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I am grateful for my family's support throughout my education.

Completing a dissertation is a big deal, but my greatest achievement during my years as a research assistant was marrying Marion. Without her, I wouldn't get anything done.

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Zusammenfassung

Die in dieser Arbeit dargestellten Studien berühren eine große Bandbreite von Themen in der Arbeitsökonomik, Bildungsökonomik und Entwicklungsökonomik, wobei ein gemeinsamer Nenner der Schwerpunkt auf die Lösung von Problemen statistischer Endogenität im Rahmen der jeweiligen empirischen Forschungsfrage darstellt. Kapitel 2 verwendet ein *Regression Discontinuity Design*, um den Effekt einer Rentenkürzung auf das Arbeitsangebot anhand von Rentendaten niedrigqualifizierter Deutscher zu bestimmen. Kapitel 3 bis 5 wenden sich Fragen in Bezug auf Bildung und Arbeitsangebot in Entwicklungsländern zu.

Um den Effekt starker Rentenkürzungen auf das Arbeitsangebot zu schätzen, nutzt die Analyse in Kapitel 2 drei Natürliche Experimente aus, bei denen solche Rentenkürzungen eine Gruppe überwiegend geringqualifizierter deutscher Spätaussiedler betraf. In zwei dieser Natürlichen Expermiente wurde die Rente um 8 bis 16 Prozent gekürzt, jedoch zeigen die *Regression Discontinuity*-Schätzungen keine statistisch signifikante Verzögerung des Renteneintritts. Dieses Ergebnis kann nicht dadurch erklärt werden, dass die Rentenkürzung eine selektive Erhöhung Mortalität verursacht hätte. In einem dritten Natürlichen Experiment hatten Arbeitnehmer starke Anreize, einer Rentenkürzung durch vorgezogenen Renteneintritt auszuweichen. Die Analyse zeigt jedoch, dass es auch hier keinen signifikanten Effekt auf den Renteneintrittszeitpunkt gab. In der Gesamtbetrachtung sind diese Ergebnisse konsistent mit der Interpretation, dass es für niedrigqualifizierte ältere Deutsche angesichts eines im Verhältnis zum Rentenniveau relativ geringen Marktlohnes optimal ist, so früh, wie es institutionelle Rahmenbedingungen erlauben, in Rente zu gehen.

Kapitel 3 wendet sich der Analyse von Daten aus einem Entwicklungsland zu. Es beschreibt eine Haushaltsbefragung, in die die gesamte Bevölkerung einer togoischen Gemeinde in den Jahren 2008 und 2011 einbezogen wurde, wobei individuelle Beobachtungen zwischen den beiden Befragungswellen verknüpft wurden. Die hauptsächliche Motivation für die Erhebung dieser Daten war es, die Auswirkungen eines Vorschulprojektes evaluieren zu können, welches zwischen den beiden Erhebungswellen in der untersuchten Gemeinde begann. Ein Teil dieser Evaluation, der sich mit den kurzfristigen Effekten auf das Arbeitsangebot von Frauen befasst, wird in Kapitel 4 dargestellt.

In Kapitel 3 werden die Besonderheiten in der Durchführung der Haushaltsbefragung diskutiert und einzelne Befragungsmodule motiviert und erläutert. Ein Schwerpunkt liegt hierbei auf den Alleinstellungsmerkmalen des entstandenen Datensatzes, insbesondere hinsichtlich der gewonnen Maße für Zeitnutzung, Einkommensquellen und kongitive Fähigkeiten der Befragten. Zwar war die Wahl der Befragungsinhalte getrieben durch den Zweck, das Vorschulprojekt zu evaluieren, doch das Kapitel verdeutlicht auch die inhaltliche Bandbreite der entstandenen Haushaltsdaten, die entsprechend Möglichkeiten für weiterführende Forschung bietet.

Auf Basis der in Kapitel 3 beschriebenen Daten behandelt Kapitel 4 die Evaluation des Vorschulprojekts, das in der untersuchten Gemeinde im Jahr 2010 begann. Zunächst wird in einer detaillierten deskreptiven Analyse gezeigt, dass (an Stelle alternativer Maße der Betreuungssituation) insbesondere ein Indikator dafür, ob *alle* Kinder unter sechs Jahren einer Mutter (d.h. Kinder, die noch nicht im Grundschulalter sind) institutionell betreut werden, relevant ist für die Stundenzahl, die eine Mutter mit Kinderbetreuung verbringt. Zugleich zeigt sich, dass eine solche vollständige institutionelle Betreuung junger Kinder nur sehr schwach mit der Arbeitszeit der betroffenen Mütter zusammenhängt.

Anschließend wird untersucht, ob dieses Ergebnis Bestand hat, wenn die empirische Analyse die Probleme statistischer Endogenität stärker berücksichtigt. Hierfür werden die Haushaltsdaten zusammengeführt mit Informationen über ein Soziales Experiment, durch welches die Zulassung zur neu in der untersuchten Gemeinde errichteten Vorschule per Zufall bestimmt wurde. Zwar gab es im beobachteten Einschulungsverhalten starke Abweichungen von diesem randomisierten Zulassungsprozess, dennoch erzeugte für Mütter von Kindern, die für die Zulassung zur ersten Vorschulklasse angemeldet wurden, das Soziale Experiment Variation in der Wahrscheinlichkeit, dass alle jungen Kinder einer Mutter institutionell betreut werden. Unter Ausnutzung dieser Variation wird der Effekt einer solchen vollständigen institutionellen Betreuung auf die Zeitnutzung der Mütter in einem Instrumental-Variablen-Modell geschätzt. Zwar sind die geschätzten Effekte insbesondere auf Grund der geringen Stichprobengröße nicht statistisch signifikant, doch die Konsistenz der geschätzten Koeffizienten über viele verschiedene Spezifikationen und Modellvariationen hinweg legt eine Bestätigung des vorangehend deskriptiv gewonnen Ergebnisses nahe: insitutionelle Betreuung von Vorschulkindern verringert die Betreuungsbelastung der Mütter, hierdurch steigt ihre Arbeitszeit aber nicht in einem vergleichbaren Maß.

Kapitel 5 wendet sich einem besonderen Phänomen in Grundschulen in Entwicklunsländern zu, das sich auch in der untersuchten togoischen Gemeinde wiederspiegelt: 20 Prozent der Schüler aller Grundschulklassen wiederholen in einem laufenden Schuljahr eine Jahrgangsstufe. Das Kapitel befasst sich mit den Konsequenzen einer so hohen Zahl von Nicht-Versetzungen. Insbesondere geht es um die Frage, ob hierdurch *Peer Effekte* erzeugt werden, dass also Mitschüler eine schlechtere schulische Leistung erbringen, wenn sie gemeinsam mit vielen Wiederholern unterrichtet werden. Die für diese Analyse verwendeten Daten basieren auf den Aufzeichnungen der Grundschulen zu den Trimester-Punktezahlen der Grundschüler aller vier Grundschulen der Gemeinde. Anhand dieser Daten ließ sich feststellen, dass es hinsichtlich des Anteils wiederholender Schüler eine große Variation auch innerhalb von Schulen gibt. Vor dem Hintergrund der stark durch eine leistungsbasierte Regel geprägten Versetzungsentscheidung lässt sich transparent darstellen, welche Faktoren diese Variation im Wiederholer-Anteil einer Klasse erzeugen können. Darauf aufbauend wird argumentiert, dass die empirischen Modelle, die für Schul-*Fixed Effects* sowie für vorangehende individuelle Punktezahlen kontrollieren, den *Peer Effekt* des Wiederholer-Anteils identifizieren. Die Ergebnisse zeigen, dass die Erhöhung des Wiederholer-Anteils in einer Klasse um eine Standardabweichung die individuelle Punktezahl von Schülern um durchschnittlich 13 Prozent einer Standardabweichung verringert.

Schlagwörter: Arbeitsökonomik, Bildungsökonomik, Entwicklungsökonomik

Abstract

This thesis covers diverse topics in the fields of labor economics, education economics and development economics which share an emphasis on solving issues of statistical endogeneity associated with the respective empirical research questions. While the analysis in chapter 2 uses a regression discontinuity design to study the impact of pension reductions on labor supply using data from low-skilled workers in Germany, chapters 3 through 5 turn to issues of education and labor supply in developing countries.

To estimate the effects of large cuts in pensions on labor force participation, the analysis in chapter 2 exploits three natural experiments in which such cuts affected a group of mostly low-skilled repatriated ethnic German workers. In two of these natural experiments, the pension rate was cut by between 8 and 16 percent, yet, according to regression discontinuity estimates, there was no significant delay in retirement. The results cannot be explained by selection bias due to increased mortality in response to the reforms. In the third natural experiment, the workers were given an incentive to avoid a pension cut by retiring earlier, but the analysis demonstrates that there was no significant effect for earlier retirement. All these results are consistent with low-skilled workers in Germany being frozen in a corner-solution equilibrium in which the optimal choice is to retire as early as possible.

Chapter 3 turns to the analysis of data from developing countries. It describes a unique household survey of the full population of a Togolese community conducted in 2008 and in 2011, linking individual observations from both survey waves. The main motivation for collecting the data was to evaluate the impact of a preschool program which started in the studied community in between the survey waves. Part of that evaluation, regarding the short-run impact on female labor supply, is discussed in chapter 4. Chapter 3 discusses particularities of the survey implementation, and it motivates and explains the survey's modules. Emphasis is laid on the uniqueness and quality of its sections concerning the measurement of time use, income sources, and cognitive skills. While the choice of topics covered by the survey has been dictated largely by its original purpose of allowing the evaluation of a preschool project, the chapter illustrates the richness of the data, leaving opportunities for future research.

Making use of the data described in chapter 3, chapter 4 discusses the evaluation

of the preschool program which started in the studied community in 2010. First, in a thorough descriptive analysis, it is shown that among measures of enrollment, an indicator for whether a mother's children younger than six years are *all* enrolled in either preschool or primary school (which I call "full enrollment") is particularly relevant to the number of hours these women spend with child care. Apparently, child care responsibilities are high on average as soon as at least one young child remains at home, and conditional on at least one child staying at home, other variables capturing the number of children and their enrollment status are not associated with the mother's time use. Furthermore, the descriptive analysis shows that the strong relationship between full enrollment and hours of child care is not mirrored by a comparable association between full enrollment and hours of work.

In order to substantiate this result, endogeneity concerns need to be taken into account. Accordingly, household data are combined with information on a social experiment, where the access to a newly constructed preschool in the studied community had been randomized. Even though compliance with randomization was poor, having a child admitted to the first grade of preschool significantly increases a mother's likelihood of full enrollment. This relationship is, apparently, partly due to the randomization affecting *primary* school enrollment. In the studied community, primary schools accept some children who would be considered too young in light of the regular school entry age, so they constitute an alternative institution providing daycare for children. Exploiting the variation in full enrollment induced by the randomization of access to first grade of preschool, I estimate instrumental variables models of time use. Although, due to the small sample size, estimated effects are generally not statistically significant, results from a wide range of different model specifications suggest a clear pattern: full enrollment reduces the time mothers spend caring for young children by at least three to four hours. However, confirming the descriptive results, this effect is not accompanied by any noticeable change in time use related to work. Since this result is in line with OLS results for the full population of mothers of young children (including those not participating in the preschool admission procedure), I argue that the lack of a response in labor supply cannot be attributed solely to the short time frame of the evaluation or to the selectivity of the sample of participants. A full evaluation of the benefits of an expansion of publicly provided preschool education in developing countries would, however, have to take into account the direct benefits of such programs on participating children.

As described in chapter 4, primary schools in the studied community accept some children of preschool age. This leads to strong heterogeneity of students in the first grades of primary school with respect to age and, consequently, school readiness. In fact, this may be one of the reasons for so many primary school students failing test score targets, and, accordingly, having to repeat a grade: on average, in the community's primary schools, more then 20 percent of students in each class are retained instead of being promoted to the next grade. Chapter 5 investigates the consequences of such a high incidence of grade repetitions. More specifically, it addresses the potential peer effects which may result from being exposed to many repeaters.

The data used to estimate the peer effects of grade retention in chapter 5 are administrative records of individual student trimester exam scores obtained from all four primary schools in the studied community. They reveal that there is very strong within-school variation in the share of repeating classmates. Given that within schools, there is only one class per grade in each school year, and in light of a merit-based retention rule which explains a large share of repetitions, the sources of variation in the share of repeaters per class are rather transparent. The identifying assumption for the models estimated in this chapter is that the share of repeaters per class is exogenous in regression models of individual exam scores in third trimester when controlling for first trimester scores, school dummies, and individual repeater status. The results indicate that a one standard deviation increase in the share of repeaters per class reduces individual test scores by 13 percent of a standard deviation. As various modifications of the model specification demonstrate, the result is robust to changes in the source of variation in the share of repeaters per class.

Key words: Labour economics, economics of education, development economics

Contents

Chapter 1

Introduction

Much of the empirical research in labor economics centers on solving issues of statistical endogeneity. The types of problems that occur (e.g. unobserved heterogeneity) and the methods employed to solve them are rather universal. The diversity of topics covered in this thesis illustrates this notion. While the analysis in chapter 2 uses a regression discontinuity design to study the impact of pension reductions on labor supply using data from low-skilled workers in Germany, chapters 3 through 5 turn to issues of education and labor supply in developing countries.

To estimate the effects of large cuts in pensions on labor force participation, the analysis in chapter 2 exploits three natural experiments in which such cuts affected a group of mostly low-skilled repatriated ethnic German workers. In two of these natural experiments, the pension rate was cut by between 8 and 16 percent, yet, according to regression discontinuity estimates, there was no significant delay in retirement. The results cannot be explained by selection bias due to increased mortality in response to the reforms. In the third natural experiment, the workers were given an incentive to avoid a pension cut by retiring earlier, but the analysis demonstrates that there was no significant effect for earlier retirement. All these results are consistent with low-skilled workers in Germany being frozen in a corner-solution equilibrium in which the optimal choice is to retire as early as possible.

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The data used to estimate the peer effects of grade retention in chapter 5 are administrative records of individual student trimester exam scores obtained from all four primary schools in the studied community. They reveal that there is very strong within-school variation in the share of repeating classmates. Given that within schools, there is only one class per grade in each school year, and in light of a merit-based retention rule which explains a large share of repetitions, the sources of variation in the share of repeaters per class are rather transparent. The identifying assumption for the models estimated in this chapter is that the share of repeaters per class is exogenous in regression models of individual exam scores in third trimester when controlling for first trimester scores, school dummies, and individual repeater status. The results indicate that a one standard deviation increase in the share of repeaters per class reduces individual test scores by 13 percent of a standard deviation. As various modifications of the model specification demonstrate, the result is robust to changes in the source of variation in the share of repeaters per class.

Chapter 2

Labor supply effects of changes in pensions

- Regression discontinuity evidence from low-skilled workers[1]

2.1 Introduction

Pension systems are currently becoming less generous in many industrialized countries, a reform that could be expected to have positive effects on the labor supply, notably for older workers, especially in the presence of myopic savings behavior, liquidity constraints, or unexpected pension cuts (cf. Card et al. (2007)). These potential labor supply effects should in turn induce important fiscal effects by increasing tax and social security revenues and decreasing pension fund payouts. The size of these effects, however, depends on the labor supply elasticity of mostly older workers, a factor that is hard to determine empirically because of the rarity of exogenous shocks to budget constraints (wages, pension rights).

In this paper, we use administrative data from the German pension register to estimate labor supply and mortality reactions to a series of large pension cuts for repatriated ethnic German men, which occurred in the 1990s and can be regarded as natural experiments. This group of older repatriated ethnic Germans resembles low-skilled workers in Germany in that 54 percent of the males aged 55-65 work in blue-collar jobs compared to

[1]This chapter is co-authored with Patrick Puhani. Part of this research was supported by the German Research Foundation (DFG) within the project 'Labour Market Effects of Social Policy', which is part of the research initiative 'Flexibility in Heterogeneous Labour Markets'. The chapter was presented at conferences and seminars at CLE, UC Berkeley; at IZA, Bonn; the German Economic Association, the European Association of Labour Economists; the European Economic Association; at the Universities of Frankfurt and Nuremberg; and at the research initiative's IAB meeting in Nuremberg.

56 percent of low-skilled (i.e. without vocational training/apprenticeship) male German workers aged 55-65. These numbers differ significantly from the 29 percent figure for all German workers in that age group (see Table 2.10 in the appendix for occupational distributions and dissimilarity indices).[2] In addition, these older low-skilled workers constitute a sizable enough group that they should interest policy makers: 35 percent of adult men and 39 percent of adult women are over 55, and 12 percent of men and 27 percent of women aged 55-65 are low skilled, as defined by not even having completed an apprenticeship (author calculations based on the German Microcensus 2005).

Low-skilled workers are a key target group of labor market and social policies, because they face low wages so that they either risk belonging to the working poor or - as in many European countries - have limited incentives to work because their potential earnings hardly exceed social benefits.

Our study demonstrates this situation for older low-skilled workers in a continental European economy. More specifically, the three natural experiments investigated provide two types of incentives: The first two experiments reduced pension rates and hence increased the price of leisure, meaning that we would expect workers to retire later. The third provided incentives for early retirement to avoid a pension cut. Because the first set of cuts was based on the repatriation date and enacted retrospectively, it can be analyzed using regression discontinuity designs. However, we find no significant effects of these reforms on labor supply and estimate an upper bound for the extensive life-time Marshallian labor supply elasticity of 0.07 for men. Based on the third experiment, we also observe that workers do not react to incentives for early retirement to avoid a pension cut and thus argue that low-skilled German workers are already retiring as young as is feasible according to administrative rules. We therefore conclude that low-skilled men are bogged down in a "corner solution" made up of incentives to retire as early as possible.

The German case investigated here can be seen as an example of how some European

[2] Many repatriated ethnic Germans might have been regarded as skilled in their source countries, but differences in production methods and working cultures and lack of recognition of educational degrees from former socialist countries, combined with language problems, devalued much of their human capital. The attachment of the repatriated ethnic Germans to German culture varied considerably, with some people still speaking German at home, whereas others spoke no German at all so that some repatriated ethnic Germans were seen as "foreign" immigrants by some German observers. According to Bauer and Zimmermann (1997), p. 365, between 41 and 53 percent of ethnic Germans arriving between 1989 and 1993 were enrolled in German language courses.

welfare systems provide few labor supply incentives/opportunities for older low-skilled workers. Given the high replacement rates of state pensions in many European countries, our study therefore exemplifies how generous social security benefits can impact the labor supply.[3]

Ours is one of the few causal studies on the effects of pension rights reduction on retirement behavior, an analysis made possible by the fact that two of the pension cuts we consider were enacted retrospectively for repatriated ethnic Germans after immigration into Germany, the cuts being dependent on the immigration date. Krueger and Pischke (1992) analyze a pension cut of similar size as that in our study by exploiting the 1977 amendments to the Social Security Act in the United States. Over a transitional period of a few years, these legislative changes gradually decreased the average social security pensions by about 13 percent for the 1920 birth cohort compared to those of the 1916 birth cohort. Yet the authors find no effect for this large cut on retirement behavior and conclude that the continuing downward trend in male labor force participation in the United States cannot be explained solely by increasing social security benefits, even though these variables are negatively correlated over a long time period in the post-war era.

The two factors used by Krueger and Pischke (1992) to explain their findings - private pensions or private wealth substituting for pensions - cannot explain the absence of any pension cut effect on retirement age in our study. Having returned to Germany from former socialist countries, the repatriates we analyzed can safely be expected to have had almost no private wealth or company pension. Rather, their alternative income sources are their spouse's pension/earnings or support from their children, who under German law at the time were obligated to care for parents who are not eligible to draw social security benefits.[4]

[3] It is also worth noting that Germany relies mostly on a mandatory pension system for all employees (except many self-employed workers and civil servants), with company pension plans acting only as supplements.

[4] Although a social welfare program for the elderly was in place when the individuals in our sample retired, the Federal Statistical Office and the German Parliament report that take-up rates were generally low because many elderly shied away from asking their children - who were required by law to support parents in need - to disclose their financial situations (Bundesamt (2008); Bundestag (2001); für Sozialforschung und Gesellschaftspolitik (1999) also reports that the share of pensioners overall who received social welfare in 1997 was only 1.3 percent and for repatriated ethnic Germans who migrated before 1993, that figure was as low as 3.3 percent (für Sozialforschung und Gesellschaftspolitik (2002)). There is no separate figure for repatriated ethnic Germans who are pensioners. However, it is

Yet, even though the actual pensions received were 8-16 percent lower for the treatment than for the control group, we find no significant effects of pension cuts on retirement age. Such comparative large falls in income from an already low level raise the question how they affected the health and life expectancy outcomes of these low-income pensioners. However, for the pension cuts we investigate here, we find no increase in mortality (the only health indicator we observe in our data).

A similarly sized pension cut in a natural experiment in Russia (the Russian pension crisis) produced an effect in terms of increased mortality Jensen and Richter (2004); however, this latter finding contrasts with Snyder and Evans (2006) report of positive health results from working longer. The difference between these results may be explained by the fact that Germany like most European countries (and the U.S. for elderly people) has universal health care coverage and the natural experiments in Germany and in the U.S. did not reduce anybody's pension payment to zero, which is what happened in Russia.

To sum up, given our low labor supply elasticity estimates, we conclude that significant changes in the pension rate for low-skilled workers in either direction seem mostly to have redistributive consequences, without any significant changes in the labor supply or mortality of the affected workers.

The paper is structured as follows: Section 2.2 sketches the retirement system in Germany, as well as the pension situation for repatriated ethnic Germans. Section 2.3 describes the reforms of the repatriates' pension rights as well as the data source, Section 5.4 presents the results, and Section 2.5 concludes the paper.

important to note that when the individuals in our sample retired, eligibility determination for social security took into account both spouse's and children's income, as well as other income sources. Hence although pensions for people in our sample were generally low, high labor force participation rates for both men and women in former socialist countries generally resulted in "family pensions" above the subsistence level. Based on these observations, the pension cuts analyzed here could not simply have been cushioned by higher social welfare receipt.

