard errors in parentheses, based on 10,000 bootstrap replications. R_{max} empirically selected following Oster (2017).

C.4.5. Alternative specifications for heterogeneity across intensity and types of shock

C.4.5.1. Multiple treatment dummies

Equation (4.1) includes one treatment dummy $0 < \omega_{0/1} < 1$. Extending this approach, the specification applied here yields two treatment dummies $0 < \omega^l_{0/1} < \omega^u_{0/1} < 1$ which vary along the exposure distribution. Extending equation (4.1) to include the second fully interacted treatment indicator and following previous notation yields two coefficients of interest, $\hat{\tau}^l$ and $\hat{\tau}^u$, for the double interactions $(S_{ict} \times \omega^l_{0/1} \times T_t)$ and $(S_{ict} \times \omega^u_{0/1} \times T_t)$, respectively. Applying education in years of schooling and building up on figure (4.3), figures (C.13) and (C.14) are two examples for fixing a $\omega^l_{0/1} = \bar{\omega}^l_{0/1}$ and letting $\omega^u_{0/1}$ vary. The figures display the estimates of the two double interaction effects. As evident, either the upper or the lower coefficient is significant, while the difference between the two estimates is significantly different from zero. This confirms the previous results. The dual shock and the pure physical capital shock have different effects but there exists less or no difference between non-exposed individuals and those experiencing only physical capital shock. Similarly, figure (C.2) can be extended following the same approach. The derived results prevail. Results available upon request.

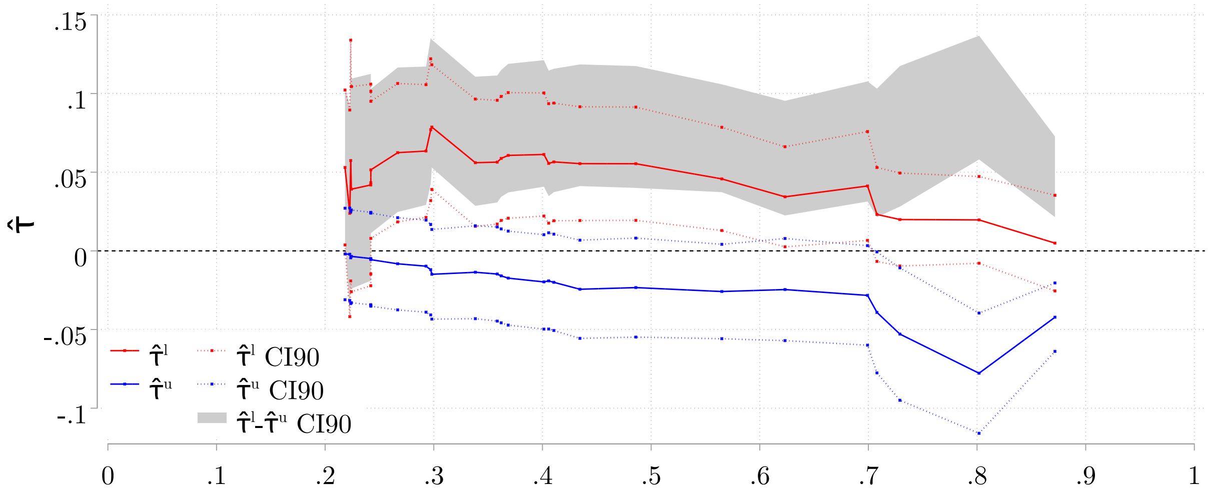


Figure C.13.: Each dot represents the coefficients $\hat{\tau}^l$ and $\hat{\tau}^u$ from estimations applying a lower and an upper treatment dummy $\bar{\omega}^l_{0/1} \approx 0.16$ and $\omega^u_{0/1} \in [.3, .9]$.