2.2 Institutional background: the German public pension system and special rights for repatriated ethnic Germans

2.2.1 Retirement in the German public pension system

Before explaining the particular rules pertaining to repatriated ethnic Germans, we briefly sketch the key features of the German pension system. For the cohorts we study, the system was characterized by a comparatively high pension rate and considerable flexibility concerning the age of retirement, with built-in incentives to retire early. In Germany, the most important component of income in old age is the mandatory 'public pension insurance', which covers about 85 percent of workers (generally excluding civil servants, who have a separate pension system, and self-employed workers, who are mostly voluntarily self-insured; Berkel and Börsch-Supan (2004). By international comparison, this system is characterized by a high replacement rate of about 70 percent (according to Borsch-Supan (2000), p. F29; only 58 percent according to Boeri and van Ours (2008), p. 123), meaning that public pension benefits constitute by far the most important source of income for elderly Germans (over 80 percent of income for households headed by persons over 64 years of age; Borsch-Supan (2000).

Although the statutory retirement age for men in Germany during our observation period was 65, under certain preconditions, some workers could receive public pension payments earlier, most notably at ages 63, 60 or even earlier. For instance, any individual whose employment history (as far as relevant to the pension insurance) exceeded 35 years could retire flexibly between the ages of 63 and 65. Several other arrangements also allowed workers to receive pension payments as early as age 60; most particularly, the so-called 'reduced earnings capacity' of, for example, workers who were administratively classified as not being 'appropriately employable' because of 'health or labor market reasons'. The eligibility criteria for such pensions were also met if no vacancies were available at the labor office for the worker's specific job description and changing to a different job type would cost the worker an earnings loss of at least 50 percent.[5] Dur-

[5]More precisely, when these criteria were met, an individual would not necessarily receive a full pension but could be awarded a reduced pension. However, during the 1990s, the underlying rules

ing the 1990s (our observation period), these rules were interpreted liberally enough that Berkel and Börsch-Supan (2004) term them 'soft eligibility rules'. As a result, Arnds and Bonin (2002) suggest that at that time, many individuals had at least some discretionary power to retire as early as 60.[6] In fact, under these same rules, male workers could receive a pension due to 'reduced earnings capacity' even before reaching 60 as long as they had contributed to the insurance system for at least five years and had worked three out of the last five years Riphahn (1997). Women, on the other, hand, could generally retire at age 60 provided they had worked for at least 10 years since age 40.

None of the above retirement schemes, however, were accompanied by an actuarial adjustment of the monthly pension benefit (Arnds and Bonin (2002)). Rather, in the case of early retirement, pension benefits were lowered only because during the years remaining until the regular retirement age of 65, the individual accumulated no additional pension rights. No additional actuarial adjustment was made, however, to take into account the fact that by retiring earlier, individuals increased their expected duration of pension receipt, which, in turn, increased social security wealth (i.e. the expected present value of cumulative pension payments). The lack of any such adjustment created very strong incentives for early retirement, and empirical studies of retirement over time suggest that actual retirement behavior was strongly influenced by changes in these incentives (Borsch-Supan and Schnabel (1998); Borsch-Supan (2000)).

2.2.2 Repatriated ethnic Germans

In this study, we evaluate a population that was affected by large cuts in pension rights; namely, repatriated ethnic Germans (Aussiedler). Germany, like Israel, has a 'right-of-return' law that allows ethnic Germans to settle in the Federal Republic of Germany as German citizens immediately after arrival. More specifically, according to the German constitution, both citizens and refugee ethnic Germans are 'Germans'.

were interpreted so generously that most workers who retired because of 'reduced earnings capacity' received a full pension. Another pathway to early retirement was a worker's having been unemployed for at least one year out of the previous 1.5 years after having contributed payments to the pension insurance for at least 8 out of the previous 10 years. Individuals could also retire at any age in case of severe disability.

[6] Other possibilities for retiring before age 60 were by way of so-called 'partial retirement plans' or the disability retirement allowable at any age for sufficiently severe disabilities.

Ethnic German immigration into (West) Germany from 1950 to 2005 amounted to about 4.5 million people (Wikipedia), about 5.5 percent of Germany's total population, many of whom came from the former Soviet Union (2.3 million), Poland (1.4 million), and Romania (0.4 million). Although the criteria for who is an ethnic German and who may immigrate as a citizen have recently been made stricter, the laxer rules in place during the cold-war period were still in effect during our observation period of the 1990s.[7]

Yet, despite these generous immigration and naturalization laws for ethnic Germans, initially, few ethnic Germans settled in West Germany because the Iron Curtain prevented them from exercising their right to West German citizenship. This situation changed radically in the late 1980s, however, when the law finally felt the effect of the Iron Curtain's fall.[8]

2.2.3 Pensions for repatriated ethnic Germans

Given that many repatriated ethnic Germans spent large parts of their working lives outside of Germany without paying contributions to the German public pension insurance, an Alien Pension Law (Fremdrentengesetz, FRG) was legislated in West Germany in 1959. Under this law, the pension system acknowledged the period of employment

[7]The main motivation for the right-of-return law enacted in the (West) German constitution were considerable settlements of people of German decent in central and eastern Europe, as well as in other territories of the former Soviet Union. Many of these settlements have in one way or another remained German in culture and even language. The historical reasons for settlement include Russia's invitation in the 18th century for Germans to settle on its territory and the fact that in the 19th century, large parts of central Europe were part of either Germany or Austria-Hungary, which produced pockets of German-speaking settlements all over central Europe (see Figure C1 for a map). Although many ethnic Germans were forced to leave these territories after the Second World War, some remained for personal reasons like intermarriage or simply because the countries 'forgot' to expel them, meaning that German minorities remained in the Soviet Union, Poland, Romania, and several other countries. Because the Federal Republic of Germany felt that these minorities were disadvantaged in their countries of residence by their German ethnicity, it granted them the right to settle in Germany and become citizens immediately upon arrival. The Federal Republic of Germany, unlike the Republic of Austria, also assumed responsibility for ethnic Germans from former territories of Austria-Hungary. As a result, ethnic Germans from Romania could become citizens of modern-day Germany but not of modern-day Austria, and all East German residents were likewise regarded as West German citizens.

[8]After the initial heavy population movement immediately after World War II, ethnic Germans moved to Germany in relatively small numbers, but with the collapse of the Soviet Union, their number quickly increased again, with 1.5 million immigrating from 1989 to 1993 (see Figure C2). As a result, during the 1990s, German legislation gradually changed until the criteria for approval as an ethnic German refugee became stricter and fewer potentially ethnic German migrants set out for Germany (Bauer and Zimmermann, 1997). For example, although ethnic Germans living abroad can still migrate to Germany, they must now pass a language test. The population studied here, however, immigrated to Germany before 1997 and is hence part of the large influx of the early 1990s.

in the previous country of residence (e.g. Soviet Union) exactly as if the individual had worked in the same occupation in West Germany. Based on this recognition, it granted repatriated ethnic Germans generous pension rights. Hence, an ethnic German coming to Germany at age 65 after having worked in the Soviet Union for 40 years could go straight into retirement and receive a full pension just like a German-born individual who had worked in Germany for 40 years in the same type of job. Retirement earlier than 65 (i.e. at age 63, 60, or earlier) was similarly possible, because the same rules applied to repatriated ethnic Germans as applied to native Germans: time worked in the source country (e.g. Soviet Union) counted just like time worked in Germany for application of the rules outlined in Section 2.2.1.

After the fall of the Iron Curtain, however, this rule led to a significant drain on the pension system because repatriated ethnic Germans (and East Germans) could receive pensions without ever having paid into the system. The outcome was a series of reforms cutting these repatriated ethnic Germans' pensions. For instance, for most of the immigration cohorts in our study, although all years worked in the source countries (e.g. Soviet Union) still counted as active work, the pension level was calculated based on East German rather than West German pay scales.[9]

In the German public pension system, pension rights are usually based on the contributions made by employees over their working life, which are translated into so-called 'earnings points' that reflect the employee's earnings position relative to other workers in the economy. One earnings point corresponds to the average earnings in the economy in a given calendar year. Therefore, depending on individual earnings in any given year, the individual may gain more or less than one earnings point per calendar year, depending on his or her position in the wage distribution. The pension level is calculated based on the total number of earnings points collected. The reforms we investigate reduced pensions by cutting the number of earnings points obtained by a repatriated German through previous employment in the original country of residence (hereafter,

[9]First, repatriated ethnic Germans are assigned to a 'qualification group' according to a supplement to the German social security law that classifies the educational attainment of repatriated ethnic Germans into five categories. The worker's former job is then allocated to one of 23 industries. To simulate the earnings points for the pension rights, each qualification-industry combination has a hypothetical income assigned for each calendar year since 1950. The data used for analysis, however, do not include information on the qualifications and industries used for this simulation, only the number of earnings points accumulated by each individual.

source country).

Having immigrated mostly at a relatively high age (55 and older), the repatriated ethnic Germans studied in this paper spent most of their working lives outside Germany and their pension rights, rather than being based on actual contributions to the system, were mostly calculated by type and length of employment in the source country. The reforms we investigate that involve cuts in these pension rights, therefore, translate into large reductions of the repatriated immigrants' total pension rights.

The pension level is not, however, a linear function of the earnings points, which explains why the pension cuts observed in the data are smaller than the original cuts in earnings points. That is, after being cut according to the described legislative changes, the earnings points earned before 1993 (the date after which this rule was repealed) were increased again so as not to fall below a certain threshold. In other words, the German public pension insurance 'beefed up' low pension levels by raising part of an individual's pension by up to 50 percent.[10]

2.3 Pension reforms for repatriated ethnic Germans during the 1990s and corresponding administrative data

During the 1990s, repatriated ethnic Germans effectively faced several cuts in the pension rights they had accumulated outside Germany. In order to exploit these pension reforms as natural experiments that allow estimation of low-skilled workers' reactions to unexpected cuts in pension benefits, we first briefly describe both the reforms and the corresponding administrative data. More detailed descriptions of the reforms are provided in German by both Polster (1990), Polster (1992), Polster (1997), and Heller (1997).

Our administrative data are taken from the Federal German Pension Insurance (Deutsche Rentenversicherung Bund, DRV-Bund), the mandatory state pension system for most German workers, which began providing access to a sample of its administrative

[10] This rule, which increased only the part of a pension based on social security-relevant activities (primarily, dependent employment) before the year 1993, has not yet been replaced by any other variant of a minimum pension for pension earnings points gained after 1993.

data in 2005. We obtained remote access to the complete population of pension data on repatriated ethnic Germans for the calendar year 2008, the only year for which 'date of immigration' (accurate to the day) - a necessary variable for our regression discontinuity analyses - was available. We base our analysis on the full population of ethnic German immigrant birth cohorts covered in the data set.

These administrative data provide personal information on the entire population of repatriated ethnic Germans who retired before 2008 and were still alive in 2008, including pension level in euros, year and month of retirement, individual's age, date of immigration into Germany, and source country. Unfortunately, however, they include no additional socioeconomic characteristics. We must also exclude from the sample repatriated ethnic Germans who immigrated from Poland because a special regulation prevented them from being affected by any of the subsequent reforms.[11]

Natural Experiment 1: On July 25, 1991, earnings points acquired abroad (and used to calculate the pension level) were cut by 30 percent for all repatriated ethnic Germans who had immigrated after December 31, 1990 (according to Renten-Überleitungsgesetz, RÜG, Art. 14,20a and Art. 15). Due to the nonlinear relationship between earnings points and pensions, actual pensions were reduced by about 8 to 11 percent, only a little less than the 13 percent in the reform analyzed by Krueger and Pischke (1992). Because the legislation was passed after the date of immigration, it amounted to an ex-post reduction in pension rights. Hence, the effect of the reform can be evaluated using a regression discontinuity design that compares the retirement behavior of immigrants arriving shortly before and after December 31 1990. Because the 1st of January is often a date when new laws or regulations are implemented, we checked whether there were any other rule changes affecting the budget constraint of immigrants arriving after that date: we found no such changes.

The oldest cohorts in our estimation samples are individuals who turned 60 in 1992 (and were thus 76 years of age when observed in 2008) and were not yet retirement age when the reform was implemented (i.e. cohorts born on or after January 1 1932). The youngest cohorts are individuals born in March 1936 (who were 72 years of age in 2008)

[11]We initially considered using immigrants from Poland as a control group in a difference-in-differences identification strategy; however, the number of people immigrating from Poland was at a very low level from 1991 onwards (see Figure C2) so that the sample sizes for the cohorts we consider are too small.

because the retirement behavior of cohorts younger than these were potentially affected by the Natural Experiment 3 irrespective of their immigration date.

As part of the regression discontinuity design implementation, we use the immigration date to define a sample that is a subset of the population of repatriated ethnic Germans in these birth cohorts. This subset is restricted to individuals who immigrated between July 1990 and June 1991. Those who immigrated between January and June 1991 comprise the treatment group and those who immigrated between July and December 1990 make up the control group. Two additional discontinuity samples use 'tighter' immigration date windows around the cutoff: workers that immigrated between October 1990 and March 1991 (a 6-month window) and those who immigrated in December 1990 or January 1991 (a 2-month window). Although our administrative data contain the population of repatriated ethnic German pensioners, the sample restrictions by cohort and immigration date leave us with sample sizes of 2,554, 1,083, and 348 for the three regression discontinuity samples, respectively (see Table 2.12 and Table 2.13 in the appendix; the figures for women are 3,405, 1,479 and 482, respectively, see Table 2.15). We do, however, have to rely on the regression discontinuity design as an identification strategy for lack of sufficient socio-economic control variables in the administrative data. Tobit estimates will take account of the censoring.

When evaluating Natural Experiment 1, we also censor the retirement date relative to April 30 1996 because after the announcement of the reform associated with Natural Experiment 3, strategic behavior may have occurred to avoid it. Thus, in an attempt to isolate the effects of Natural Experiment 1, we censor retirement date observations for all individuals who had not yet retired by the end of April 1996, which results in the censoring of about 20 percent of our estimation sample's retirement ages.

Natural Experiment 2: On September 25 1996, an upper bound for earnings points (acquired abroad) was introduced for all repatriated ethnic Germans who immigrated after May 6 1996 (according to the Wachstums- und Beschäftigungsförderungsgesetz, WFG, Art. 3 and Art. 4, September 25 1996). The limit was 25 earnings points, which, as shown below, effectively amounted to a reduction in actual pensions of between 10 and 16 percent, similarly to the reform analyzed by Krueger and Pischke (1992).[12] The

[12] The limit of 25 earnings points applied to singles, the calculation for married couples was more complicated.

causal effect of the cut in pension rights on retirement behavior can thus be derived using a regression discontinuity design as long as those immigrating just before versus just after the cutoff date do not differ systematically on other characteristics.

Natural Experiment 2 was generated by the same law as Natural Experiment 3 (see below). In order to separate the effects of these two regulation changes, we consider only men who turned 60 in 1997 or later - that is, the cohorts born on or after January 1 1937. We thereby minimize the number of individuals who could strategically retire and avoid the reform associated with Natural Experiment 3. The youngest cohorts are those born in December 1941 because individuals in all succeeding birth cohorts might not yet have retired by 2008.[13] For this analysis, the discontinuity samples consist of 12-month (immigrated between November 6 1995, and November 6 1996), 6-month (immigrated between February 6 1996 and August 6 1996), and 2-month (immigrated between April 6 and June 6 1996) sampling windows (with May 6 1996 as the cutoff date). Table A1 and Table A4 show how this leaves us with 1,902, 849 and 319 observations for men in our regression discontinuity samples. The corresponding numbers for women are 2,687, 1,191, and 474, respectively (Table A5).

Natural Experiment 3: This reform, generated by the same law as Natural Experiment 3 (the Wachstums- und Beschäftigungsförderungsgesetz, WFG, Art. 3 and Art. 4), cut earnings points acquired in the source country by 40 percent for all repatriated ethnic Germans retiring after October 1 1996 irrespective of immigration date. Hence, in contrast to Natural Experiments 1 and 2, which provided incentives for later retirement, it provided incentives for men who would normally have retired later (i.e. after October 1996) to retire before that date to avoid the pension cut. We use two different identification strategies to evaluate this natural experiment, described in Section 4.4 below.

2.4 Results

Figure 1 plots the survival estimates for age at retirement for men. As the figure shows, the retirement behavior of repatriated ethnic Germans is similar to that of low-skilled

[13]Extending the sample somewhat by including adjacent birth cohorts changes neither the point estimates nor the standard errors in any relevant way.

Germans (those having below apprenticeship education). Both these groups have a modal retirement age of 60, with the second most common retirement age being 63. In contrast to qualified Germans, very few men in these groups retire at age 65. For women, the corresponding graph is given in Figure A1: both repatriated ethnic German women and low-skilled German women have a modal retirement age of 60, in contrast to skilled German women for whom the modal retirement age is 65. Hence, repatriated ethnic Germans are not only similar to low-skilled workers in terms of their job distributions (as demonstrated in the Introduction), but they are also similar to low-skilled workers in their retirement behavior.

Because our administrative data set only provides information on the formal act of retirement and not on labor supply, we draw additional data from the German Microcensus to gauge how retirement correlates with labor force participation. Using 2005 Microcensus data, we find that among repatriated ethnic German men/women who had immigrated since 1990 and were aged 55 to 65, 88/64 percent (66/52 percent) of men/women not receiving a pension were participating (working) in the labor market (n = 393/434) and 7/13 percent (5/12 percent) of men/women receiving a pension were participating (working) in the labor market (n = 299/407). These numbers are almost identical to those observed for low-skilled workers overall in Germany (n = 1637; 1525).[14][15]

These figures suggest that measuring labor supply based on retirement might overestimate labor supply elasticities, meaning that the labor supply elasticities reported below can be seen as upper bounds on the true elasticities.[16] It seems that for both men and women, the decision to retire is highly, albeit not perfectly, correlated with the decision to stop supplying labor.

[14]For males, these figures are almost exactly identical; for females, however, the labor force participation (employment) rate of low-skilled German women is somewhat lower at 45(39) percent than that of repatriated ethnic Germans at 64(52) percent.

[15]For lack of data, we do not analyze the relationship between unemployment benefit receipt and (early) retirement here. Tatsiramos (2010) shows that low employment rates of older workers in Europe can partly be explained by generous unemployment benefits which often act as a "pathway [in]to early retirement". Similar effects are found by Lalive (2008) for Austria and Kyyrä and Ollikainen (2008) for Finland.

[16]The only information provided in the administrative pension data is whether a pensioner earns more than Euro 400 per month, at which point the pension is reduced. Only 0.7 percent of repatriated ethnic Germans who had retired in the previous three years (as of 2008) had had their pension reduced because they were earning more than Euro 400 through work.

2.4.1 Effect of the two pension cut reforms on age at retirement

Table 2.13 through Table 2.17 in the appendix list the population means for men and women for the first two natural experiments that consist of ex post pension cuts for people having immigrated after the corresponding cutoff date. Because the administrative data cover so few sociodemographic characteristics, we must rely on the regression discontinuity design to identify the causal effect of pension cuts. The only sociodemographic characteristic that allows assessment of the 'balancing quality' of the regression discontinuity sampling windows is the source country. As Table 2.13 and Table 2.15 demonstrate, for Natural Experiment 1, the samples with a 12-month window for an immigration date around the cutoff point are not well balanced in terms of source country (i.e. the treatment group is more likely to immigrate from the former USSR than from Romania). As soon as we consider a 6-month (or 2-month) sampling window, however, the treatment and control groups are well balanced for this variable. Nevertheless, although we control for source country and immigration date in the estimates reported below, the paucity of socioeconomic information in the administrative data leads us to regard the 6-month sampling window as more reliable for the estimation of causal effects than the 12-month window. By the same token, the 2-month sampling window provides an even more credible regression discontinuity design identification strategy, although the standard error is comparatively large because, compared to the 6-month sampling window, the number of observations is limited.

Table 2.1 and Table 2.2 summarize the effects of the reforms in terms of the effective pension cuts for men and women combined (Table 2.19 in the appendix presents the results for women only), and Table 2.3 and Table 2.4 present the corresponding estimates for age at retirement for this same combined sample (Table 2.20 reports the results for women only).[17]

[17] Figure A4 (Figure A6) plots individual pension levels by date of immigration for men (women) for Natural Experiments 1 (Panel A) and 2 (Panel B). The graphs illustrate the pension cuts, including the pension cap introduced with Natural Experiment 2 (Panel B), but also the significant variation in pension levels both above and below the fitted lines due to the absence of a minimum pension.

In Table 2.3 and Table 2.4, we estimate the following regression discontinuity model:

$$y_i = \alpha + \tau \mathbf{1}(z_i > c_z) + \delta z_i + \beta x_i + \epsilon_i, E[\epsilon_i \mid x_i] = 0 \tag{2.1}$$

where the outcome variable y is the date of retirement (measured by the actual day, although in Germany retirement is only possible on the first day of each month), and z is the date of immigration into Germany (measured by the day). $\mathbf{1}()$ is the indicator function, which equals 1 if the individual arrived in Germany after the critical date and is thus affected by the reform (treated), and x is a vector of the few available control variables (date of birth measured by the month and dummy variables for the source country). By including the birth date as a control variable, we effectively estimate the reform's impact on age at retirement.

In Table 2.1 and Table 2.2, we estimate a variant of equation 2.1 in which the dependent variable is the logarithm of the actual pension paid. These estimates are important for identifying the size of the effective pension cut generated by the two reforms. The estimates differ, however, in the sets of control variables used. Whereas Model 1 includes only the date of birth and the source country as controls, Model 2 adds in the date of immigration, with the treatment indicator defined as an additional control. Model 3 then adds in a quadratic term for immigration date that serves as yet another control (see Angrist and Lavy (1999), for an application of this approach in a different context).

The regression discontinuity estimates in Table 2.1 suggest that Natural Experiment 1 reduced the average pension for men by between 8 percent (12- and 6-month sampling windows) and 11 percent (2-month sampling window). The standard errors associated with these estimates are 1, 2 and 4 percentage points, respectively. In order to obtain more precise estimates, we will in the following combine both the samples for men and women and the samples relating to the two natural experiments. In the combined sample for men and women, the point estimates are smaller, ranging between 6 and 10 percent (Table 2.2), probably because for women, there are no statistically significant pension cuts; point estimates are about 4 percent (Table 2.19). This latter might be attributable to the fact that women, although generally exhibiting high labor force

Tab. 2.1: Effective log pension changes caused by Natural Experiments 1 and 2: men.