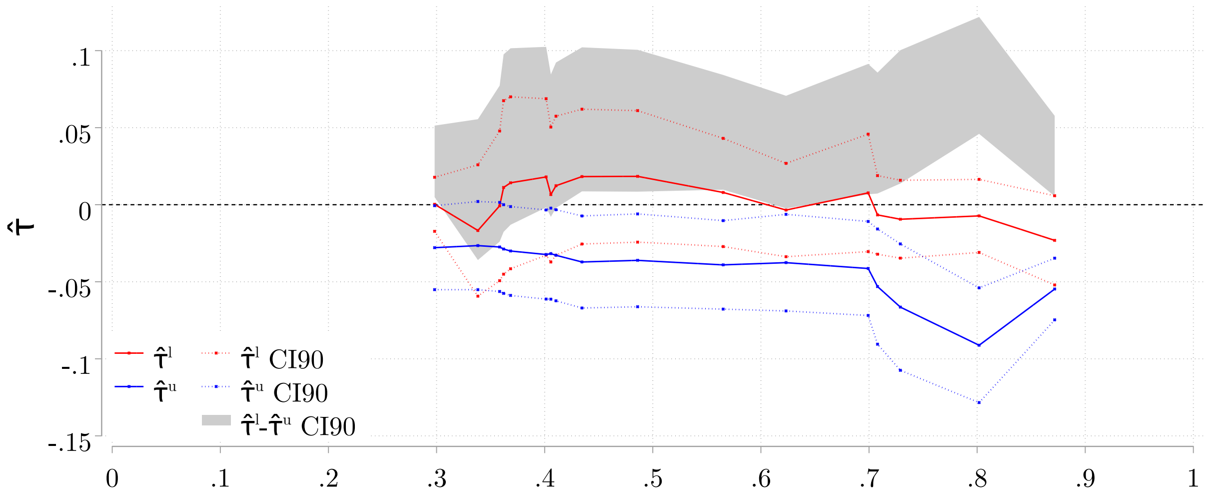


Figure C.14.: Each dot represents the coefficients $\hat{\tau}^l$ and $\hat{\tau}^u$ from estimations applying a lower and an upper treatment dummy $\bar{\omega}^l_{0/1} \approx .30$, $\omega^u_{0/1} \in [.3, .9]$.

C.4.5.2. Higher order polynomials

The results in table (C.9) columns (1a)-(3a) build up on table (4.2) column (2b). Applying equation (4.1), column (1a) employs ω^1_c (the original specification), columns (2a) and (3a) add higher order treatment polynomials ω^2_c and ω^3_c, respectively. While some the higher polynomials are significant, others turn statistically insignificant. However, the null hypotheses that the interaction effects are jointly equal to zero is rejected in all specifications. This puzzling observation may be due to the high correlation of the elements in the treatment-education-time matrix Θ_{ict}, especially in case of higher order polynomials. For order 2: $\Theta_{ict} = \left[S_{ict}, T_t \times S_{ict}, T_t, \omega^1_c, \omega^2_c, T_t \times \omega^1_c, T_t \times \omega^2_c, S_{ict} \times \omega^1_c, S_{ict} \times \omega^2_c, T_t \times S_{ict} \times \omega^1_c, T_t \times S_{ict} \times \omega^2_c\right]$. While the multicollinearity affects the efficiency of the estimators by increasing the standard errors, it does not affect its consistency. As such, the results in table (C.9) confirm the previous results. There is a difference between dual shock and pure physical capital shock but there is less or no difference between non-exposed individuals and those experiencing only physical capital shock.

Columns (1b)-(3b) in table (C.9) build up on table (4.2) column (4), following the same structure as columns (1a)-(3a). The previously attained results are being confirmed.

Table C.9.: Higher order polynomials

	Years of education			Levels of education		
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)
S (Years) × ω_c^1 × T	−0.05**	0.10	0.18			
	(0.03)	(0.09)	(0.17)			
S (Years) × ω_c^2 × T		−0.19*	−0.38			
		(0.10)	(0.42)			
S (Years) × ω_c^3 × T			0.12			
			(0.28)			
Primary × ω_c^1 × T				−0.47**	−1.17	1.63
				(0.22)	(0.91)	(1.81)
Secondary × ω_c^1 × T				−0.39	1.48	1.32
				(0.40)	(1.08)	(2.21)
Tertiary × ω_c^1 × T				−0.76*	0.67	2.72
				(0.40)	(1.47)	(3.90)
Primary × ω_c^2 × T					0.82	−7.68
					(0.97)	(4.66)
Secondary × ω_c^2 × T					−2.22*	−1.15
					(1.20)	(5.48)
Tertiary × ω_c^2 × T					−1.79	−7.26
					(1.56)	(9.35)
Primary × ω_c^3 × T						6.33**
						(3.20)
Secondary × ω_c^3 × T						−1.01
						(3.74)
Tertiary × ω_c^3 × T						3.82
						(6.00)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes	Yes	Yes
Dep. FE × T	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,989	8,989	8,989	8,910	8,910	8,910
R^2	0.32	0.33	0.33	0.32	0.32	0.33
F	98.82	137.36	215.42	94.29	266.30	1,753.51
p(IE=0)¶	0.05	0.02	0.03			
p(Primary IE=0)¶				0.04	0.09	0.04
p(Secondary IE=0)¶				0.33	0.10	0.10
p(Tertiary IE=0)¶				0.06	0.02	0.06
p(Primary=Secondary)¶¶				0.84	0.27	0.03
p(Primary=Tertiary)¶¶				0.50	0.04	0.05
p(Secondary=Tertiary)¶¶				0.30	0.24	0.85

The dependent variable in all models is labor income including autoconsumption, education in years of schooling. Controls in matrix Z: gender (male=1, female=0); employment (employee=1, self-employed=0); urban (urban (metropolitan area)=2, urban (other)=1, rural=0; included as two dummies with rural as comparison group); department FE; department FE × time. ¶ Test for joint significant difference from zero for interaction effects. ¶¶ Test for equality of sum of estimators for interaction effects. Eicker–Huber–White robust standard errors, clustered at commune level, in parentheses. Significance: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

C.4.6. Cluster level fixed effects

Table (C.10) extends the main analysis by modifying matrix Z. In particular, in both panels, instead of including department fixed effects and year specific department fixed effects, columns (1a) include only department fixed effects, columns (1b) replace department fixed effects with commune fixed effects, and columns (1c) include commune fixed effects, department fixed effects, and year specific department fixed effects. Columns (2a)-(2c) follow the same logic. The substance of the results is unchanged.

Table C.10.: Cluster level fixed effects

A: Education measured as years of education

	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
S (Years) × $\omega_{0/1}$ × T	−0.04**	−0.04**	−0.03*			
	(0.02)	(0.02)	(0.02)			
S (Years) × ω_c × T				−0.06**	−0.06**	−0.05**
				(0.03)	(0.03)	(0.03)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Commune FE	No	Yes	Yes	No	Yes	Yes
Department FE	Yes	No	Yes	Yes	No	Yes
Dep. FE × T	No	No	Yes	No	No	Yes
Observations	8,989	9,077	8,989	8,989	9,077	8,989
R^2	0.32	0.35	0.36	0.32	0.35	0.35
F	90.32	.	.	103.67	.	.

B: Education measured as levels of degree attainment

	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Primary × $\omega_{0/1}$ × T	−0.41**	−0.49***	−0.39***			
	(0.18)	(0.16)	(0.13)			
Secondary × $\omega_{0/1}$ × T	−0.31	−0.35	−0.26			
	(0.25)	(0.25)	(0.25)			
Tertiary × $\omega_{0/1}$ × T	−0.71**	−0.69**	−0.59**			
	(0.30)	(0.28)	(0.26)			
Primary × ω_c × T				−0.55**	−0.69***	−0.58***
				(0.25)	(0.23)	(0.19)
Secondary × ω_c × T				−0.44	−0.47	−0.42
				(0.39)	(0.39)	(0.39)
Tertiary × ω_c × T				−0.89**	−0.88**	−0.80**
				(0.41)	(0.38)	(0.37)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Commune FE	No	Yes	Yes	No	Yes	Yes
Department FE	Yes	No	Yes	Yes	No	Yes
Dep. FE × T	No	No	Yes	No	No	Yes
Observations	8,910	8,998	8,910	8,910	8,998	8,910
R^2	0.31	0.34	0.35	0.31	0.34	0.35
F	110.66	.	.	93.40	.	.