	Model 1	Model 3	Model 3
Natural Experiment 1 - OLS			
12-month sampling window	-0.07***	-0.08***	-0.08***
n = 2554	(0.01)	(0.01)	(0.01)
6-month sampling window	-0.08***	-0.08***	-0.08***
n = 1083	(0.01)	(0.02)	(0.02)
2-month sampling window	-0.07***	-0.11***	-0.11***
n = 348	(0.02)	(0.04)	(0.04)
Natural Experiment 2 - OLS			
12-month sampling window	-0.21***	-0.11***	-0.11***
n = 2217	(0.01)	(0.03)	(0.03)
6-month sampling window	-0.14***	-0.13***	-0.13***
n = 989	(0.02)	(0.04)	(0.04)
2-month sampling window	-0.13**	-0.18***	-0.18***
n = 369	(0.05)	(0.04)	(0.04)
Both Nat. Experiments pooled - OLS			
12-month sampling window	-0.09***	-0.11***	-0.09***
n = 4456	(0.01)	(0.02)	(0.02)
6-monthsampling window	-0.09***	-0.10***	-0.10***
n = 1932	(0.01)	(0.03)	(0.03)
2-month sampling window	-0.08***	-0.14***	-0.14***
n = 667	(0.03)	(0.03)	(0.03)

Note: Model 1 controls for date of birth and source country, Model 2 also controls for immigration date (discontinuity design estimator), and Model 3 additionally controls for the square of the immigration date (discontinuity design estimator). *Source:* Administrative German pension data; author calculations.

Tab. 2.2: Effective log pension changes caused by Natural Experiments 1 and 2: men and women combined.

	Model 1	Model 3	Model 3
Natural Experiment 1 - OLS			
12-month sampling window	-0.068***	-0.067***	-0.065***
n = 5959	(0.010)	(0.019)	(0.019)
6-month sampling window	-0.075***	-0.060**	-0.058**
n = 2562	(0.013)	(0.027)	(0.027)
2-month sampling window	-0.064***	-0.089*	-0.095*
n = 830	(0.024)	(0.050)	(0.053)
Natural Experiment 2 - OLS			
12-month sampling window	-0.198***	-0.150***	-0.150***
n = 5336	(0.008)	(0.017)	(0.017)
6-month sampling window	-0.161***	-0.158***	-0.157***
n = 2376	(0.013)	(0.024)	(0.024)
2-month sampling window	-0.157***	-0.161***	-0.160***
n = 907	(0.025)	(0.032)	(0.032)
Both Nat. Experiments pooled - OLS			
12-month sampling window	-0.129***	-0.111***	-0.114***
n = 11295	(0.007)	(0.013)	(0.013)
6-monthsampling window	-0.116***	-0.110***	-0.111***
n = 4928	(0.009)	(0.018)	(0.018)
2-month sampling window	-0.106***	-0.140***	-0.134***
n = 1737	(0.017)	(0.028)	(0.028)

Note: Model 1 controls for date of birth and source country, Model 2 also controls for immigration date (discontinuity design estimator), and Model 3 additionally controls for the square of the immigration date (discontinuity design estimator). Source: Administrative German pension data; author calculations.

participation rates in former socialist countries, on average have gathered fewer earnings points than men, meaning that they were more greatly affected by the rule for 'beefing up' low pensions (see Footnote 10). This interpretation is substantiated by Figure A2, Panel A, which shows that only women with higher pensions experienced pension cuts.

For Natural Experiment 2, the regression discontinuity estimates (columns 2 and 3) of the effective pension cut reported in Table 2.1 are somewhat higher than those found for the first, with point estimates for men varying between 11 and 18 percent and those for men and women combined ranging between 15 and 16 percent. The more narrowly we define the sampling window, the larger the discontinuity estimates. The precision of the estimates remains at between 3 and 4 percentage points for men and 2 and 3 percentage points for men and women combined.

To obtain smaller standard errors for our estimates, we pool the data from both pension cut experiments. Doing so produces pension cut discontinuity estimates of between 9 and 14 percent for men, with a standard error of 2 or 3 percentage points, and pension cut discontinuity estimates of between 11 and 14 percent for men and women combined, with a standard error of 1 or 3 percentage points. The largest point estimate is for the 2-month sampling window.

The question remains, however, of how repatriated ethnic Germans reacted to these pension cuts. We report estimates for the reforms' effects on retirement age in Table 2.3 (men only), Table 2.4 (men and women combined), and Table 2.20 in the appendix (women only).[18] The first striking result is that none of the estimates are statistically significant. For Natural Experiment 1, unexpectedly, most point estimates are negative rather than positive. For men, the results based on the 12- and 2-month sampling windows show point estimates close to zero, with the estimated retirement age changing by between -0.06 and 0.03 years (see Models 2 and 3 for the tobit estimates); that is, between -22 and 11 days. The associated standard errors are between 0.15 and 0.37 years (55 and 137 days), respectively, so that the estimated confidence interval is not

[18] In this table, whenever we use data from Experiment 1, we estimate both the OLS and tobit models because individuals in the sample used to evaluate this experiment were also affected by a further pension cut if they decided to retire after September 1996. In order not to confound these two reforms, we censor the date of retirement at April 30 1996 and estimate both OLS models using the censored outcome variable or corresponding tobit models to better take account of the censoring. As Table 2.13 in the appendix shows, about 20 percent of the observations in the sample for Experiment 1 are censored. In the sample used to evaluate Experiment 2, in contrast, no outcome variables are censored.

very narrow around zero for the smallest sampling window. For the 12-month sampling window, the estimated confidence interval for Model 2 has lower and upper bounds of -96 (i.e. $(0.03 - 1.96 \times 0.15) \times 365$) and 118 (i.e. $(0.03 + 1.96 \times 0.15) \times 365$) days, respectively. The point estimate based on the 6-month sampling window is more negative at -0.26 years, with a standard error of 0.22 yielding an estimated confidence interval with lower and upper bounds of -0.70 years (-252 days) and 0.17 years (62 days), respectively. The estimates for Natural Experiment 2 are also insignificantly different from zero.[19]

[19] Individual retirement ages by date of immigration are plotted in Figures A5 and A7 for men and women, respectively.

Tab. 2.3: Effects of pension cuts on retirement age: men.

	Model 1	Model 3	Model 3
Natural Experiment 1 - OLS			
12-month sampling window	-0.06	0.00	-0.05
n = 2554	(0.05)	(0.11)	(0.11)
6-month sampling window	-0.04	-0.21	-0.23
n = 1083	(0.08)	(0.16)	(0.16)
2-month sampling window	-0.09	-0.16	-0.14
n = 348	(0.14)	(0.29)	(0.30)
Natural Experiment 1 - Tobit			
12-month sampling window	-0.12	0.03	-0.03
n = 2554	(0.07)	(0.15)	(0.15)
6-month sampling window	-0.04	-0.26	-0.26
n = 1083	(0.10)	(0.22)	(0.22)
2-month sampling window	-0.12	-0.06	-0.06
n = 348	(0.17)	(0.37)	(0.37)
Natural Experiment 2 - OLS			
12-month sampling window	0.09	-0.09	-0.09
n = 2217	(0.08)	(0.16)	(0.16)
6-month sampling window	-0.00	0.05	0.06
n = 989	(0.12)	(0.22)	(0.23)
2-month sampling window	0.14	0.28	0.28
n = 369	(0.19)	(0.37)	(0.36)
Both Nat. Experiments pooled - OLS			
12-month sampling window	0.00	-0.05	-0.03
n = 4771	(0.05)	(0.10)	(0.10)
6-monthsampling window	-0.03	-0.09	-0.08
n = 2072	(0.07)	(0.14)	(0.14)
2-month sampling window	-0.04	0.20	0.08
n = 717	(0.12)	(0.25)	(0.25)
Both Nat. Experiments pooled - Tobit			
12-month sampling window	-0.06	-0.01	-0.02
n = 4771	(0.06)	(0.13)	(0.13)
6-monthsampling window	-0.03	-0.14	-0.15
n = 2072	(0.09)	(0.19)	(0.19)
2-month sampling window	-0.04	0.19	0.16
n = 717	(0.15)	(0.31)	(0.31)

Note: Model 1 controls for date of birth and source country, Model 2 also controls for immigration date (discontinuity design estimator), and Model 3 additionally controls for the square of the immigration date (discontinuity design estimator). Source: Administrative German pension data; author calculations.

Tab. 2.4: Effects of pension cuts on retirement age: men and women combined.

	Model 1	Model 3	Model 3
Natural Experiment 1 - OLS			
12-month sampling window	0.057	0.073	0.050
n = 5959	(0.036)	(0.075)	(0.075)
6-month sampling window	0.069	0.011	-0.014
n = 2562	(0.053)	(0.108)	(0.108)
2-month sampling window	-0.003	-0.068	-0.074
n = 830	(0.092)	(0.204)	(0.208)
Natural Experiment 1 - Tobit			
12-month sampling window	0.043	0.084	0.057
n = 5959	(0.045)	(0.092)	(0.094)
6-month sampling window	0.074	-0.018	-0.019
n = 2562	(0.064)	(0.132)	(0.132)
2-month sampling window	-0.015	-0.041	-0.041
n = 830	(0.110)	(0.234)	(0.234)
Natural Experiment 2 - OLS			
12-month sampling window	0.087*	-0.029	-0.028
n = 5336	(0.045)	(0.092)	(0.091)
6-month sampling window	0.032	-0.057	-0.055
n = 2376	(0.068)	(0.131)	(0.131)
2-month sampling window	0.009	-0.028	-0.020
n = 907	(0.110)	(0.226)	(0.226)
Both Nat. Experiments pooled - OLS			
12-month sampling window	0.071**	0.013	0.031
n = 11295	(0.029)	(0.060)	(0.059)
6-monthsampling window	0.042	-0.018	-0.031
n = 4928	(0.043)	(0.086)	(0.085)
2-month sampling window	-0.035	0.060	-0.016
n = 1737	(0.072)	(0.155)	(0.154)
Both Nat. Experiments pooled - Tobit			
12-month sampling window	0.045	0.036	0.031
n = 11295	(0.038)	(0.078)	(0.078)
6-monthsampling window	0.058	-0.083	-0.095
n = 4928	(0.055)	(0.109)	(0.109)
2-month sampling window	-0.064	0.016	-0.012
n = 1737	(0.089)	(0.182)	(0.183)

Note: Model 1 controls for date of birth and source country, Model 2 also controls for immigration date (discontinuity design estimator), and Model 3 additionally controls for the square of the immigration date (discontinuity design estimator). Source: Administrative German pension data; author calculations.

Combining the estimation samples for Natural Experiments 1 and 2 in order to increase the precision of the estimated coefficients yields no statistically significant point estimates either, with the largest point estimate for men being 0.19 for the 2-month sampling window (with a standard error of 0.31; Table 2.3, Model 2, bottom). If the samples for men and women and both natural experiments are combined, the maximum point estimate is 0.036 (with a standard error of 0.078; 2.4, Model 2, bottom). For the 6-month sampling window, the estimated OLS confidence interval for Model 3 has lower and upper bounds of -72 (i.e. (-0.031 - 1.96 $\times$ 0.085) $\times$ 365) and 49 (i.e. (-0.031+ 1.96 $\times$ 0.85) $\times$ 365) days, respectively. As the sample means show, the 6-month sampling window already balances the distribution of the source country well. By narrowing the sampling window even further, down to two months, we obtain a confidence interval lower and upper bounds of -104 (i.e. (0.016 - 1.96 $\times$ 0.154) $\times$ 365) and 116 (i.e. (0.016 + 1.96 $\times$ 0.154) $\times$ 365) days, respectively. For women, none of the regression discontinuity design estimates (Models 2 and 3) is statistically significant and the point estimates are even more consistently close to zero than those for men (Table 2.20 in the appendix).

2.4.2 Implied extensive labor supply elasticities

We then consider what extensive labor supply elasticities these estimates imply given the following lifetime extensive labor supply elasticity. To this end, we want to know how lifetime labor supply reacts to changes in the price of leisure (w-r). However, what we estimate is the reaction of lifetime labor supply to changes in the pension rate r. Assuming that the wage rate is unaffected by the reforms in question, we obtain the following relationship between the labor supply elasticity and the effect of a pension change on the retirement age, i.e. what we estimate:

$$\begin{aligned}
\eta_{LFP,price\ of\ leisure} &= \frac{d(years\ worked)}{d(w-r)} \times \frac{\overline{w-r}}{years\ worked} \\
&\approx \frac{\Delta(years\ worked)}{\Delta(w-r)} \times \frac{w_{median,MZ} - \overline{r}_{before}}{years\ worked} \\
&\approx \frac{\Delta(years\ worked)}{-\Delta r} \times \frac{w_{median,MZ} - \overline{r}_{before}}{years\ worked}
\end{aligned} \tag{2.2}$$

To gauge the price of leisure, we use pensions and net earnings data for repatriated ethnic Germans from the German Microcensus (MZ) and include these figures into the above formula. Based on a net earnings estimate of 1,000 Euros from the microcensus data minus the average pension from the administrative pension data, we 'guesstimate' the price of leisure for men in the control group to be around 140 Euros per month in Natural Experiment 1 and 400 Euros per month in Natural Experiment 2. For women, we find that net earnings are around 500 Euros, and an average pension is about the same or a little higher, yielding a price of leisure that is zero or negative. We therefore focus on men for the simulation of the labor supply elasticity.

The statistics needed to calculate the extensive life-time labor supply elasticity for men - derived from our estimates, the administrative pension data, and the microcensus data - are given in Table 2.11 in the appendix. The number of years worked is 39 and 36 for the control groups in Natural Experiments 1 and 2, respectively. The table also reports the price of leisure on an annualized basis before and after the reforms associated with the two natural experiments.

The elasticity estimates are given in Table 2.5, which summarizes the estimates for the first and second terms (derivative and means ratio) of the product that represents the elasticity given in equation 2.2. Based on our standard error estimates for the reforms' labor supply (retirement) effects, we also provide upper and lower confidence limits, treating the means ratio as non-stochastic. As Table 2.5 shows, both the point estimates and the confidence limits are close to zero. For the first reform, the estimated elasticity is -0.013, with a lower and upper limit of -0.034 and 0.008, respectively. For the second, the point estimate is 0.008, with lower and upper limits of -0.050 and 0.065, respectively. All these numbers are very small.

2.4.3 Does selective mortality affect the results?

If the reforms under study have an impact on the mortality rate of the affected workers, our estimates might be subject to selection bias. That is, workers who retired at a later age because of lower pensions might have died earlier and be systematically underrepresented in our sample, which is taken from the stock of living pensioners in 2008. In addition, mortality is an interesting dependent variable in its own right which has been investigated in the pension cut studies by Snyder and Evans (2006) for the United States and Jensen and Richter (2004) for Russia. The pension cuts investigated

Tab. 2.5: Estimates of the extensive labor supply elasticity.

	(1) Difference Ratio	(2) Means ratio	(3) Elasticity
Nat. Exp. 1			
Point estimate	-0.00030	43	-0.013
Lower limit	-0.00079	43	-0.034
Upper limit	0.00020	43	0.008
Nat. Exp. 2			
Point estimate	0.00006	133	0.008
Lower limit	-0.00037	133	-0.050
Upper limit	0.00049	133	0.065

Note: Columns (1) and (2) provide the figures for the first and second terms of equation (2), respectively, whose product equals the extensive labor supply elasticity provided in column (3). *Source:* Administrative German pension data; German Microcensus; author calculations.

here affect low-income groups, and for them, changes in their budget constraints could affect health investments, leading to lower life expectancy.

In Table 2.6, we draw on the 2006-2008 administrative data on exits from the pension system because of death to estimate whether workers affected by the pension cut generated by Natural Experiments 1 and 2 had a higher probability of dying during the years 2006 through 2008 (the only time span for which such data were available). Unfortunately, the administrative data made available do not contain the exact date of immigration, but only the year of immigration, so that we cannot build the discontinuity samples as we would like and as we did in the Section 2.4.1. Hence, the samples used for this analysis are larger than those previously discussed because of the necessarily larger sampling windows. Sample means for the estimation samples are provided in Table 2.21 and Table 2.22 in the appendix. Between 5 and 11 percent of men and between 2 and 5 percent of women passed away within this 3-year interval, depending on the natural experiment and sample considered. We use the same restrictions as before concerning the birth date intervals for the estimation samples.

For Natural Experiment 1, we create two estimation samples, which, because the exact immigration date is not observed, must include all repatriated ethnic Germans who immigrated at any time during 1990 or 1991. Some workers who immigrated in 1990, however, experienced additional pension cuts through another reform that could not be evaluated here because of data issues. Nevertheless, even without the exact immigration date, we were able to identify whether an individual was affected because the data allowed us to de facto exclude workers who immigrated in the first half of 1990 from Sample A.[20] In Sample B, we include all 1990 immigrants in the control group.

For Natural Experiment 2, we build two further samples, for which we again do not observe the exact immigration date, only the year plus an indicator of whether the individual was affected by the reform associated with the natural experiment. Sample A includes only those who immigrated in 1996, whereas Sample B also includes immigrants who arrived in 1995 (control group) or 1997 (treatment group).

Table 2.6 reports the estimates for the reforms (with treatment defined by immigration year) both on the log pension and on mortality for men in the 3-year period from 2006 through 2008. The estimates we obtain for the effective pension cuts are mostly similar to those for the regression discontinuity discussed in Section 4.1. For the two 'B samples', the estimates are larger, indicating a 20 percent cut in pensions, to be expected given the additional reform that cut pensions for immigrants arriving in the second half of 1990 (Natural Experiment 1) and the fact that all immigrants who arrived in 1997 were affected by pension cuts related to both Natural Experiment 2 and Natural Experiment 3.

For Natural Experiment 1, we find no statistically significant evidence for a mortality effect of pension cuts: the point estimates are around zero, at -0.9 and 0.9 percentage points for Samples A and B, respectively. For Natural Experiment 2, as in Snyder and Evans (2006), the point estimate is both negative and statistically significant, implying that a pension cut of 14 percent decreased mortality by 2.3 percentage points (Sample A, significant only at the 10 percent level). The point estimates for men and women combined are somewhat smaller (Table 2.7), because the point estimates for women are closer to zero than those for men. For women, only the estimate for Sample A, Natural Experiment 2, is statistically significant (Table 2.23 in the appendix).

These results contrast with those by Jensen and Richter (2004) for Russia, where decreases in household income by 24 percent on average pension increased male mor-

[20] Even though the month (and day) of immigration is not observed in this sample, the data do include a marker that identifies those who immigrated before July 1990 provided they retired before 1996.

Tab. 2.6: Pension cuts and mortality: men.

Dependent variable	Sample A	Sample B
Natural Experiment 1		
Died in 2006-2008	-0.009	0.009
	(0.012)	(0.008)
Log(net pension)	-0.11***	-0.196***
	(0.008)	(0.008)
Observations	2,923	8,227
Natural Experiment 2		
Died in 2006-2008	-0.023*	-0.017***
	(0.012)	(0.006)
Log(net pension)	-0.140***	-0.216***
	(0.018)	(0.006)
Observations	2,266	7,361

Note: Sample B = immigrated in 1990 and 1991; Sample A excludes those who immigrated before July 1990 and those who retired after 1995. *Source:* Administrative German pension data; German Microcensus; author calculations.

Tab. 2.7: Pension cuts and mortality: men and women combined.

Dependent variable	Sample A	Sample B
Natural Experiment 1		
Died in 2006-2008	-0.016	-0.009
	(0.006)	(0.003)
Log(net pension)	-0.080***	-0.151***
	(0.007)	(0.008)
Observations	7,113	18,195
Natural Experiment 2		
Died in 2006-2008	-0.016***	-0.009***
	(0.006)	(0.003)
Log(net pension)	-0.145***	-0.195***
	(0.010)	(0.004)
Observations	5,355	17,146

Note: Sample B = immigrated in 1990 and 1991; Sample A excludes those who immigrated before July 1990 and those who retired after 1995. *Source:* Administrative German pension data; German Microcensus; author calculations.

tality by almost 6 percentage points within a 2-year interval (with no effects on female mortality). However, contrary to the case of German repatriates, some households temporarily received no pension during the Russian pension crisis, and the Russian public health system is not comparable with the one in Germany. Regarding the result of Snyder and Evans (2006) for the U.S., on the other hand, it seems to be confirmed by our estimates. These authors find that pension cuts stemming from the social security notch decreased mortality by 2 percentage points within a 5-year interval. Nevertheless, without precise information on immigration date, the estimates presented in tables 2.6, 2.7 and in table 2.23 in the appendix are not true regression discontinuity estimates. Therefore, doubts remain about whether they are a result of the pension cut reform or of unobserved differences between treatment and control groups that are unrelated to pensions but cannot be controlled for because of the paucity of sociodemographic information in the administrative data. Hence, keeping this caveat in mind, we conclude that with the data at hand, we can detect no significant effects of the pension reforms in terms of increased mortality. We assume that the significant negative effects found for Natural Experiment 2 would not hold in narrow discontinuity samples such as those built in Section 2.4.1.

2.4.4 Can workers retire earlier to avoid a pension cut?

Having found that pension cuts do not increase the labor supply significantly, we ask whether the reverse is true: are low-skilled workers willing to decrease their labor supply if given incentive to do so? Such an incentive is provided by Natural Experiment 3 for repatriated ethnic Germans, one on which we draw to investigate this question. Specifically, repatriated ethnic Germans immigrating to Germany before 1991 (and hence unaffected by the first reform) faced a 40 percent cut in earnings points if they retired on or after October 1 1996, although, as already explained, actual pension cuts were smaller, albeit still significant, because earnings points translate nonlinearly into pension levels.

To investigate whether workers evaded this reform, we compare repatriated ethnic Germans who turned 60 before (treatment) or after (control) September 1996, the rule being that one can retire on the first day of the month following one's critical birthday.

This design is motivated by the fact that, as confirmed by both the regulations discussed in Section 2.2.1 and the empirical evidence on the survivor functions that show the retirement age distribution, retirement before age 60 was much more difficult than retirement at or after that age.

Table 2.8 shows that retiring before October 1, 1996 is associated with a 11-13 percent higher pension level. Table 2.9 provides the regression discontinuity estimates (models 2 and 3), where the treatment group is defined based on birth date such that a person turns 60 years of age before September 1996.[21] Sampling windows are defined on birth dates within 12, 6, 4 and 2 months' intervals, so that the 12-month interval runs from birth months March 1936 through January 1937. The size of the point estimates, which are all statistically insignificant, suggests that turning 60 before the cutoff date leads to a retirement age about $0.296 \times 12 = 3.6$ months younger. The standard error for this estimate is $0.221 \times 12 = 2.65$ months (models 2/3 for the 2-month discontinuity sample). If we assume that those people who retire at age 60 because of the reform would have retired at age 63 in the absence of the reform, our point estimate implies that $3.6 / (3 \times 12) = 10$ percent of repatriated ethnic Germans react to Natural Experiment 3. Such a result may be interpreted to mean that, by and large, the vast majority of repatriated ethnic German workers retired as early as the regulations would allow and hardly any were able to retire even earlier to avoid the cut in pension rights.

To obtain another perspective on Natural Experiment 3, we plot in Figure 4 the transition rates into retirement by calendar time to test whether there is any heaping of transitions into retirement in September 1996, that is immediately before the cutoff date. We consider all repatriated ethnic German men of the birth cohorts 1927 through 1942 who immigrated between 1980 and 1990 and were at least 55 years old at their age of retirement. These are birth cohorts similar to the ones analyzed in Natural Experiments 1 and 2 who are at or close to retirement age around the cutoff date and immigration cohorts who are not affected by pension cut reforms 1 and 2, so that they have most to gain from retiring before October 1, 1996. In Figure 4, we compare repatriated ethnic Germans affected by Natural Experiment 3 (treatment group) with repatriated

[21] Because so many women retire at age 60 and it is difficult to retire earlier, we expect it to be much more likely to observe an effect for men, so that we limit ourselves to this group. Retirement before the age of 55 is excluded because it is mostly governed by more severe medical conditions and not relevant for the cohorts investigated in Natural Experiments 1 and 2.

Tab. 2.8: Effect on log pensions of retiring before October 1 1996.

Month-of-birth interval around 1 September 1936		Model 1	Model 3	Model 3
12-month	(n = 2,611)	0.113***	0.118***	0.118***
		(0.010)	(0.010)	(0.010)
6-month	(n = 1,342)	0.107***	0.112***	0.112***
		(0.015)	(0.014)	(0.014)
4-month	(n = 926)	0.126***	0.132***	0.134***
		(0.018)	(0.018)	(0.018)
2-month	(n = 492)	0.129***	0.128***	0.128***
		(0.026)	(0.025)	(0.025)

Note: Model 1 controls for date of birth and source country (raw gap); Model 2 also controls for the month of birth, and Model 3 additionally controls for the square of the month of birth. Because we do not observe the exact birth date, models 2 and 3 are identical for the 2-months sampling window. *Source:* Administrative German pension data; author calculations.

Tab. 2.9: Effect on retirement age of turning 60 before September 1 1996 (with possible retirement before October 1 1996).

Month-of-birth interval around 1 September 1936		Model 1	Model 3	Model 3
12-month	(n = 2,611)	-0.090	-0.086	-0.083
		(0.159)	(0.159)	(0.159)
6-month	(n = 1,342)	-0.272	-0.242	-0.243
		(0.230)	(0.229)	(0.229)
4-month	(n = 926)	-0.377	-0.304	-0.237
		(0.300)	(0.299)	(0.324)
2-month	(n = 492)	-0.339*	-0.296	-0.296
		(0.192)	(0.191)	(0.191)

Note: Model 1 controls for date of birth and source country (raw gap); Model 2 also controls for the month of birth (discontinuity design estimator), and Model 3 additionally controls for the square of the month of birth (discontinuity design estimator). *Source:* Administrative German pension data; author calculations.

ethnic Germans from Poland, who are not affected due to a German-Polish Accord (control group). It is shown that there is some heaping of transition into retirement shortly before the cutoff (September 1996 and to some extend August 1996) among the treatment group but not among the control group. However, this heaping is hard to distinguish from the amplitude of the general "noise" (variation) in these data. A difference-in-differences estimator for the transition rate differences between treatment and control groups immediately before and after the cutoff date (September versus October) yields an effect of (0.0130-0.0106) - (0.0098-0.0092) = 0.0018, with a standard error of 0.0016. This would be an increase in the transition rate of about 16 percent (21 percent) in September compared to the level observed in July (March) for the treatment group. However, this effect is not statistically significant.

2.4.5 Explanations for the empirical results: "corner solution" or behavioral approach?

Combining the above observations with the estimates of the labor supply effects of exogenous pension cuts as implemented in Natural Experiments 1 and 2, a standard labor supply model would suggest that low-skilled workers like the repatriated ethnic Germans are mired in a 'corner solution' of retiring at the earliest possible date (as learnt from Natural Experiment 3) and that reducing their pensions did not significantly increase their labor supply in the form of later retirement (as learnt from Natural Experiments 1 and 2). Theoretically, this finding can be explained by the fact that in this corner solution, the marginal rate of substitution between leisure time spent in retirement and consumption is higher than the price of leisure, a fact that still held true even after the cuts in the price of leisure brought about by Natural Experiments 1 and 2.[22]

[22]To illustrate, consider a simple labor supply model where the budget constraint faced by the worker can be represented as

$$C = Tw - (w - p)R \tag{2.3}$$

where C is total consumption, T is the time left from the earliest possible retirement date until the expected end of life, w is the wage rate that the individual could earn (per period), p is the pension earned per period, and R is the number of periods spent in retirement. Given a relatively low w (Calculations based on the microcensus data show that the median income of repatriated ethnic German men or women who immigrated after 1997, being older than 55 in 2005 and currently out of

Behavioral economics can provide an alternative explanation why Natural Experiments 1 and 2 have not increased labor supply. We can think of a reference-dependent utility model, where even after the pension cuts, repatriated ethnic Germans are likely to have had pension incomes above their "aspiration levels", thanks to the windfall gains of obtaining a German pension. These aspiration levels may have been defined earlier on in their lives when ethnic Germans still lived in the Soviet Union or Romania (the main source countries for people in our samples). It is unclear whether in the case of ethnic Germans, aspiration levels would not have increased by moving to Germany. Bertrand et al. (2000) show that networks defined by location and language groups have an impact on welfare participation, a finding that might be interpreted such that these groups define aspiration/reference levels. On the other hand, we observe that the distribution of occupations and the retirement age for repatriated ethnic Germans are very similar to the ones of low-skilled ethnic Germans. Ethnic Germans might therefore have adapted their aspiration levels to those of other low-skilled Germans. Hence, not only the job and retirement outcomes of these two groups, but also their reactions to reforms as the ones we analyze may be similar. This would imply that the estimates we have obtained for repatriated ethnic Germans are informative for low-skilled German workers in general. Of course, because there was no such reform for other low-skilled Germans, we cannot test whether this reasoning holds empirically.

2.5 Conclusions

Theoretically, the three natural experiments on pension reforms analyzed here, by exposing workers to exogenous pension cuts and the possibility of avoiding a decrease in pension, provided incentives for both later (Natural Experiments 1 and 2) and earlier (Natural Experiment 3) retirement. We therefore analyze the effects of these compar-

the labor market, is around 500 Euros - income data are given only in intervals -. For the same group, working men earn between 1,000 Euros and 1,200 Euros and working women around 500 Euros) and a relatively high p (albeit one still lower than w), we expect the budget constraint to be relatively flat when drawn in C-R space (or even to have a positive slope, which is not indicated by the figures discussed above, though). A reform that lowers p, like the one considered here, will slope the budget line even more negatively, and even more so if the slope was negative before reform. As regards a corner solution in which workers retire as soon as the administration allows - meaning that R = T - such a solution could still conceivably exist post reform if the difference between w and the new pension level is still sufficiently small relative to the marginal rate of substitution of R for C in the point where R=T and C=Rp.

atively large pension changes (between 8 and 16 percent) on the retirement age of one specific group of low-skilled workers, repatriated ethnic Germans, the only group to whom the pension reforms applied.

First, we find hardly any reaction to exogenous pension cuts (Natural Experiments 1 and 2) in terms of retirement behavior. Rather, combining our regression discontinuity design estimates with descriptive evidence on the price of leisure, we find an upper bound for the extensive lifetime labor supply elasticity estimate of 0.07, a very small number. Second, we find no evidence that the pension cuts increased mortality in either men or women. Third, according to the results for Natural Experiment 3, there was barely any worker reaction to incentives to retire earlier in order to avoid the 13-16 percent pension cut.

Overall, our study demonstrates that because European welfare states, of which Germany is an exemplary case, provide few work incentives for older low-skilled workers; for most, quitting the labor market as early as possible seems the optimal choice. More specifically, low-skilled workers in Germany, as represented here by repatriated ethnic Germans, are bogged down in a 'corner solution' of retiring as early as possible, one in which the price of leisure is so low that even the comparatively large pension cuts analyzed here provide no incentives to work longer.

One major policy implication of this finding is that even significant decreases or increases in the pension rate - for example, of between 8-16 percent as analyzed here - have virtually no incentive effect in terms of labor supply and thus have predominantly distributional consequences (assuming the intensive labor supply elasticity to also be low). There thus seems ample scope for redistribution in both directions through changes in the pension rate.

2.6 Appendix: additional tables and figures

Tab. 2.10: Occupational distribution of workers aged 55-65: repatriated ethnic Germans versus low-skilled and skilled Germans.

	Men			Women		
	REGs	Low-Sk.	Skilled	REGs	Low-Sk.	Skilled
Self-employed w/o employees	6	10	10	3	5	7
Self-employed w/ employees	6	6	12	2	2	5
Home worker (family business)	0	1	1	2	6	4
Civil servant or judge	4	2	11	1	0	8
White-collar employee	29	25	41	42	43	61
Blue-collar employee	54	56	26	49	44	16
Index of dissimilarity to REGs	-	7	28	-	6	33

Note: REG = repatriated ethnic Germans immigrated in 1990 or later; low-skilled workers = employed individuals without even apprenticeship education; skilled workers = employed individuals with apprenticeship education or higher. *Source:* German Microcensus 2005; author calculations.

Tab. 2.11: Statistics for calculating labor supply elasticity.

	(1)	(2)	(3)	(4)	(5)	(6)
	Coeff.	Annual pension change	Annual pension change	PoL before	PoL after	Years worked
Nat. Exp. 1	-0.26	-873	10,308	1,692	2,565	39
(s.e.)	(0.22)					
Nat. Exp. 2	0.06	-1,048	7,183	4,817	5,866	36
(s.e.)	(0.23)					

Note: PoL = price of leisure in euros per year; the tables provides the statistics needed for the calculation of the labor supply elasticity as given in equation (2) of the paper. To calculate the elasticity, first, the estimated coefficient, given in column (1) is divided by the change in the price of leisure, which equals the annual pension change, that is column (2) equals column (4) minus column (5). This ratio is then multiplied by the ratio of the price of leisure before the reform, column (4), divided by the average number of years worked before the reform, column (6). The annual pension before the reform, column (3), is given for descriptive purposes. *Source:* Administrative German pension data; German Microcensus; author calculations.

Tab. 2.12: Sample selection for Natural Experiments 1 and 2.

	Nat. Experiment 1		Nat. Experiment 2	
	All All	Former USSR	All All	Former USSR
Born Jan. 1932-Mar. 1936/Sep. 1936-Dec. 1941	128,032		188,424	
Males	56,748		84,765	
Excluding former Polish residents	36,223		55,170	
Date of immigration available	35,829		54,359	
Immigrated Jul. 1990-Jun. 1991/Nov. 1995-Nov. 1996	2,645		2,286	
Date of retirement available	2,640	1,567	2,283	
Retired after immigration (Sample 1A/2A) 2,554	1,547	2,217	2,097	
Immigrated Oct. 1990-Mar. 1991/Feb. 1996-Aug. 1996 (Sample 1B/2B)	1,083	779	989	939
Immigrated Dec. 1990-Jan. 1991/Apr. 1996-Jun. 1996 (Sample 1C/2C)	348	270	369	348

Source: Administrative data on the German pension insurance.

Tab. 2.13: Sample means for Natural Experiment 1 - treatment and control groups in different discontinuity samples: men.

	12-month window		6-month window		2-month window	
Treatment	Control	Treatment	Control	Treatment	Control	
Age at retirement	60.5	60.8	60.5	60.5	60.3	60.5
	(1.76)	(1.96)	(1.86)	(1.85)	(1.71)	(1.79)
Date of retirement	1994.8	1995.0	1994.8	1994.7	1994.6	1994.6
	(2.21)	(2.41)	(2.29)	(2.27)	(2.20)	(2.28)
Date of retirement (censored)	1993.8	1993.7	1993.7	1993.7	1993.7	1993.7
	(1.41)	(1.51)	(1.46	(1.45)	(1.41)	(1.42)
Share - censored	0.23	0.30	0.24	0.23	0.21	0.21
Retired before October 1996	0.81	0.76	0.80	0.81	0.83	0.82
Pension payment in Euros	786.0	836.7	789.0	859.0	810.7	874.7
	(118.2)	(161.6)	(114.4)	(165.1)	(109.7)	(195.6)
Date of birth	1934.3	1934.2	1934.3	1934.2	1934.3	1934.1
	(1.24)	(1.26)	(1.24)	(1.26)	(1.25)	(1.27)
Age on January 1 1990	55.8	55.8	55.7	55.8	55.7	55.9
	(1.24)	(1.26)	(1.24)	(1.26)	(1.25)	(1.27)
From Romania	0.25	0.45	0.24	0.27	0.17	0.21
From the former USSR	0.72	0.53	0.73	0.71	0.79	0.76
From another country	0.04	0.02	0.03	0.02	0.03	0.02
Number of observations	1,007	1,547	500	583	145	203

Source: Administrative German pension data; author calculations. Standard deviations are given in parenthesis.

Tab. 2.14: Sample means in Natural Experiment 1 - treatment and control groups in different discontinuity samples: women

	12-month window		6-month window		2-month window	
Treatment	Control	Treatment	Control	Treatment	Control	
Age at retirement	60.23	60.31	60.17	60.05	60.06	60.06
	(1.88)	(2.2)	(1.9)	(1.99)	(1.74)	(1.94)
Date of retirement	1994.4	1994.52	1994.42	1994.26	1994.24	1994.27
	(2.12)	(2.41)	(2.08)	(2.19)	(2.03)	(2.19)
Date of retirement (censored)	1993.83	1993.71	1993.86	1993.68	1993.76	1993.67
	(1.45)	(1.6)	(1.46)	(1.59)	(1.56)	(1.6)
Share - censored	0.12	0.17	0.13	0.13	0.12	0.14
Retired before October 1996	0.9	0.86	0.9	0.9	0.92	0.9
Pension payment in Euros	675.68	661.04	679.2	711.64	702.83	737.19
	(211.62)	(249.9)	(207.23)	(226.32	(196.54)	(223.62)
Date of birth	1934.17	1934.21	1934.25	1934.22	1934.18	1934.21
	(1.27)	(1.24)	(1.24)	(1.26)	(1.27)	(1.33)
Age on January 1 1990	55.83	55.79	55.75	55.78	55.82	55.79
	(1.27)	(1.24)	(1.24)	(1.26)	(1.27)	(1.33)
From Romania	0.24	0.44	0.23	0.25	0.16	0.17
From the former USSR	0.72	0.53	0.74	0.7	0.8	0.78
From another country	0.05	0.03	0.03	0.04	0.03	0.05
Number of observations	1,339	2,066	676	803	182	300

Source: Administrative German pension data; author calculations. Standard deviations are given in parenthesis.

Tab. 2.15: Sample means in Natural Experiment 1 - treatment and control groups in different discontinuity samples: women

	12-month window		6-month window		2-month window	
Treatment	Control	Treatment	Control	Treatment	Control	
Age at retirement	60.23	60.31	60.17	60.05	60.06	60.06
	(1.88)	(2.2)	(1.9)	(1.99)	(1.74)	(1.94)
Date of retirement	1994.4	1994.52	1994.42	1994.26	1994.24	1994.27
	(2.12)	(2.41)	(2.08)	(2.19)	(2.03)	(2.19)
Date of retirement (censored)	1993.83	1993.71	1993.86	1993.68	1993.76	1993.67
	(1.45)	(1.6)	(1.46)	(1.59)	(1.56)	(1.6)
Share - censored	0.12	0.17	0.13	0.13	0.12	0.14
Retired before October 1996	0.9	0.86	0.9	0.9	0.92	0.9
Pension payment in Euros	675.68	661.04	679.2	711.64	702.83	737.19
	(211.62)	(249.9)	(207.23)	(226.32	(196.54)	(223.62)
Date of birth	1934.17	1934.21	1934.25	1934.22	1934.18	1934.21
	(1.27)	(1.24)	(1.24)	(1.26)	(1.27)	(1.33)
Age on January 1 1990	55.83	55.79	55.75	55.78	55.82	55.79
	(1.27)	(1.24)	(1.24)	(1.26)	(1.27)	(1.33)
From Romania	0.24	0.44	0.23	0.25	0.16	0.17
From the former USSR	0.72	0.53	0.74	0.7	0.8	0.78
From another country	0.05	0.03	0.03	0.04	0.03	0.05
Number of observations	1,339	2,066	676	803	182	300

Source: Administrative German pension data; author calculations. Standard deviations are given in parenthesis.

Tab. 2.16: Sample means in Natural Experiment 2 - treatment and control groups in different discontinuity samples: men.

	12-month window		6-month window		2-month window	
Treatment	Control	Treatment	Control	Treatment	Control	
Age at retirement	60.88	60.8	60.89	60.89	60.84	60.83
	(1.84)	(2.08)	(1.82)	(2.01)	(1.75)	(2.05)
Date of retirement	2000.03	1999.98	2000.01	2000	1999.85	2000.21
	(2.54)	(2.83)	(2.57)	(2.81)	(2.51)	(2.96)
Pension payment in Euros	514.58	637.96	518.06	607.43	506.97	601.64
	(86.97)	(131.52)	(86.25)	(132.21)	(84.44)	(134.38)
Date of birth	1939.15	1939.18	1939.12	1939.11	1939.01	1939.38
	(1.49)	(1.47)	(1.47)	(1.49)	(1.44)	(1.51)
Age on January 1 1990	50.85	50.82	50.88	50.89	50.99	50.62
	(1.49)	(1.47)	(1.47)	(1.49)	(1.44)	(1.51)
From Romania	0.04	0.04	0.04	0.04	0.06	0.03
From the former USSR	0.95	0.95	0.95	0.95	0.92	0.97
From another country	0.01	0.01	0.01	0.01	0.02	0.01
Number of observations	1,120	1,097	554	435	191	178

Source: Administrative German pension data; author calculations. Standard deviations are given in parenthesis.

Tab. 2.17: Sample means in Natural Experiment 2 - treatment and control groups in different discontinuity samples: women.

	12-month window		6-month window		2-month window	
Treatment	Control	Treatment	Control	Treatment	Control	
Age at retirement	60.22	60.14	60.25	60.21	60.18	60.2
	(1.32)	(1.43)	(1.36)	(1.49)	(1.41)	(1.55)
Date of retirement	1999.39	1999.34	1999.35	1999.44	1999.33	1999.53
	(1.98)	(2.06)	(1.98)	(2.07)	(1.99)	(2.05)
Pension payment in Euros	495.97	603.6	493.88	589.41	483.28	576.28
	(97.14)	(144.87)	(99.33)	(136.24)	(105.13)	(134.99)
Date of birth	1939.17	1939.19	1939.11	1939.23	1939.15	1939.33
	(1.49)	(1.51)	(1.49)	(1.49)	(1.48)	(1.49)
Age on January 1 1990	50.83	50.81	50.89	50.77	50.85	50.67
	(1.49)	(1.51)	(1.49)	(1.49)	(1.48)	(1.49)
From Romania	0.04	0.05	0.05	0.06	0.06	0.05
From the former USSR	0.94	0.93	0.93	0.92	0.93	0.92
From another country	0.02	0.02	0.02	0.02	0.01	0.03
Number of observations	1533	1586	758	629	268	270

Source: Administrative German pension data; author calculations. Standard deviations are given in parenthesis.

Tab. 2.18: Sample means for Natural Experiment 3 - treatment and control groups in different discontinuity samples: includes individuals born in September 1936.

	12-month window		6-month window		4-month window		2-month window	
Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	
Age at retirement	61.20	61.46	61.26	61.52	61.26	61.41	61.16	61.43
	(2.11)	(2.21)	(2.12)	(2.14)	(2.13)	(2.13)	(2.25)	(2.09)
Date of retirement	1997.32	1998.59	1997.64	1998.4	1997.77	1998.15	1997.74	1998.09
	(2.14)	(2.23)	(2.13)	(2.15)	(2.13)	(2.13)	(2.25)	(2.09)
Retired at age 60	0.33	0.29	0.33	0.28	0.35	0.30	0.36	0.31
Pension payment in Euros	891.25	827.57	877.71	832.66	873.98	844.42	870.31	859.15
	(193.59)	(160.36)	(187.20)	(159.62)	(187.10)	(164.66)	(200.48	(161.18)
Date of birth	1936.12	1937.13	1936.38	1936.87	1936.51	1936.75	1936.58	1936.67
	(0.29)	(0.29)	(0.15)	(0.15)	(0.07)	(0.07)	(0.00)	(0.00)
Age on January 1 1990	53.88	52.87	53.62	53.13	53.49	53.25	53.42	53.33
	(0.29)	(0.29)	(0.15)	(0.15)	(0.07)	(0.07)	(0.00)	(0.00)
From Romania	0.46	0.46	0.45	0.49	0.45	0.50	0.44	0.46
From the former USSR	0.40	0.44	0.41	0.41	0.42	0.40	0.42	0.43
From another country	0.14	0.10	0.14	0.10	0.12	0.10	0.13	0.11
Number of observations	1,248	1,363	646	696	444	482	250	242

Source: Administrative German pension data; author calculations. Standard deviations are given in parenthesis.