The dependent variable in all models is *log* labor income including autoconsumption. Controls in matrix Z: gender (male=1, female=0); employment (employee=1, self-employed=0); urban (urban (metropolitan area)=2, urban (other)=1, rural=0; included as two dummies with rural as comparison group); department FE; department FE × Time. Eicker–Huber–White robust standard errors, clustered at commune level, in parentheses. Significance: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

C.4.7. Different weighting approaches

Table (C.11) employs alternative weights compared to mapped weights in the main specification. Column (No weights) reproduces the results from table (4.2), column (M2e), without applying any weights. Column (Scaled weights) reproduces the results from table (4.2), column (2b). Column (Original weights) replaces the weights applied in column (Scaled weights) with the original expansion weights from ECVH and the 2013 post-earthquake survey (L'Enquête sur les Conditions de Vie des Ménages Après Séisme, ECVMAS II). The substance of the results is unchanged. A similar result is obtained when repeating tables (C.11) using levels of attainment instead of years of schooling.

Table C.11.: Alternative weights

	(No weights)	(Scaled weights)	(Original weights)
S (Years) × ω_c × T	-0.05^{**}	-0.05^{*}	-0.08^{***}
	(0.03)	(0.03)	(0.02)
Controls	Yes	Yes	Yes
Department FE	Yes	Yes	Yes
Dep. FE × T	Yes	Yes	Yes
Observations	8,989	8,989	8,989
R^2	0.32	0.32	0.35
F	98.82	96.75	108.29

The dependent variable in all models is *log* labor income including autoconsumption. Controls in matrix Z: gender (male=1, female=0); employment (employee=1, self-employed=0); urban (urban (metropolitan area)=2, urban (other)=1, rural=0; included as two dummies with rural as comparison group); department FE; department FE × Time. Eicker–Huber–White robust standard errors, clustered at commune level, in parentheses. Significance: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

C.4.8. Data quality/mismeasured education variable

Table (C.12) shows the results for an alternative coding of years of education. Column (1a): Education measured in years of schooling capped at 13 years, treatment is 0/1 dummy with cutoff at scaled PGA 0.37, baseline specification without further controls. Column (1b): Like column (1a), with full matrix Z of controls, department fixed effects, and year specific department fixed effects. Columns (2a) and (2b): Like column (1a) and (1b), respectively, but replacing the one treatment dummy with one continuous treatment variable.

Table C.12.: Censored years of schooling

	(1a)	(1b)	(2a)	(2b)
S (Years; max 13)	0.11^{***}	0.10^{***}	0.11^{***}	0.10^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
S (Years; max 13) × T	0.01	−0.00	0.02	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
S (Years; max 13) × $\omega_{0/1}$	0.05^{***}	0.03^{**}		
	(0.02)	(0.02)		
S (Years; max 13) × $\omega_{0/1}$ × T	-0.06^{**}	-0.04^{**}		
	(0.02)	(0.02)		
S (Years; max 13) × ω_c			0.05^{*}	0.04^{*}
			(0.02)	(0.03)
S (Years; max 13) × ω_c × T			-0.08^{***}	-0.06^{**}
			(0.03)	(0.03)
Controls	No	Yes	No	Yes
Department FE	No	Yes	No	Yes
Dep. FE × T	No	Yes	No	Yes
Observations	11,539	8,989	11,539	8,989
R^2	0.23	0.32	0.24	0.31
F	103.47	89.39	100.22	99.55

The dependent variable in all models is *log* labor income including autoconsumption. Controls in matrix Z: gender (male=1, female=0); employment (employee=1, self-employed=0); urban (urban (metropolitan area)=2, urban (other)=1, rural=0; included as two dummies with rural as comparison group); department FE; department FE × Time. Eicker–Huber–White robust standard errors, clustered at commune level, in parentheses. Significance: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

C.5. Additional results: channels of decreasing returns to education

Table (C.13) presents the estimations for individual labor market affiliation. The dependent variables in the models are: column (1): 1=Labor income, column (2): 1=Working, column (3): 1=Any secondary job, and column (4): 1=Employed.

Table (C.14) shows the change in the sector-specific Mincerian returns in the primary activity of the individuals. Each specification limits the sample to a specific sector: column (1): Sector agriculture/fishery, column (2): Sector construction, column (3): Sector industry, column (4): Sector trade, column (5): Sector transportation, column (6): Sector education, column (7): Sector health, and column (8): Sector other.