Tab. 2.19: Effective log pension changes for women caused by Natural Experiments 1 and 2.

	Model 1	Model 3	Model 3
Natural Experiment 1 - OLS			
12-month sampling window	-0.07***	-0.05*	-0.06**
n = 3405	(0.02)	(0.03)	(0.03)
6-month sampling window	-0.07***	-0.04	-0.04
n = 1479	(0.02)	(0.04)	(0.04)
2-month sampling window	-0.06	-0.03	-0.04
n = 482	(0.04)	(0.07)	(0.08)
Natural Experiment 2 - OLS			
12-month sampling window	-0.19***	-0.18***	-0.18***
n = 3119	(0.01)	(0.02)	(0.02)
6-month sampling window	-0.18***	-0.18***	-0.18**
n = 1387	(0.01)	(0.03)	(0.03)
2-month sampling window	-0.18***	-0.15***	-0.15***
n = 538	(0.02)	(0.04)	(0.04)
Both Nat. Experiments pooled - OLS			
12-month sampling window			
n = 6524	-0.13***	-0.12***	-0.13***
6-monthsampling window	(0.01)	(0.02)	(0.02)
n = 2856	-0.12***	-0.12***	-0.12***
2-month sampling window	(0.01)	(0.02)	(0.02)
n = 1020	-0.11***	-0.12***	-0.11***

Not(0.02)e: Model(0.04) 1 contr(0.04)ols for date of birth and source country, Model 2 also controls for immigration date (discontinuity design estimator), and Model 3 additionally controls for the square of the immigration date (discontinuity design estimator). Source: Administrative German pension data; author calculations.

Tab. 2.20: Effects of pension cuts for women on age of retirement.

	Model 1	Model 3	Model 3
Natural Experiment 1 - OLS			
12-month sampling window	0.15***	0.13	0.13
n = 3405	(0.05)	(0.10)	(0.10)
6-month sampling window	0.15**	0.17	0.15
n = 1479	(0.07)	(0.14)	(0.15)
2-month sampling window	0.06	0.00	-0.04
n = 482	(0.13)	(0.28)	(0.29)
Natural Experiment 1 - Tobit			
12-month sampling window	0.15***	0.13	0.13
n = 3405	(0.06)	(0.12)	(0.12)
6-month sampling window	0.16**	0.14	0.14
n = 1479	(0.08)	(0.17)	(0.17)
2-month sampling window	0.04	0.00	-0.00
n = 482	(0.15)	(0.30)	(0.30)
Natural Experiment 2 - OLS			
12-month sampling window	0.09*	-0.00	-0.00
n = 3119	(0.05)	(0.10)	(0.10)
6-month sampling window	0.04	-0.08	-0.08
n = 1387	(0.08)	(0.15)	(0.15)
2-month sampling window	-0.04	-0.08	-0.08
n = 538	(0.13)	(0.27)	(0.27)
Both Nat. Experiments pooled - OLS			
12-month sampling window	0.12***	0.06	0.08
n = 6524	(0.03)	(0.07)	(0.07)
6-monthsampling window	0.09*	0.06	0.04
n = 2856	(0.05)	(0.11)	(0.11)
2-month sampling window	-0.01	0.02	-0.02
n = 1020	(0.09)	(0.19)	(0.19)
Both Nat. Experiments pooled - Tobit			
12-month sampling window	0.12**	0.07	0.07
n = 6524	(0.05)	(0.09)	(0.09)
6-monthsampling window	0.11	-0.01	-0.03
n = 2856	(0.07)	(0.13)	(0.13)
2-month sampling window	-0.06	0.01	-0.04
n = 1020	(0.11)	(0.22)	(0.22)

Note: Model 1 controls for date of birth and source country, Model 2 also controls for immigration date (discontinuity design estimator), and Model 3 additionally controls for the square of the immigration date (discontinuity design estimator). Source: Administrative German pension data; author calculations.

Tab. 2.21: Sample means for mortality estimates: men.

	Natural Experiment 1				Natural Experiment 2			
	Sample A		Sample B		Sample A		Sample B	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
Share who died 2006-2008	0.10	0.11	0.09	0.09	0.05	0.07	0.05	0.07
Age at retirement	59.63	59.62	60.32	60.69	61.10	60.70	60.96	60.84
	(1.31)	(1.51)	(1.85)	(1.95)	(2.05)	(2.25)	(2.11)	(2.25)
Date of retirement	1993.54	1993.44	1994.52	1994.72	2000.58	2000.08	2000.48	2000.11
	(1.35)	(1.52)	(2.23)	(2.29)	(2.82)	(3.16)	(2.92)	(3.13)
Pension payment in Euros	771.32	856.35	767.21	935.42	518.27	641.93	518.66	611.97
	(153.97)	(174.30)	(149.71)	(217.80)	(84.36)	(139.76)	(85.27)	(142.96)
Date of birth	1933.91	1933.82	1934.20	1934.02	1939.48	1939.38	1939.52	1939.27
	(1.19)	(1.21)	(1.26)	(1.26)	(1.74)	(1.74)	(1.73)	(1.74)
From Romania	0.26	0.41	0.28	0.44	0.01	0.05	0.01	0.09
From the former USSR	0.66	0.55	0.65	0.33	0.98	0.93	0.98	0.88
From another country	0.08	0.04	0.07	0.23	0.01	0.02	0.01	0.02
Number of observations	1,716	1,207	2,280	5,947	3,731	4,476	1,768	756

Note: For Natural Experiment 1, Sample A includes those who immigrated between July 1990 and December 1991 (we only observe the year of immigration, but the data include an implicit indicator for immigration before July 1990); Sample B includes those who immigrated in 1990 or 1991. For Natural Experiment 2, Sample A includes those who immigrated in 1996 (we distinguish between treatment and control groups based on an indicator of whether an individual was affected by the reform); Sample B includes those who immigrated between 1995 and 1997. *Source:* Administrative German pension data; author calculations. Standard deviations are given in parenthesis.

Tab. 2.22: Sample means for mortality estimates: women.

	Natural Experiment 1				Natural Experiment 2			
	Sample A		Sample B		Sample A		Sample B	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
Share who died 2006-2008	0.04	0.05	0.04	0.05	0.02	0.03	0.02	0.03
Age at retirement	59.59	59.39	60.17	60.33	60.23	60.05	60.23	60.19
	(1.15)	(1.40)	(1.91)	(2.10)	(1.43)	(1.61)	(1.44)	(1.52)
Date of retirement	1993.64	1993.46	1994.36	1994.36	1999.73	1999.40	1999.71	1999.48
	(1.37)	(1.61)	(2.15)	(2.32)	(2.17)	(2.35)	(2.23)	(2.32)
Pension payment in Euros	707.8	723.21	661.08	644.38	503.56	597.14	499.82	571.41
	(168.47)	(189.31)	(217.41)	(234.28)	(90.66)	(150.39)	(92.65)	(150.53)
Date of birth	1934.04	1934.06	1934.18	1934.03	1939.51	1939.35	1939.48	1939.29
	(1.19)	(1.18)	(1.25)	(1.24)	(1.70)	(1.73)	(1.70)	(1.74)
From Romania	0.21	0.38	0.25	0.44	0.01	0.06	0.02	0.10
From the former USSR	0.73	0.59	0.67	0.36	0.98	0.91	0.97	0.86
From another country	0.06	0.03	0.08	0.19	0.01	0.03	0.01	0.04
Number of observations	2,406	1,784	2,951	7,017	4,835	5,989	2,320	1,081

Note: For Natural Experiment 1, Sample A includes those who immigrated between July 1990 and December 1991 (we only observe the year of immigration, but the data include an implicit indicator for immigration before July 1990); Sample B includes those who immigrated in 1990 or 1991. For Natural Experiment 2, Sample A includes those who immigrated in 1996 (we distinguish between treatment and control groups based on an indicator of whether an individual was affected by the reform); Sample B includes those who immigrated between 1995 and 1997. *Source:* Administrative German pension data; author calculations. Standard deviations are given in parenthesis.

Tab. 2.23: Pension cuts and mortality: women.

Dependent variable	Sample A	Sample B
Natural Experiment 1		
Died in 2006-2008	0.002	0.002
	(0.007)	(0.005)
Log(net pension)	-0.068***	-0.110***
	(0.009)	(0.011)
Observations	4,190	9,968
Natural Experiment 2		
Died in 2006-2008	-0.014**	-0.003
	(0.006)	(0.003)
Log(net pension)	-0.141***	-0.172***
	(0.011)	(0.005)
Observations	3,089	9,785

Note: Sample B = immigrated in 1990 and 1991; Sample A excludes those who immigrated before July 1990 and those who retired after 1995. *Source:* Administrative German pension data; German Microcensus; author calculations.

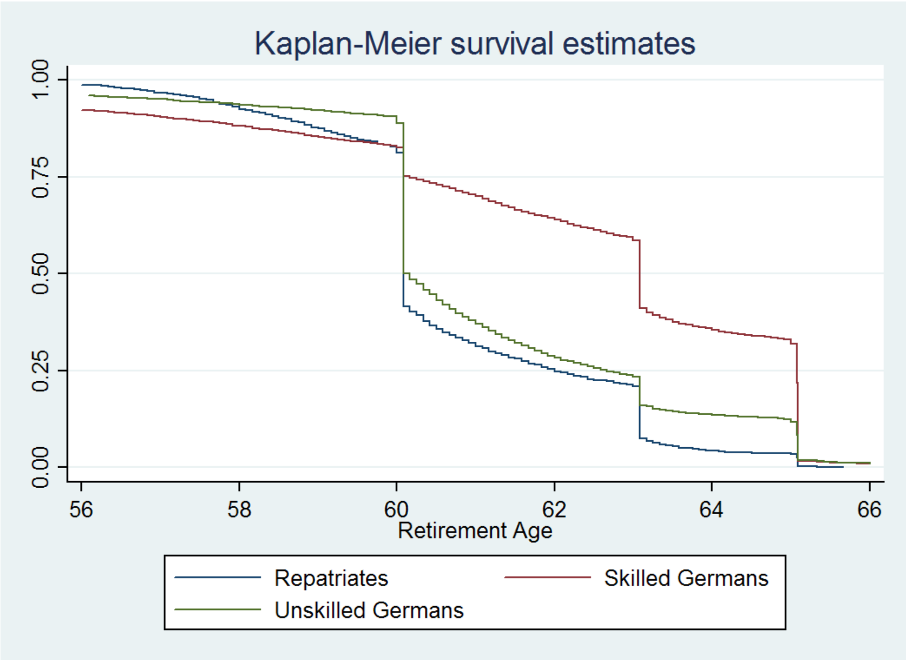


Fig. 1. Survival estimates for age at retirement: men. *Source:* Administrative German pension data; author calculations.

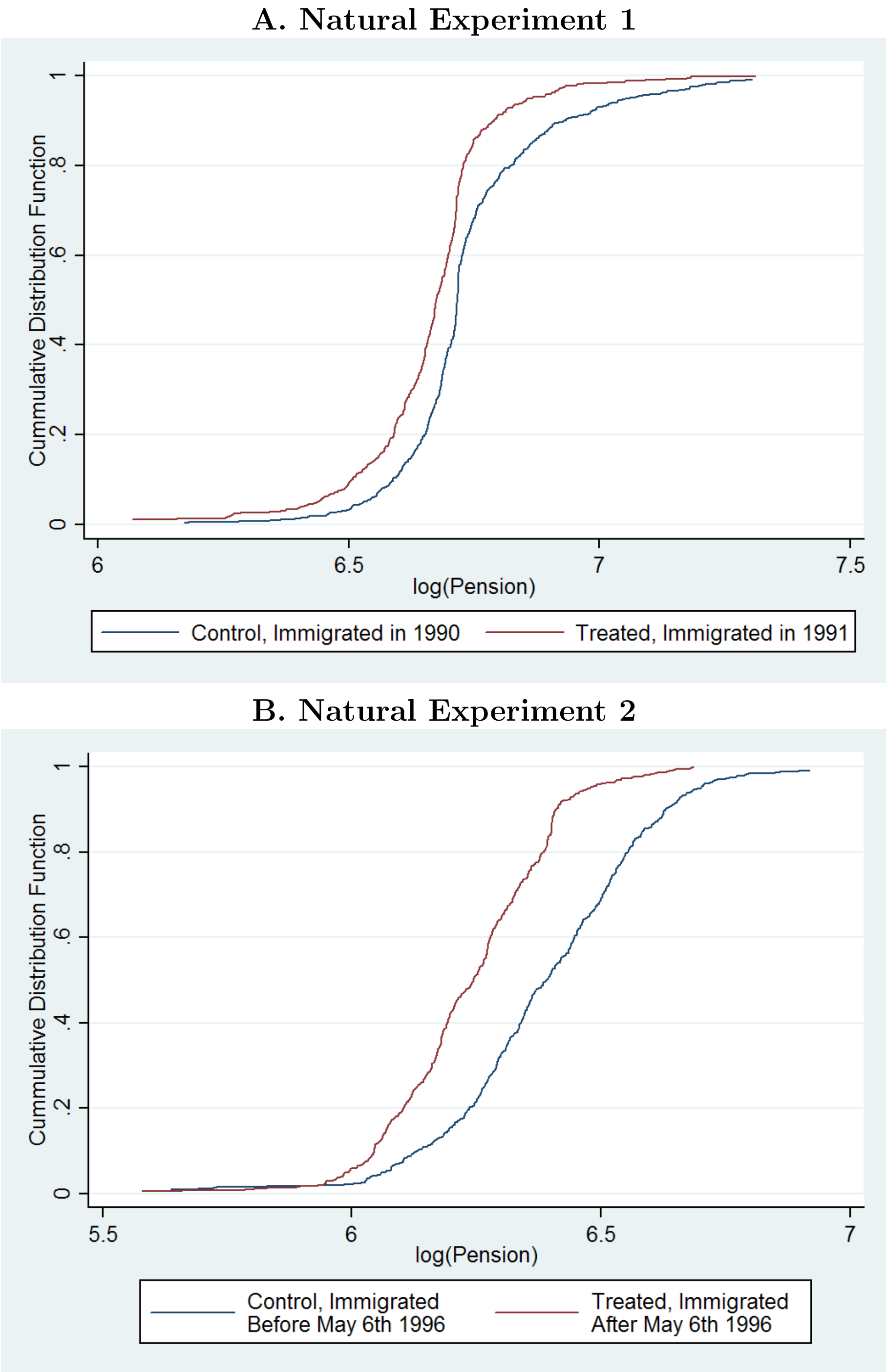
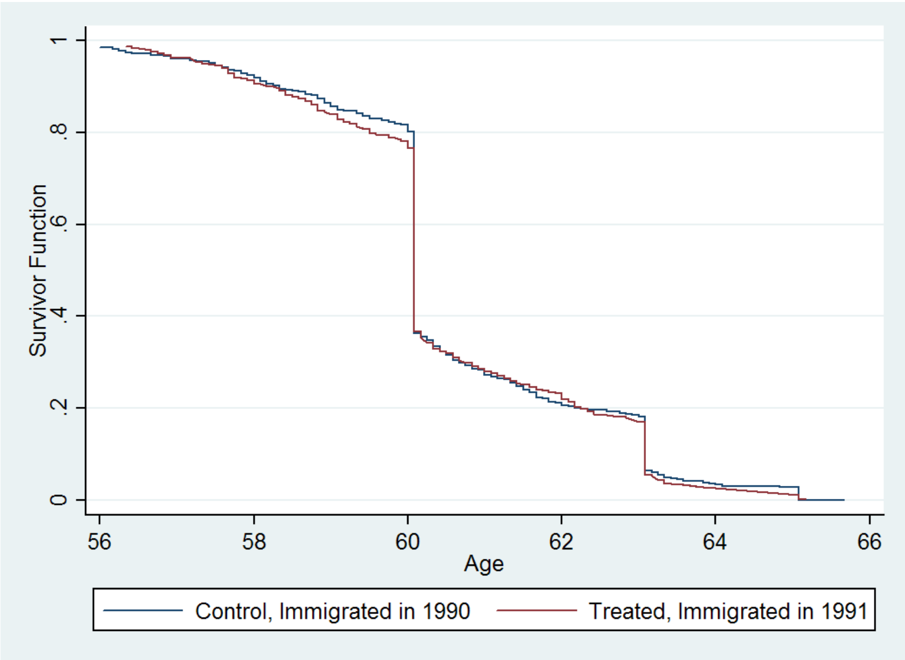


Fig. 2. Effects of pension cuts on the distribution of pension payments. The graphs are based on the data for the 6-month sampling window. *Source:* Administrative German pension data; author calculations.

A. Natural Experiment 1



B. Natural Experiment 2

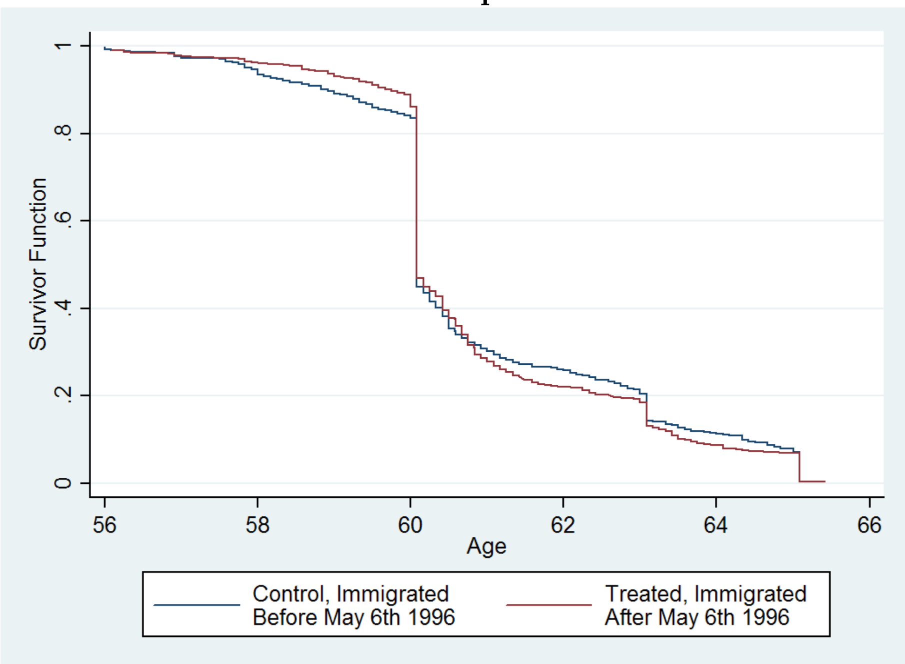
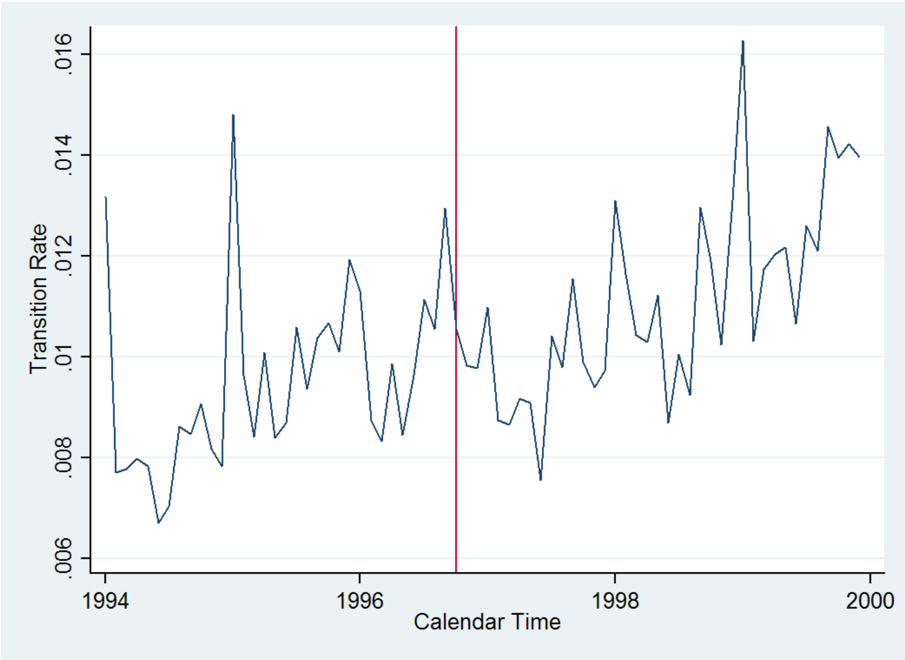


Fig. 3. Effects of pension cuts on retirement behavior. The graphs are based on the data for the 6-month sampling window. *Source:* Administrative German pension data; author calculations.

A. Treatment Group: Repatriated Ethnic Germans Not Affected By German-Polish Accord



B. Control Group: Repatriated Ethnic Germans Affected By German-Polish Accord

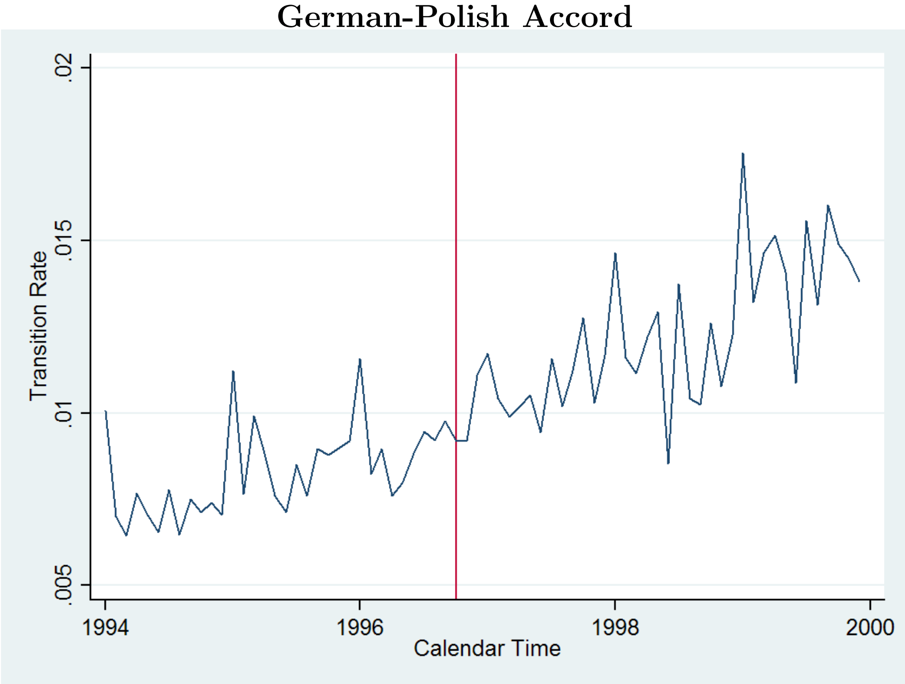


Fig. 4. Transition into retirement rates for repatriated ethnic Germans affected (Panel A) and not affected (Panel B) by Natural Experiment 3. *Source:* Administrative German pension data; author calculations.

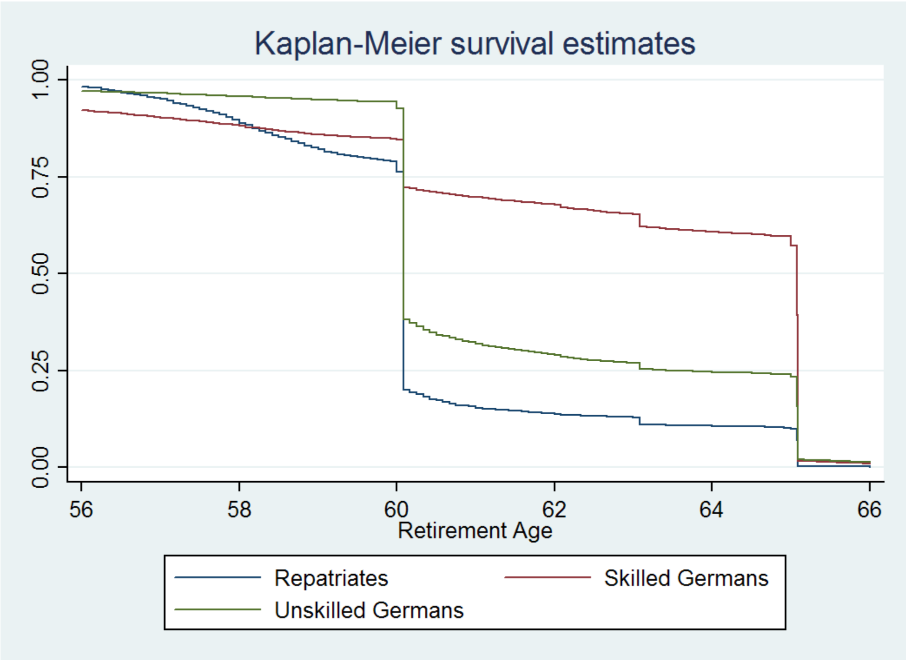


Fig. A1. Survival estimates for age at retirement: women. *Source:* Administrative German pension data; author calculations.

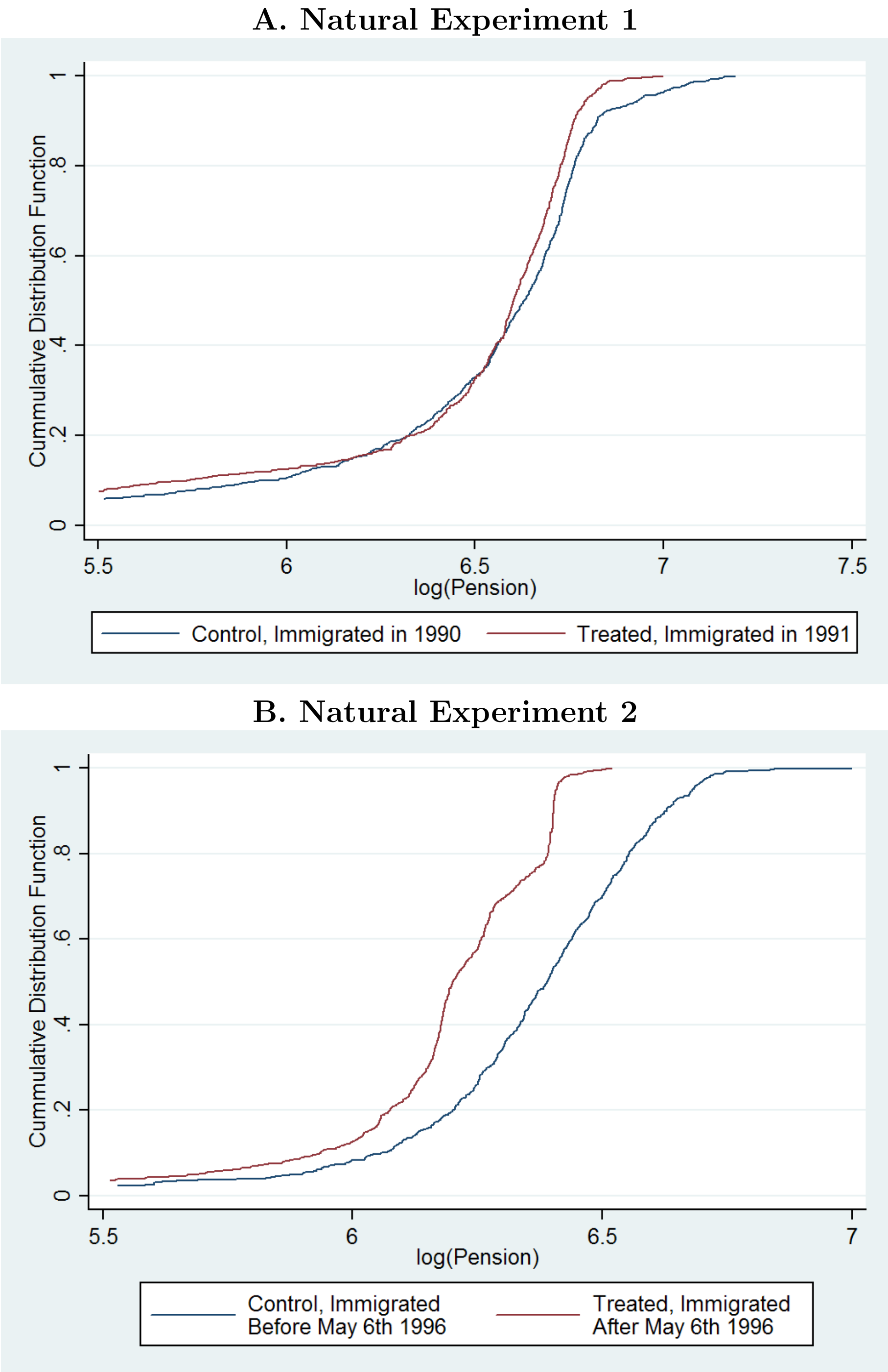
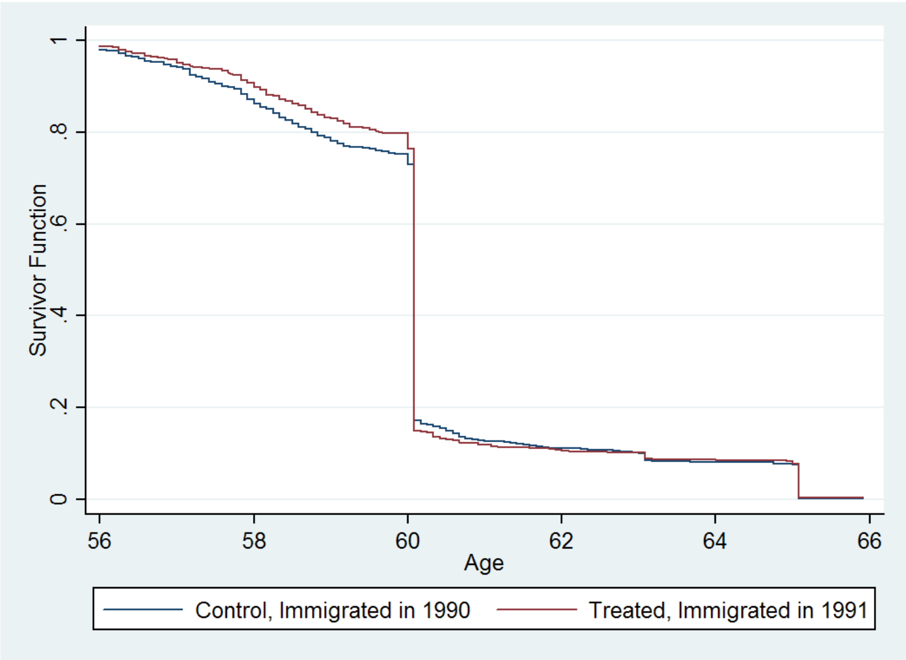


Fig. A2. Effects of pension cuts on the distribution of pension payments for women. The graphs are based on the data for the 6-month sampling window. *Source:* Administrative German pension data; author calculations.

A. Natural Experiment 1



B. Natural Experiment 2

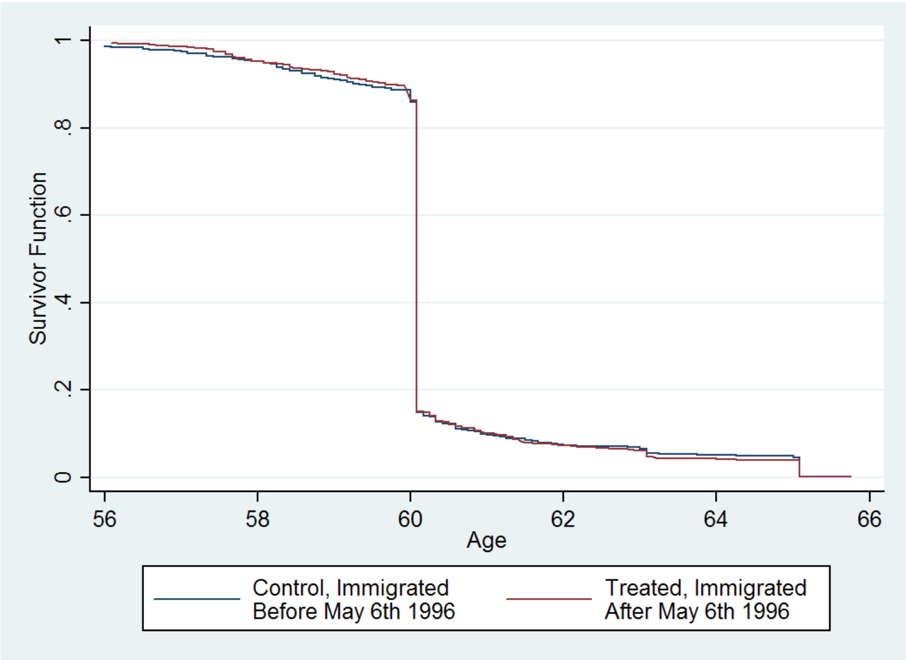
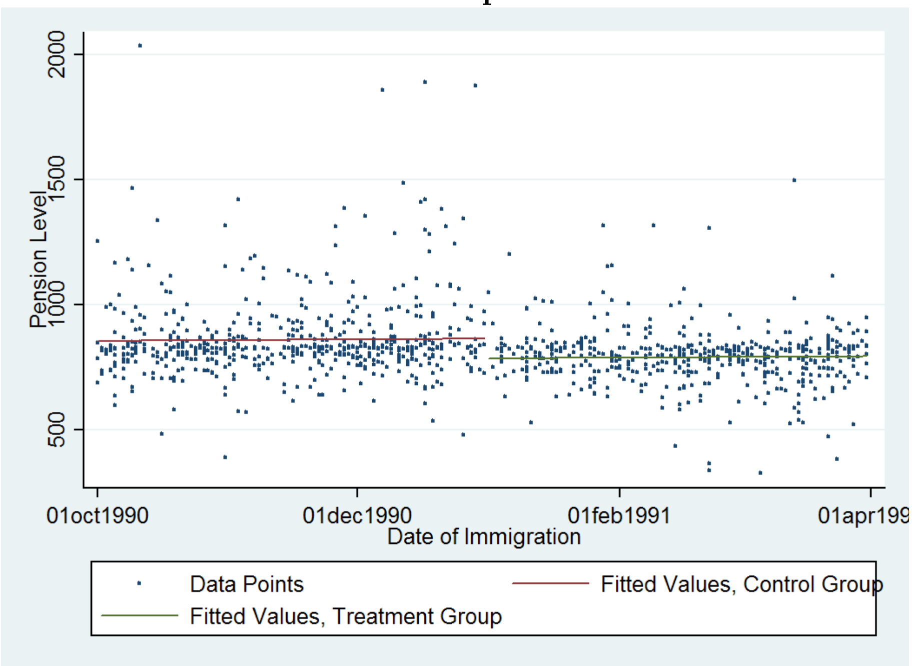


Fig. A3. Effects of pension cuts on retirement behavior of women. The graphs are based on the data for the 6-month sampling window. *Source:* Administrative German pension data; author calculations.

A. Natural Experiment 1



B. Natural Experiment 2

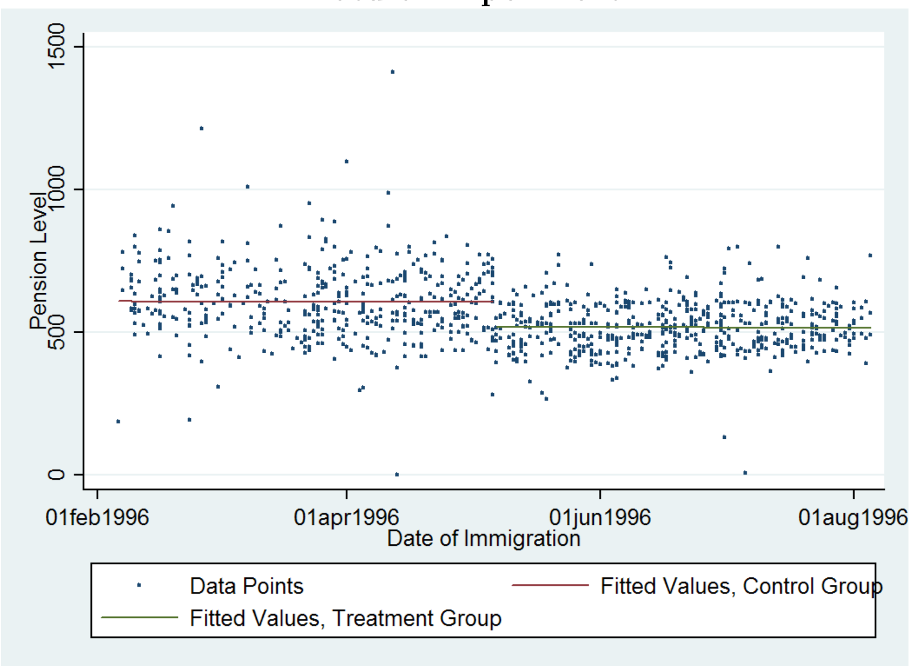
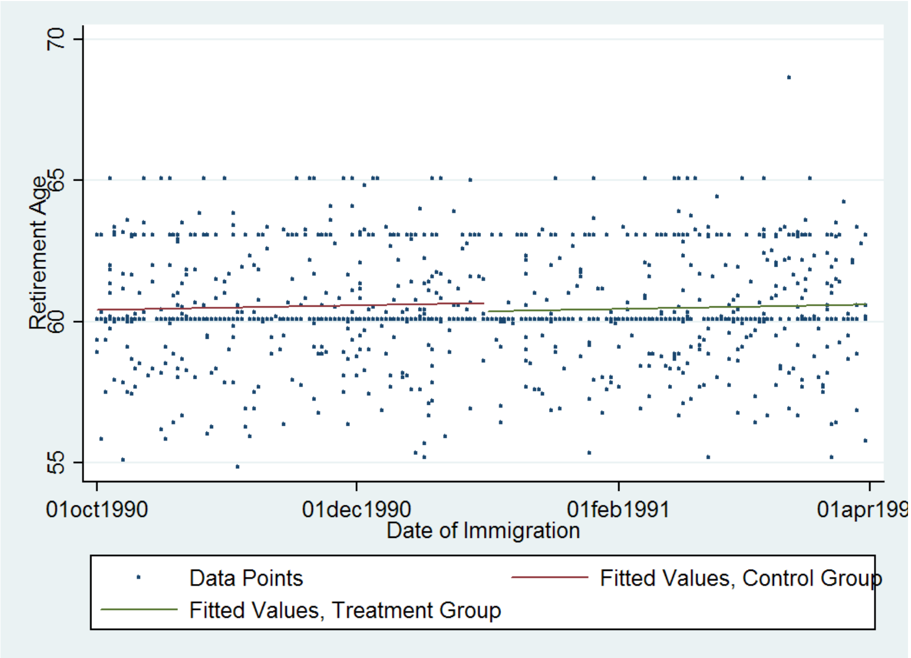


Fig. A4. Pension payments and date of immigration: men. The graphs are based on the data for the 6-month sampling window. *Source:* Administrative German pension data; author calculations.

A. Natural Experiment 1



B. Natural Experiment 2

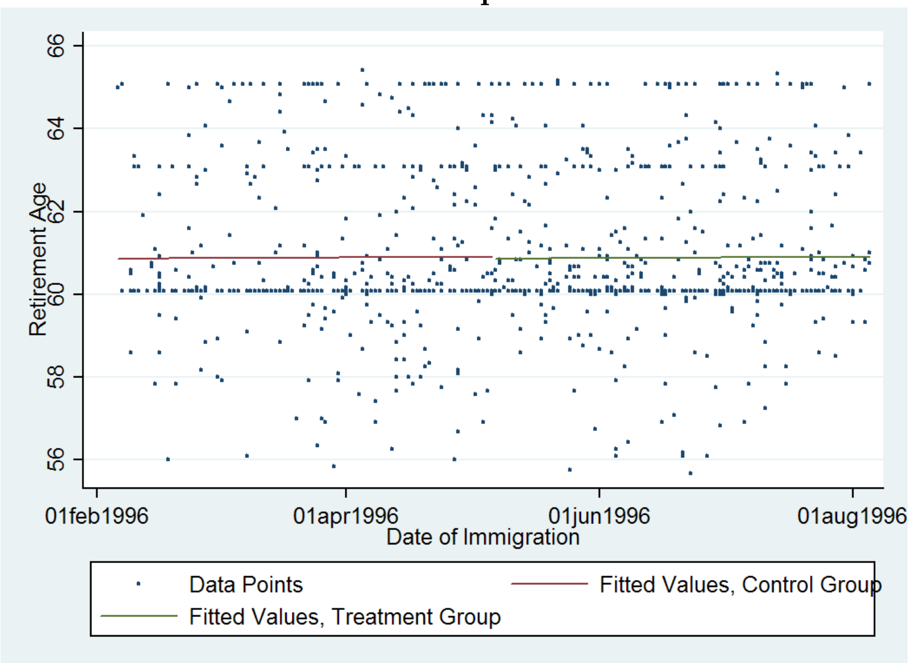


Fig. A5. Age at retirement and date of immigration: men. The graphs are based on the data for the 6-month sampling window. *Source:* Administrative German pension data; author calculations.

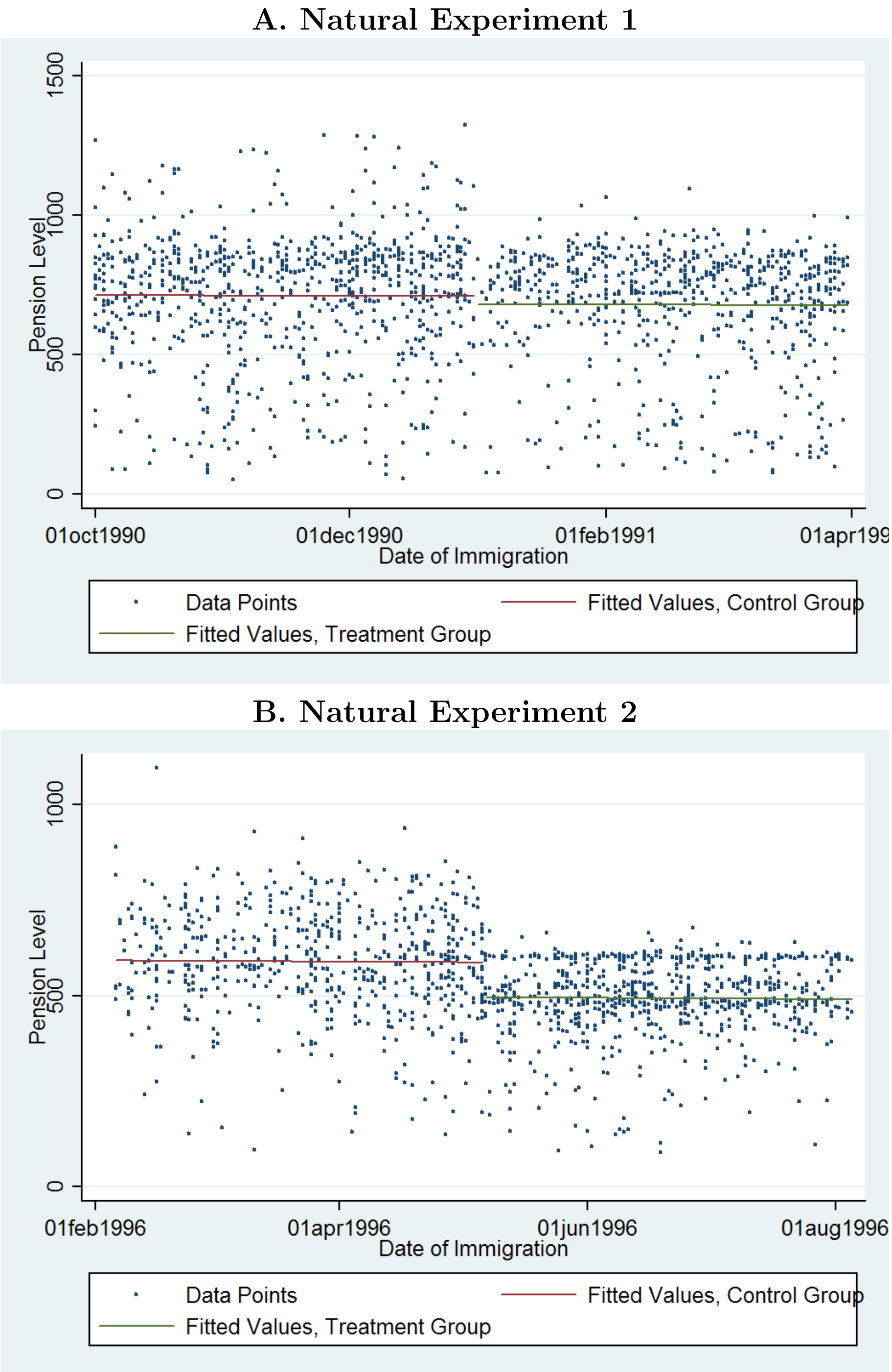


Fig. A6. Pension payments and date of immigration: women. The graphs are based on the data for the 6-month sampling window. *Source:* Administrative German pension data; author calculations.