Table (C.15) explores the sector of activity. Each specification focused on one branch. The dependent variable in the models are: column (1): 1=Sector agriculture/fishery, (2): 1=Sector construction, (3): 1=Sector industry, (4): 1=Sector trade, (5): 1=Sector transportation, (6): 1=Sector education, (7): 1=Sector health, (8): 1=Sector other.

Table (C.16) present the sub-analyses by individual characteristics. Columns (1) limit the sample to individuals aged [25,65]. Columns (2) limit the sample to individuals aged [15,24]. Columns (3) limit the sample to males. Columns (4) limit the sample to females.

Table C.13.: Labor market affiliation

A: Education measured as years of education

	(1)	(2)	(3)	(4)
S (Years) × ω_c × T	−0.00	0.01	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Controls	Yes	Yes	Yes	Yes
W/ Employment	No	No	Yes	No
Department FE	Yes	Yes	Yes	Yes
Dep. FE × T	Yes	Yes	Yes	Yes
Observations	22,607	16,266	8,989	8,989
R^2	0.30	0.31	0.08	0.21
F	184.63	324.90	34.87	36.56

B: Education measured as levels of degree attainment

	(1)	(2)	(3)	(4)
Primary × ω_c × T	−0.03	−0.00	0.05	0.13**
	(0.06)	(0.07)	(0.08)	(0.06)
Secondary × ω_c × T	−0.01	0.01	0.08	0.22*
	(0.06)	(0.07)	(0.08)	(0.12)
Tertiary × ω_c × T	−0.06	0.03	−0.15	0.05
	(0.09)	(0.12)	(0.12)	(0.14)
Controls	Yes	Yes	Yes	Yes
W/ Employment	No	No	Yes	No
Department FE	Yes	Yes	Yes	Yes
Dep. FE × T	Yes	Yes	Yes	Yes
Observations	22,309	15,972	8,910	8,910
R^2	0.30	0.30	0.07	0.24
F	327.02	274.29	26.47	109.02

The dependent variables in the columns are: (1): 1=Labor income, (2): 1=Working, (3): 1=Any secondary job, (4): 1=Employed. Controls in matrix Z: gender (male=1, female=0); urban (urban (metropolitan area)=2, urban (other)=1, rural=0; included as two dummies with rural as comparison group); department FE. Eicker–Huber–White robust standard errors, clustered at commune level, in parentheses. Significance: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table C.14.: Changing sector-specific Mincerian returns to education in primary activity

A: Education measured as years of education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
S (Years) × ω_c × T	0.06	−0.06	−0.29**	−0.05	0.11	−0.09	0.21	−0.04
	(0.05)	(0.09)	(0.11)	(0.05)	(0.13)	(0.14)	(0.21)	(0.05)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,804	302	495	2,825	223	332	120	787
R^2	0.20	0.25	0.35	0.30	0.23	0.34	0.31	0.39
F	31.62	14.67	32.22	74.72	12.24	36.02	66.29	51.69

B: Education measured as levels of degree attainment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Primary × ω_c × T	0.84	−0.29	−3.17***	−0.22	−2.15**	0.00	0.00	−0.28
	(0.56)	(0.71)	(1.12)	(0.32)	(1.05)	(.)	(.)	(0.58)
Secondary × ω_c × T	0.17	−0.45	−3.00***	0.30	−0.43	3.26**	4.79	−1.46
	(0.94)	(0.91)	(1.07)	(0.47)	(1.27)	(1.48)	(3.54)	(0.96)
Tertiary × ω_c × T	−2.47	−10.19**	−3.09***	−0.80	−3.97*	2.19	5.04	−0.75
	(1.61)	(3.89)	(0.93)	(0.93)	(2.32)	(1.41)	(3.05)	(0.84)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,789	299	490	2,801	218	329	118	767
R^2	0.19	0.26	0.33	0.30	0.21	0.28	0.32	0.42
F	30.70	31.67	66.73	89.62	171.16	.	.	105.87

The dependent variable in all models is labor income including autoconsumption, limiting the samples to specific groups/sectors: (1): 1=Sector agriculture/fishery, (2): 1=Sector construction, (3): 1=Sector industry, (4): 1=Sector trade, (5): 1=Sector transportation, (6): 1=Sector education, (7): 1=Sector health, (8): 1=Sector other. Controls in matrix Z: gender (male=1, female=0); employment (employee=1, self-employed=0); urban (urban (metropolitan area)=2, urban (other)=1, rural=0; included as two dummies with rural as comparison group); department FE. Eicker–Huber–White robust standard errors, clustered at commune level, in parentheses. Significance: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table C.15.: Changing returns to working in sector of primary activity

A: Education measured as years of education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
S (Years) × ω_c × T	0.02***	−0.00*	0.00	−0.02***	−0.01**	0.01**	0.01	−0.00
	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,986	8,986	8,986	8,986	8,986	8,986	8,986	8,986
R^2	0.43	0.07	0.05	0.35	0.04	0.20	0.05	0.10
F	198.07	17.56	14.52	286.19	62.35	28.52	12.41	70.54

B: Education measured as levels of degree attainment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Primary × ω_c × T	0.16**	0.03	−0.08*	−0.12**	−0.03	0.01	−0.02**	0.07
	(0.07)	(0.04)	(0.04)	(0.06)	(0.04)	(0.02)	(0.01)	(0.06)
Secondary × ω_c × T	0.20**	−0.05	−0.00	−0.24**	−0.11*	0.19***	0.04	−0.00
	(0.10)	(0.05)	(0.05)	(0.10)	(0.06)	(0.06)	(0.04)	(0.06)
Tertiary × ω_c × T	0.17	0.00	−0.03	−0.27**	0.02	0.01	0.13*	−0.04
	(0.10)	(0.05)	(0.08)	(0.12)	(0.03)	(0.11)	(0.07)	(0.11)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,907	8,907	8,907	8,907	8,907	8,907	8,907	8,907
R^2	0.43	0.08	0.06	0.35	0.05	0.25	0.06	0.10
F	190.70	84.63	22.79	321.74	121.38	57.51	47.62	88.48

The dependent variables in the columns are: (1): 1=Sector agriculture/fishery, (2): 1=Sector construction, (3): 1=Sector industry, (4): 1=Sector trade, (5): 1=Sector transportation, (6): 1=Sector education, (7): 1=Sector health, (8): 1=Sector other services. Controls in matrix Z: gender (male=1, female=0); employment (employee=1, self-employed=0); urban (urban (metropolitan area)=2, urban (other)=1, rural=0; included as two dummies with rural as comparison group); department FE. Eicker–Huber–White robust standard errors, clustered at commune level, in parentheses. Significance: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table C.16.: Individual characteristics

A: Education measured as years of education

	(1)	(2)	(3)	(4)
S (Years) × ω_c × T	−0.05**	0.03	−0.05**	−0.06
	(0.03)	(0.08)	(0.02)	(0.04)
Controls	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes
Dep. FE × T	Yes	Yes	Yes	Yes
Observations	8,082	907	4,884	4,105
R^2	0.31	0.31	0.34	0.30
F	92.86	31.46	75.95	84.50

B: Education measured as levels of degree attainment

	(1)	(2)	(3)	(4)
Primary × ω_c × T	−0.48*	−0.58	−0.19	−0.91***
	(0.27)	(0.87)	(0.28)	(0.31)
Secondary × ω_c × T	−0.29	−0.73	−0.76**	0.03
	(0.41)	(0.68)	(0.33)	(0.64)
Tertiary × ω_c × T	−0.75*	−1.22	−0.86**	−0.69
	(0.39)	(1.13)	(0.43)	(0.58)
Controls	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes
Dep. FE × T	Yes	Yes	Yes	Yes
Observations	8,011	899	4,834	4,076
R^2	0.31	0.32	0.33	0.30
F	112.97	43.24	89.50	154.82

The dependent variable in all models is *log* labor income including autoconsumption. Controls in matrix Z: gender (male=1, female=0); employment (employee=1, self-employed=0); urban (urban (metropolitan area)=2, urban (other)=1, rural=0; included as two dummies with rural as comparison group); department FE; department FE × Time. Eicker–Huber–White robust standard errors, clustered at commune level, in parentheses. Significance: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).