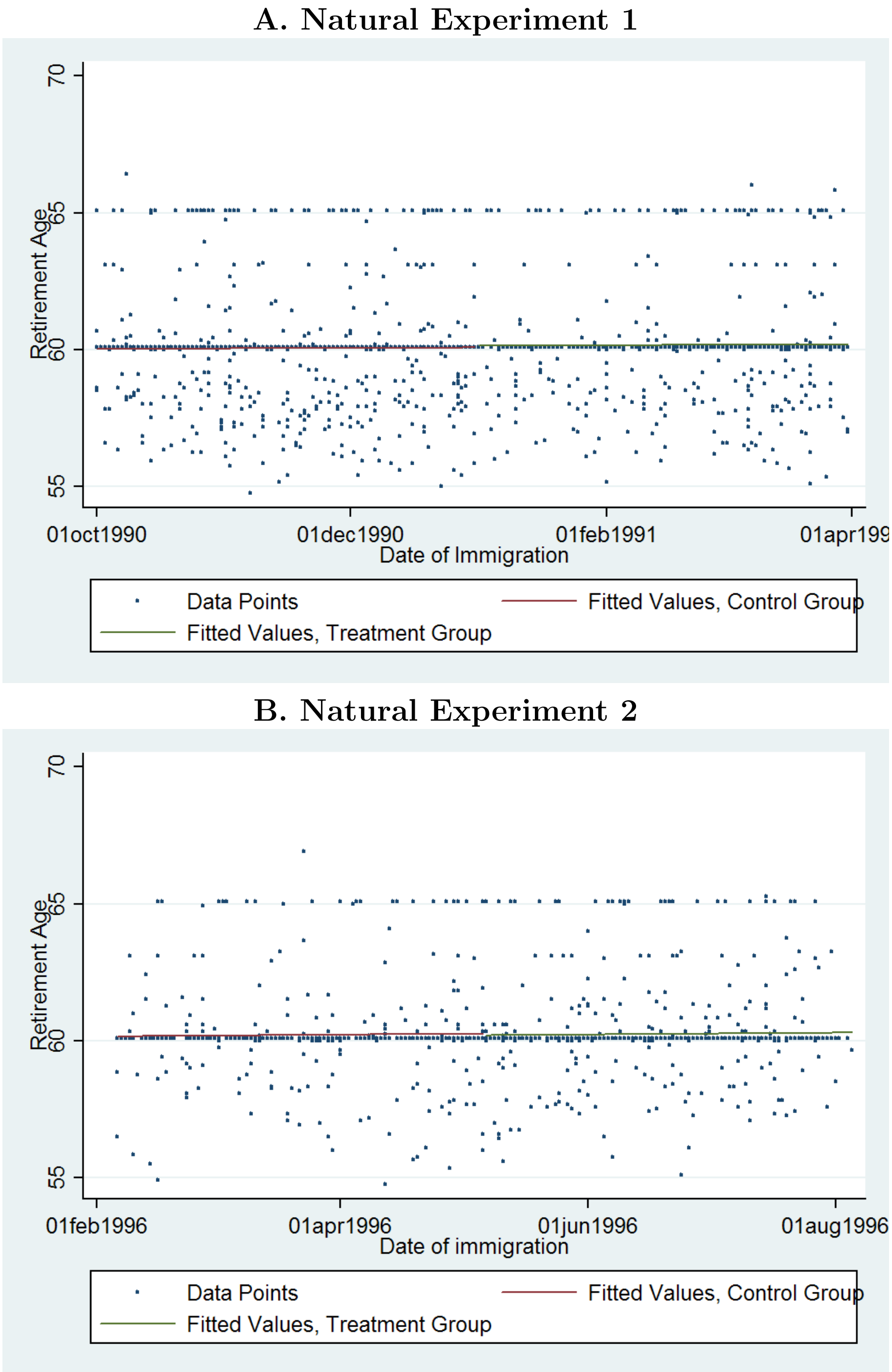


Fig. A7. Age at retirement and date of immigration: women. The graphs are based on the data for the 6-month sampling window. *Source:* Administrative German pension data; author calculations.

Chapter 3

A two-wave household panel survey of the population of a Togolese community, 2008-2011[1]

3.1 Introduction

This chapter describes a household survey that I conducted with a local team in a rural community in southern Togo in October 2008 and in January 2011. The major purpose of implementing the survey was to allow evaluating a preschool project which started in the studied community in between the two survey waves. Using the household data, chapter 4 will discuss the analysis of the short-run impact of the preschool program on time use of affected mothers. In addition to the choice of the community under study, the purpose of being able to evaluate the preschool project dictated most other aspects of the survey. In particular, the whole community was surveyed resulting in a dataset with 3615 (3541) individuals in 2008 (2011).[2] Furthermore, it had to be insured

[1]Part of this research was supported by the German Research Foundation (DFG) within the project 'Ein Paneldatensatz zur Analyse von Humankapitalinvestitionen im frühen Alter in ländlichen Regionen von Entwicklungsländern', and by the Leibniz Universität Hannover ("Forschungsfond").

[2]The survey included a second, smaller community, located about eight Kilometers south of the main community. 335 (351) individuals were interviewed in that village in 2008 (2011). The main objective for collecting data there was to gather information on an additional control group which could help identify the impact of the preschool program in the larger community. Since between the two waves of the survey the provision of public preschool education did not change in the smaller community (in fact, it had its own preschool since before 2008), differences in changes in behavior related to preschool enrollment between the two communities are likely to be attributable to the introduction of the preschool in the larger community. Unfortunately, as regards the evaluation of the preschool project discussed in chapter 4, the smaller community did not provide enough observations meeting the data requirements discussed there, in order to implement a difference-in-differences estimator exploiting differences between the communities. To ease the presentation of the survey data, answers from households in the smaller community are not included for all calculations and figures presented in this chapter, and all further explanations concentrate on the main community.

that individuals in the dataset are uniquely identifiable in order to match observations between survey waves, and to link individuals to the preschool project. Finally, and most importantly, the choice of topics covered by the survey was made with the evaluation of the preschool project in mind.

As illustrated by table 3.1, this implied covering a wide range of socio-demographic characteristics of all household members, their education, labor supply, agricultural activities, health status, time use, and cognitive ability. The order of topics in the table reflects the order of modules in the 2011 questionnaire which is shown in the general appendix 6.2 (the 2008 questionnaire is shown in general appendix 6.2). It sketches the main purpose for collecting information on each of the subjects, stating whether it is seen as providing outcome or explanatory/control variables for the empirical analysis, and giving examples for hypothesized relationships. Table 3.1 already illustrates that, despite the originally relatively narrow purpose, the survey is designed broadly enough such that it allows studying a wide range of issues. Accordingly, an exploitation of all benefits of the data is beyond the scope of the analysis presented in chapter 4. It rather leaves opportunities for future research.

Questionnaire design consisted of translating the topics listed in table 3.1 into feasible and consistent survey questions. Many parts of the questionnaire were straightforward to implement and are rather self-explanatory, and they will not be discussed further in this chapter. A starting point for the design of many of these rather standard elements of the questionnaires have been questionnaires used for conducting Living Standards Measurement Study Household Surveys (LSMS) in various countries[3]. Other inspirations included Weinert et al. (2007) and Kurth (2007) for the construction of the module assessing personality traits and physical development of children; questions pertaining to trust in other people as well as prejudices towards minorities have been adapted from the World Values Survey, particularly questions A124, A165, and A168 according to the integrated questionnaire for the 1981-2008 surveys that is published on the World Values Survey homepage.[4]

Apart from these rather standard sections of the questionnaire, other modules de-

[3]A general reference to LSMS surveys is provided by Grosh and Glewwe (1998)

[4]http://www.asep-sa.org/wvs/wvs_1981-2008/WVS_1981-2008_IntegratedQuestionnaire.pdf. Date accessed: January 16, 2012.

serve greater attention. Among them, three required particular effort for their design: time use, income sources, and cognitive skills. All these modules have in common that the data obtained from them require transformations before they can be used for analysis. Accordingly, this chapter elaborates on what motivated the specific designs of these questionnaire sections, and how their results are interpreted. The time use module (section 3.1.1) meets the requirement of adequately measuring intensive labor supply and capturing child care arrangements while being lean enough not to consume too much time during interviews. The results regarding labor supply form the time use model are fairly consistent with those obtained from the employment module. The approach taken in the income sources module (section 3.1.2) was to valuate the output of households' farms and non-agricultural enterprizes. Its results are, together with salaries obtained in the employment section, consistent with the households' own coarse evaluation of income, but they are likely to provide a more accurate ranking of households with respect to their financial opportunities. Finally, the cognitive skills module (section 3.1.3) implemented a stripped-down version of what could be called a psychological test of cognitive development. As opposed to many psychological tests, the goal is not to break down cognitive ability into subscales. Rather, the single composite score obtained from this test, which took 10 to 15 minutes per child in its 2011 version, yields a reliable measure of ability that is correlated with cognitive achievement.

Section 3.2 shortly characterizes the community in which the survey was carried out. Section 3.3 describes particularities of the survey implementation, where one of the greatest difficulties was to ensure that it would be possible to match individual observations from both waves of the survey. A good overall data quality was achieved, leading to relatively few missing responses. Since the number of individuals who were eligible for particular questions varied between questionnaire modules, the response rate cannot be expressed as a single number. However, to give a few examples, the share of illegible individuals in 2011 who responded to basic questions regarding their schooling (number of completed school grades, school diploma obtained, number of years spent in primary school, number of years spent in secondary school, last school attended) varied between 91 and 98 percent. Even for more demanding sections of the questionnaire, response rates were still acceptable. For instance, 95 percent of eligible individuals responded to the time use questions, and 92 percent of 3- to 14-year old children took part in the cognitive tests.

Tab. 3.1: Questionnaire modules and their purpose

Module	Purpose	Example/explanation
Demographic information	Control	Sex and age are most likely correlated with different uses of time; religion and ethnicity may capture heterogeneity in preferences
	Technical	Information used to uniquely identify individuals and link observations between survey waves (see section 3.3)
Fertility	Control	Reflects fundamental differences that may affect schooling choices; recent fertility affects time use of mothers
Schooling	Outcome	Does having attended preschool affect the likelihood of enrollment in primary school?
	Control	Education of parents affects schooling choices they make for their children as well as their labor market opportunities; schooling expenditures as a measure of parental investments human capital
School choice (2011)	Outcome	Do students self-select into schools?
Employment/ working hours/ months worked/ frequency of going to work	Outcome	How do childcare arrangements affect labor supply of cohabiting adults?
	Control	Accounting for previous labor supply accounts for individual heterogeneity in models of current labor supply
Occupation (including information on household enterprizes)	Control	Determinant of income; determinant of time use
Place of work	Control	Determinant of time use
Time use	Outcome	How do childcare arrangements affect time use of cohabiting adults?
Cognitive skills (adults)	Control	Potential determinant of schooling choices as well as productivity/labor market opportunities
Agriculture	Outcome	How does caring for a child while working affect productivity?

	Control	Determinant of income
Household infrastructure/ ownership of production and luxury goods	Control	Approximate household wealth as an alternative to measuring income; determinant of health status
Access to schools	Control	Determinants of enrollment decisions
Transfers from other households	Control	Determinant of income (remittances)
Recent deaths in family	Control	Potentially measures exogenous negative income shock (funeral expenditures, loss of labor income); may also capture health shocks to the whole family
Opinions regarding child care arrangements/ labor supply of women/ mothers	Control	Capture preferences that potentially determine both enrollment and time use decisions
Opinions regarding NGO/ preschool	Control	Determinants of preschool enrolment
Opinions regarding trust and attitude towards minorities	Outcome	Does being exposed to peers from different religions and ethnicity reduce prejudice?
	Control	Determinants of participation in a public project
Household work of children	Outcome	Does early enrollment reduce the likelihood of children to be working in the household in the future?
Personality traits	Outcome	Does preschool education affect children's personality?
	Control	Determinants of enrolment decisions and learning behavior
Child health/physical development	Outcome	Do child care arrangements affect the physical development of children?
	Control	Determinant of enrollment and learning behavior
Cognitive development of children	Outcome	Does preschool education affect cognitive development of children?
	Control	Determinant of enrollment and learning behavior
Direct measures of income	Control	Determinant of investments in human capital

3.1.1 Time use

The research questions which motivated the implementation of the household survey are intimately related to different uses of time, most importantly, work and child care. More generally, measuring the intensity of human capital investments and more accurately measuring labor supply in contexts where most labor is informal has frequently been the objective of surveys designed to capture time use (Harvey and Taylor (2000)). Furthermore, economic theory provides hypothesis that could be tested by use of time use surveys.[5] In addition, accounts of time use have been the subject of household surveys in many fields (see Gross (1984) for an example from anthropology) for a long time. Still, the empirical analysis of the determinants of different uses of time based on time use data is still a relatively young field (Hamermesh and Pfann (2005)). In particular, analysis of time allocation of women in developing countries are very rare.[6]

While acknowledging that measuring time use is important for a number of economic research questions and for the analysis of child care arrangements in particular, it might be argued that, in a very simple form, measures of time use could be easily implemented in household surveys by asking respondents, for example, to indicate the number of hours per week they spend with work or child care. However, for a number of reasons, researchers have found it to be advantageous to implement questionnaire modules which allow a more thorough assessment of time use where the objective is to fully account for individuals' activities throughout a given time period (Harvey and Taylor (2000)). Three arguments are particular relevant to the analysis here.

First, a separate time use module may be better designed in order to reduce to probability that respondents have difficulties to accurately recall their use of time. In the Togolese survey, as a means to help respondents in recalling timing and duration of their activities, interviewers guided them through the day so that respondents could indicate beginning and end of an activity such that the length of the activity could be deduced.

[5] Becker (1965) and Leibowitz (1974) provided foundations for the economic analysis of the allocation of time.

[6] Malathy (1994) constitutes an exception.

Second, a time use module may come closer to fully accounting for time uses of all household members. If, for instance, time use information is only gathered in specific sections, then there will be no information on time use available for individuals for whom these sections do not apply. For example, if only hours of work are measured (in the work module), then there would be no information on time use for individuals who do not work. Regarding the research questions central to the project, this would preclude analyzing the impact of child care arrangements on mother's activities other than working as well as studying the impact of children's time use (e.g. the number of hours they spend at home) on other household members' time use.

Third, contextual information such as location as well as information on concurrent activities are more easily recorded in a separate time use module. This quality is particularly useful regarding research questions related to issues of child care arrangements (Harvey and Taylor (2000)): For instance, it will allow to study determinants and consequences of mothers caring for children parallel to other activities as well as mothers and children spending time either at home or outside the home. In fact, given such flexible forms that child care arrangements can take, economists recognized long ago that accurately measuring how much time individuals devote to child care is hard to measure outside tailored time use modules which usually are not included in household surveys (Hill and Stafford (1980)).

In light of these considerations, the Togolese surveys had to reconcile two opposed goals. On the one hand, it had to be detailed enough in order to provide useful measurements of labor supply and child care arrangements. On the other hand, it had to be succinct just not to consume too much time during interviews and not to overstrain interviewers and respondents. A stylized activity log simpler than the one proposed by (Harvey and Taylor (2000)) was implemented, where the main differences are that the set of activities to choose from is less detailed, and that the number of distinct time periods per day, for which activities are recorded separately, is reduced.[7].

Regarding the activities captured by the time use module, several types of activities were precoded, and individuals were asked to indicate how many hours within

[7] A more complete picture regarding time use may be given by more complex modules, for example, what Harvey and Taylor (2000) call an open interval time diary. One additional difficulty associated with such modules are the considerable complications that they add to data entry.

a given time period on an average weekday[8] they spend with activities falling under each of these categories. These activity groups were: going to school, doing homework/studying at home, work as an apprentice, doing household work/chores, working in a work shop/shop/etc. outside the home, working at home, running errands, doing nothing, other activity.[9] This seemingly short list of activities, including a residual category of "other activities", already permits reliably retracing the course of a typical day for the largest part of any population, and measures of intensive labor supply based on these questions can be assumed to be fairly accurate. Moreover, it allows studying the impact of social programs such as the preschool project described in chapter 4 on a whole range of outcomes for all household members, which would not be feasible using data from a regular household survey. Thus, despite its simplicity relative to other time use surveys that have been conducted in the past, the module used for the Togolese survey adds significantly to the opportunities of investigating the research questions which motivated it.

As regards the number of distinct time periods per day implemented in the time use model, a much higher number was chosen in 2008 than in 2011, which leads to the quite different appearances of the sections in the two years. The 2008 questionnaire distinguishes between periods of 30 minutes within one day whereas the 2011 questionnaire only distinguishes between the two halves of one day (where up to two activities per half-day and the length of the major activity are recorded, and, in addition, the number of hours caring for a child). Effectively, however, they do measure time use in almost the exact same way. A higher number of periods within a day has the virtue of capturing both the incidence and the timing (meaning the temporal location) of activities more accurately. Evidently, this is most relevant in cases where individual behavior is such that one typically observes many relatively short spells of activities within one day. At the level of aggregation that was chosen for activity classification, though, the spells of activities that can be observed in the 2008 Togolese data are rather long. In other words, only in rare cases more than one activity spell related to work or educa-

[8] In 2008, respondents were actually asked to indicate what they do on an average weekday. In 2011, each respondent reported his activities for two weekdays of the previous seven days, and the average responses for an individual is interpreted as describing his time use on an average weekday.

[9] The number of precoded activities was smaller in the 2008 questionnaire. However, in 2008, a separate row in the questionnaire recorded the place of an activity. Combining these infirmations allows to construct activity categories analogous to the ones used in 2011.

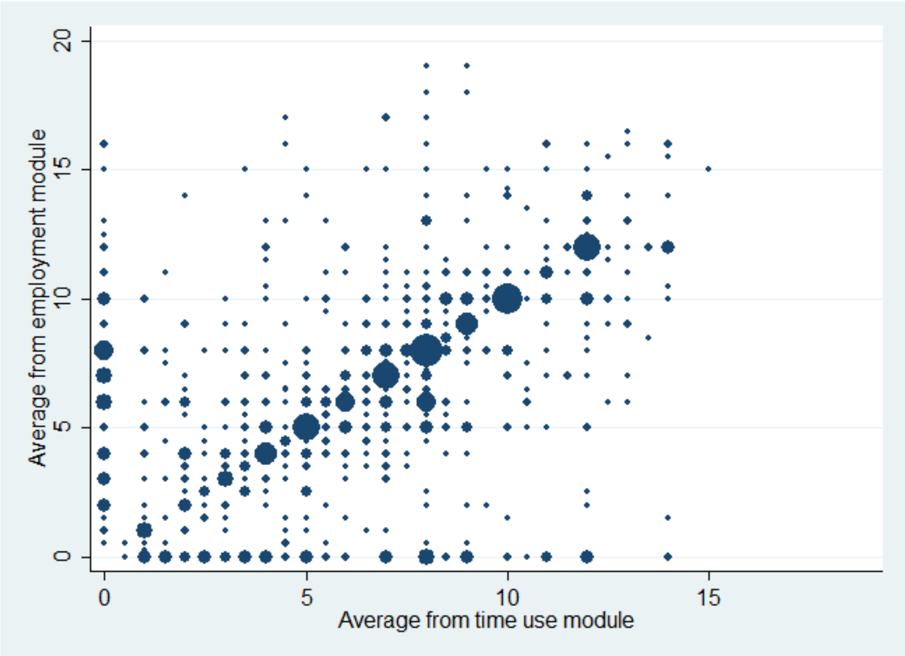
tion fall within one half of a day. Thus, regarding the measurement of timing and the length of the major activities, a module distinguishing between periods of 30 minutes within a day and a module distinguishing between the two halves of a day are, by and large, performing equally well.[10] Accordingly, this simplification was implemented for the 2011 survey, which tremendously reduced the amount of time necessary for filling out the time use section during interviews.

While one of the motivations given above for implementing a time use module was to obtain more accurate measures than would be obtained by, for example, simply asking for hours of work within the occupation module, the latter was done as well, because it is not very costly to do so. This allows for cross-validating measures for the length of various activities obtained from both the time use module and other sections of the interviews. Significant correlations between the measurements would indicate consistency of the responses, increasing confidence in that they capture what was originally intended to be measured. As figures 3.1 and 3.2 show, measures of labor supply computed based on the two different modules reveal a consistency of answers for a very large share of individuals, where the two measures of working hours are either identical or very close to each other. However, there are also deviations, as indicated by observations lying far off the 45-degree-line in these figures. This result highlights that there are potential gains in accuracy from implementing a time use module. While it is not valid to presume that the measure obtained from the time use module always is more accurate, having both measures adds flexibility for empirical analysis, which can, for example, be exploited by investigating the robustness of empirical results to the choice of the labor supply measure. Figure 3.3 makes a similar comparison regarding hours spent with homework. It shows that the duration of homework according the time use module is systematically shorter. This may either imply that individuals exaggerate if they are asked to indicate the time they spend doing homework without being confronted with the context of a whole day's activities. On the other hand, the choice of reducing the distinct time periods per day in the time use module may have

[10] In addition, issues related to the temporal location of activities, which have been the explicit focus of other time use surveys (Harvey and Taylor (2000)), are not particularly relevant to the research questions that the Togolese survey has been designed for. For instance, when evaluating the impact of institutional child care on mothers' labor supply (see chapter 4), there is no relevant variation in temporal location within such a social program that can be exploited, because the working hours of schools and child care institutions are identical for everyone who is affected.

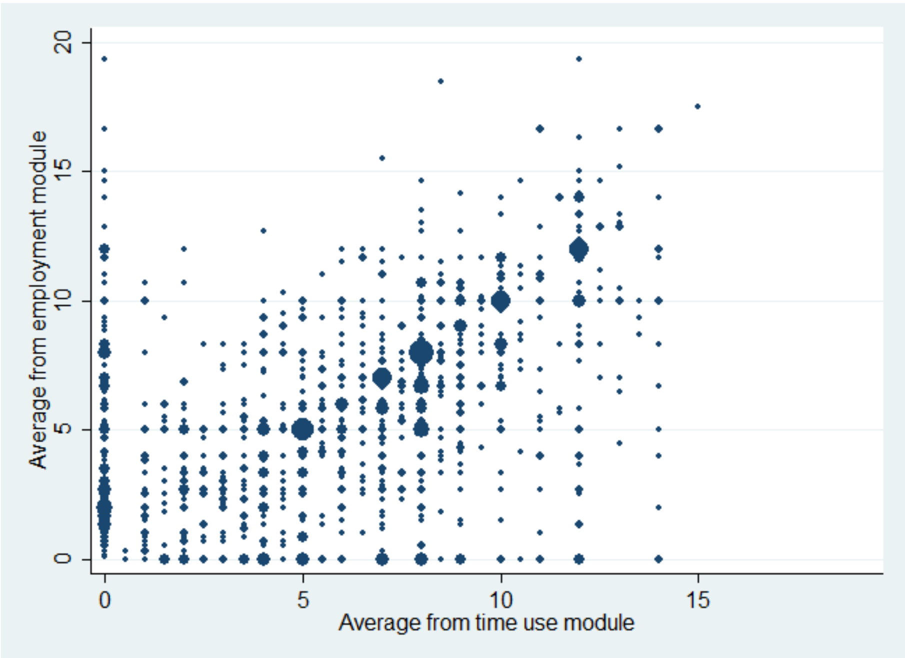
resulted in underestimating the incidence of relatively short activities such as homework.

Fig. 3.1: Average daily working hours from employment module vs. average daily working hours from time use module



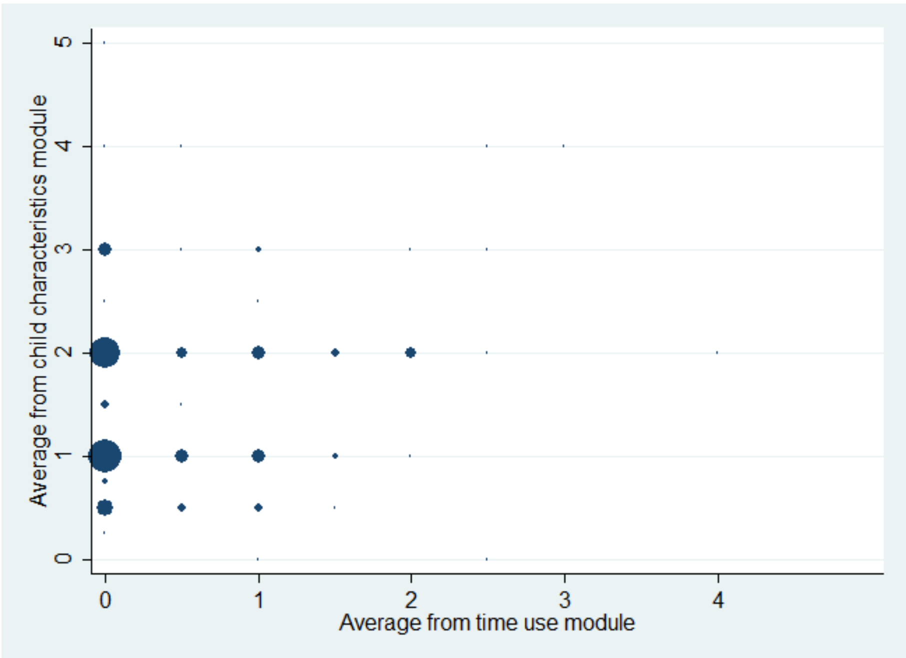
Sample: Individuals from the household survey in 2011 with non-missing data on working hours in both the employment and the time use module who indicated to have an occupation, and who reported more than zero working hours per day in either the employment or the time use module; 14 observations have been dropped because they were considered to be outliers (these were responses according to which individuals claimed in the employment module to work, on average, 20 hours or more per day); N=999. The size of markers in the graph corresponds to the frequency with which the respective combination of values for average working hours from the two questionnaire modules is observed in the data. The correlation between the two variables is equal to 0.44. If only observations are included where more than zero working hours are indicated in both the employment and the time use module (N=811), then the correlation coefficient is equal to 0.64.

Fig. 3.2: Average working hours per day implied by weekly working hours from employment module vs. average daily working hours from time use module



Sample: Individuals from the household survey in 2011 with non-missing data on working hours in both the employment and the time use module who indicated to have an occupation, and who either reported more than zero working hours per week in the employment or more than zero daily working hours in the time use module; N=1062. The size of markers in the graph corresponds to the frequency with which the respective combination of values for average working hours from the two questionnaire modules is observed in the data. The correlation between the two variables is equal to 0.46. If only observations are included where more than zero working hours are indicated in both the employment and the time use module (N=860), then the correlation coefficient is equal to 0.59.

Fig. 3.3: Average hours of homework per day from child characteristics module vs. average hours of homework from time use module



Sample: Individuals from the household survey in 2011 with non-missing data on homework hours in both the child characteristics, who either reported more than zero homework hours per day in the child characteristics or more than zero daily homework hours in the time use module; N=723. The size of markers in the graph corresponds to the frequency with which the respective combination of values for average working hours from the two questionnaire modules is observed in the data. The correlation between the two variables is equal to 0.11.

3.1.2 Measuring income

It was considered important to this project to assess the economic situation of households in order to be able to construct useful control variables for the empirical analysis which account for significant heterogeneity associated with the outcomes under study. For instance, income can affect the demand for education, particularly if households tend to be credit constrained (Lochner and Monge-Naranjo (2011)). Unfortunately, reliably assessing household's income and living standards constitutes one of the biggest challenges to all household surveys conducted in developing countries, particularly in Sub-Saharan Africa (Stifel and Christiaensen (2007)). The approach taken in this project was to focus on measuring salaries, and, most importantly, output from the households' farms and from non-agricultural enterprizes (in other words, partly measuring the valuation of factor incomes)[11]

One major difficulty that makes measuring income in developing countries so complicated is that it is often very uncommon for respondents to keep accounts of economic activities. Accordingly, when being asked to indicate profits from farms or household enterprizes, individuals usually do not have the information necessary to indicate the respective amount right away. In addition, individuals may be reluctant to reveal information on the amount of their income (McKay (2000)). Consequently, simply asking individuals to indicate their income is not feasible.

An alternative approach used very frequently has been to measure total consumption of households (Deaton (1997), Lipton and Ravallion (1995) as cited in McKay (2000)) as a measure of living standards. The idea is that in developing countries, income sources a very volatile and subject to strong seasonal effects, and it is argued that consumption is a measure that better reflects long-run resources or permanent income (Ghuman et al. (2005)). However, that approach was not suitable for this project either, because it is unreasonably costly. I rather chose to approximate a valuation of factor incomes which was possible to implement by adding only relatively few additional survey questions, mostly to modules that would have existed anyway (e.g. employment). Measuring consumption, on the contrary, would have required a large module on its own.[12] As

[11]These three sources are expected to explain the largest share of incomes. Other parts of the questionnaires that are also related to the measurement of income include a question on whether a household has received transfers from another household.

[12]Moreover, there was no need to measure consumption *in addition* to income, because the project

regards income from agricultural and non-agricultural enterprizes, the approach was further simplified by focusing on the measurement of output rather than revenues from these enterprizes, thus avoiding the measurement of production inputs.[13] It is reasonable to assume that, in the studied community, labor is by far the most important input to production for agricultural and non-agricultural enterprizes. Thus differences in farm output will mainly reflect differences in the amount of labor input and in the productivity of labor. The survey assesses the labor supply of all household members as well. Combined with this information, measuring household enterprizes' output will allow to appropriately group households according to their financial opportunities.

Assessing the salary of employees was straightforward to implement in the questionnaires. Salaries are recorded in the employment module, and they have been standardized to CFA per week. Measuring the output from household enterprizes, however, required the combination of information from several survey items with external sources of information.

In particular, the survey data from the agriculture module[14] is combined with additional information on prices, and information from the non-agricultural household enterprize module is matched with external data on average revenues. The price data must also include information on the conversion of local units of measurement into the units for which price data are available (in many developing countries, particularly in West Africa, respondents will only be able to indicate quantities of agricultural goods in local units, Jolliffe (1995) as cited in Reardon and Glewwe (2000)). During the implementation phase of the 2011 survey, two project staff members interviewed informed citizens on prizes of agricultural goods and average revenues of entrepreneurs. Respon-

did not aim at computing household savings.

[13]Many household surveys in developing countries dedicate a large share of their questionnaires to the accurate measurement of agricultural revenue by assessing inputs, outputs and other determinants. Often, the objective is to be able to estimate agricultural production functions or to investigate issues of intra-household distribution (Reardon and Glewwe (2000)), which are both not the focus of this project.

[14]Additional decisions that had to be made when designing the agriculture module concerned the level of aggregation at which farm output is measured as well as the choice of the recall period. Regarding the level of aggregation, the survey followed the recommendation to gather agricultural information on the plot level (rather than the household level) in order to help respondents recalling the correct amounts of output (Reardon and Glewwe (2000)). As regards the recall period, the studied community is part of the subhumid savanna zone with one agricultural season. Accordingly, choosing a recall period of one year for the agricultural module is appropriate. A different recall period may be warranted for regions with more than one agricultural season per year, because respondents might have difficulties taking into account more than one harvest at a time.

dents were chosen by neighborhood chiefs who were asked to indicate a neighborhood community member whom they judged to be best informed on the local economy. Interviews with 11 experts were completed.

In order to compute the value of an individual's agricultural output, all entities of what was harvested on any fields an individual was responsible for were multiplied with the respective good's price as determined by averaging the prices the experts' had indicated. In order to valuate a non-agricultural household enterprize, the experts' average responses regarding revenues were simply matched to respective types of enterprizes and multiplied by the fraction of the year prior to the survey during which an enterprize was operating.

Similar to the strategy followed when measuring labor supply as described in section 3.1.1 (where it was motivated why labor supply is measured in both the time use module and the employment module), the survey also included questions directly asking respondents to reveal their income. In spite of the limited reliability of such information, it was gathered because only few questions were required to do so, and in order to exploit them as benchmarks for the alternative measures of income just described. At two occasions, respondents were asked directly to reveal information on their income. First, they were asked to indicate what someone would have to pay them such that they ceased all other economic activities in order to work for that person (called "reservation wage" in what follows[15]). Second, household heads were asked to indicate the household's yearly income.

In order to determine whether the measures of income components discussed above (salaries and output of agricultural and non-agricultural household enterprizes) succeed in capturing actual heterogeneity in income, I calculated correlations between these measures and the responses to the questions asking for household income and for reservation wage. A positive correlation between indicated household income and the measures of income components would suggest that all these variables share a common

[15] While answers to this questions may partly reflect preferences for job characteristics other than salary or revenue, respondents will, presumably, also partly reveal what they estimate to be their current income. Note that the survey question is designed such that the respondent is supposed to evaluate his current utility level. This is different from similar questions in other surveys which ask respondents to indicate their reservation wage for accepting an employment offer in a hypothetical situation of being unemployed, regardless of whether they currently are unemployed or not (Grogger (2009))

component that does reflect heterogeneity in income.

Table 3.2 gives pairwise correlation estimates between indicated household income[16] and each of the three income components (on the household level). Table 3.3 shows correlations between the responses to the reservation wage question and income components on the individual level. Apparently, at least for 2011, answers to the survey question on household income and the three income components computed in the fashion described above are highly correlated. For 2008, correlations and their significance are lower, particularly for wages. Still, for agricultural output, which, presumably, is the most important determinant of household income, the correlation is significant. Regarding the correlations between these income components on the individual level and answers to the reservation wage question, however, information from these separate elements of the survey questionnaire are much less consistent as can be seen from table 3.3; there is only some weak correlation between reservation wage and wages from primary occupation. That result has a number of possible explanations; probably the most relevant one is that output from household farms and non-agricultural enterprizes was attributed to individuals based on indications as regards which household members are considered responsible for a particular plot or enterprize. However, responsibility for a plot or enterprize does not necessarily imply that income generated from these sources truly pertains to the individual. It is much more likely that income generated by the whole farm and by all household enterprizes is shared among household members according to some unobserved scheme (Duflo and Udry (2003)). Accurately capturing the relevant channels of intra-household distribution is beyond the scope of this survey.

These results suggest that, at least on the household level, the information captured by the alternative measures of income sources are relatively consistent. The quality of the income data should be put into perspective by referring to the substantial difficulties other survey projects in developing countries have seen. For instance, as (McKay (2000)) documents, several LSMS surveys exhibit severe inconsistencies, such that, for example, income and consumption are only weakly correlated, and comparisons between average

[16]Note that in the questionnaire, in order to encourage respondents not to refuse answering, household income is recorded in intervals (where for 2008, the lower bound of the high income interval is lower than in 2011). For the calculations in table 3.2, each income category was assigned the average of its lower and upper bound; for the highest income interval, it was chosen to be equal to 400 000 for the highest income group in 2011 and equal to 350 000 for the highest income group in 2008.

Tab. 3.2: Correlation of gross household income with measures of income components for households

		2008	2011
Total salaries	Correlation	0.0676	0.2985
	P-value	0.1219	0.0000
Agricultural output	Correlation	0.1381	0.1876
	P-value	0.0015	0.0000
Revenue non-agric. enterprize	Correlation	0.1021	0.1937
	P-value	0.0193	0.0000

Sample: All households with non-missing income data (N=525 for 2008; N=582 for 2011). In the 2011, one outlier with respect to total salaries per household was removed from the sample.

Tab. 3.3: Correlation of reservation wage with measures of income components for individuals

		2008	2011
Salary, primary occupation	Correlation	0.0777	0.0610
	P-value	0.0226	0.0805
	N	861	823
Salary, secondary occupation	Correlation	0.0664	0.0767
	P-value	0.2124	0.1925
	N	354	290
Agricultural output	Correlation	0.0073	0.0246
	P-value	0.8239	0.4064
	N	925	1142
Revenue non-agric. enterprize	Correlation	0.0531	0.0396
	P-value	0.3682	0.4403
	N	289	381

Sample: All individuals with non-missing responses to the reservation wage question (N=925 for 2008; N=1142 for 2011). Additional observations are removed when the second variable used to compute the correlation coefficient has missing values. Resulting sample sizes are given in the table. In the 2011, one outlier with respect to salary for the primary occupation was removed from the sample.

consumption and income imply unrealistically high dissavings rates. McKay (2000) discusses the example of the Ghana Living Standards Measurement Survey analyzed in an earlier study. The quality with which income was measured varied between Ghana's regions. However, since the data from the Togolese survey project described here are obtained from one single community, the sampled population can be assumed to be much more homogenous with respect to how reliably income is measured. Accordingly, even if income is over- or under-estimated, the resulting measures should still provide a relatively accurate ranking of households that reflects their true income distribution. Finally, it has to be stressed that most surveys from developing countries focussing on issues in child development do not bother to collect information on income at all, given the difficulties associated with it (e.g. the Demographic and Health Surveys, DHS, and the data analyzed by Ghuman et al. (2005)[17]).

3.1.3 Cognitive skills

Since the central aim of the survey is to allow evaluating a preschool project as well as studying other issues related to education, measures of cognitive development of children must be an integral part of the survey. On the one hand, measuring ability before any kind of treatment takes places allows to take into account individual heterogeneity that is important the optimal decision regarding investment in human capital and to educational outcomes in the future. In addition, cognitive development may, in the long-run, also be an outcome of educational interventions. Furthermore, decisions regarding investments in human capital are often made by parents. These decisions may, in turn, be affected by the parent's cognitive abilities (e.g. if decisions depend on the ability to process complex information regarding the returns to investments in human capital, or on information regarding educational institutions, Ghuman et al. (2005)). Since, in addition, individual cognitive ability is usually correlated within families (Plug and Vijverberg (2003)), being able to measure cognitive ability of adults as well is beneficial

[17]These two surveys took another route to measuring living standards by assessing a household's physical assets in order to construct a measure of household wealth. Similarly, the Togolose survey also included some questions on characteristics of the home and on the ownership of various types of goods. Answers to these questions are, for example, used as explanatory variables for the analysis presented in chapter 4.

to the survey's value.[18] Measuring child development for the age group eligible for preschool is also relatively rare for surveys from developing countries, because they often focus on infants and children up to three years of age (see, for example, the DHS surveys and the data analyzed by Ghuman et al. (2005)).

Regarding the design of appropriate cognitive tests for the Togolese survey, two conflicting needs had to be weighed against each other. On the one hand, a major criterion for implementing meaningful tests of cognitive ability is whether a test is reliable, that is, whether it would yield approximately the same result if the same individual were to take it several times. On the other hand, the resulting survey module had to be lean in enough in order not to demand too much resources.[19]

All common psychological tests of cognitive development (e.g. the often used Wechsler Intelligence Scale for children) are very large and take one hour per subject and even longer. Typically, such tests are structured into various parts, and within each part, test items are iterated, slightly modifying them at each step. Such groups of many items similar can be seen as thorough attempts to measure one single construct, i.e. one "aspect of intelligence".[20] The motivation for repeating similar items many times is to increase the reliability of the test. In other words, each item within a group is considered to provide a valid measurement of the same aspect of intelligence, but each measurement may contain an error. By using information on a large number of measurements of the same aspect of intelligence, the error is supposed to be reduced. The number of items typically used is also inflated by the fact that psychological tests of cognitive development are usually intended to allow some kind of diagnosis regarding the development of one particular child. Accordingly, cognitive development is usually

[18] The remainder of this section discusses the details of the design of the cognitive tests for children. The tests administered to adolescents and adults constituted a modified version of the test for 11- to 14-year-old children described below. Males between 15 and 25 years and females between 15 and 30 years were eligible for this section of the questionnaire. The age limits were chosen in order to avoid refusal, given that, during the preparatory period, interviewers argued that older adults would mostly feel humiliated by being asked to perform such a test.

[19] In particular, the duration of a feasible test was constrained by the fact that in some households, up to 10 children between 3 and 14 years were to be tested; the average number of children in that age group per household was 2.4 (as of 2011).

[20] For example, in their evaluation of an East African preschool project, Mwaura et al. (2008) implement a total of seven item groups which are adapted from the British Ability Scales II and the African Child Intelligence Test. The item groups are called block building, verbal comprehension, early number concept, picture similarity, verbal meaning, exclusion and closure.

broken down into several dimensions[21] , and since each dimension is supposed to be reliably measured, the number of items necessary increases.

Since, in the context of this project, the principal aim of testing children and adults is not to break down test scores into several dimensions of cognitive ability but rather to compute a single composite score per individual, the number of items can be reduced. In fact, the number of test items could be chosen arbitrarily small, but any reduction would come at the cost reduced reliability of the resulting composite measure.

Based on this reasoning, the approach taken for the Togolese survey was to implement a number of test items which could be interpreted as a subset of items from a comprehensive psychological test. This was implemented by designing items inspired by parts of the German version of the Kaufman assessment battery for children (K-ABC, Melchers and Preuß (2005)) as well as LSMS (see section 3.1).[22] Regarding the choice of types of items, the following criteria had to be met: the overall test length was chosen to be less than 10 to 15 minutes per subject; test items would have to require only limited additional testing material, and they had to be straightforward enough to be handled by interviewers who are not trained psychologists.

Table 3.4 provides an overview of which types of items were implemented as cognitive tests for different age groups. The following paragraphs give a quick explanation of the item types listed in the table.

In order to assess visual processing capabilities, children were shown only partially completed drawings of objects. Children hat to mentally fill in gaps in these drawings such that they were able to indicate the correct name or description of an object ("Gestalt Closure").

Short term memory was a prominent component of the employed test, and it was implemented using several types of items. In three of the exercises, children had to repeat sequences of the interviewer's actions or indications. These actions could include hand movements[23], reading out a list of names of objects which the child had to indicate

[21] To illustrate, results obtained from implementing the seven item groups in Mwaura et al. (2008) as mention in footnote 20 were used to calculate seven corresponding "subscales".

[22] Another virtue of common psychological tests is that they are standardized, meaning that the distribution of test scores for a reference population is known. Since the only objective in the context of the Togolese survey was to provide a ranking of individuals according to their cognitive abilities that is meaningful *within* the surveyed population, it is not necessary to use a standardized test.

[23] Interviewers were given instructions to demonstrate sequences using the gesture elements of making a fist, placing the flat hand on the table/thigh, and placing the edge of the hand on the table/thigh.

on pictures in the correct order[24], and reading out a sequence of numbers which the child had to repeat in the correct order. Furthermore, in two exercises, children also had to repeat what the interviewer read to them, but this time without being obligated to stick to a specific ordering of elements. In one case, these were just names of objects, but in between the interviewer reading out the objects' names and asking the child to repeat them, the child was being asked other, unrelated questions. In the other case, a very short story was read to the child, and the task was to repeat as many core elements of the story as possible.

In order to measure general reasoning, the well known Raven's progressive matrices were used (see Moore et al. (2008) for another example of applying a matrix reasoning test in the context of evaluating a preschool program). The items used consisted of matrices with either four or six graphic elements, where one element was left blank. Respondents had to recognize the pattern or deduce the logic underlying the matrix elements in order to choose the correct element to fill in out of several proposed solutions.

In each age group, questions were asked to test knowledge in several fields. Simple questions that required a verbal response were asked either in the form of riddles[25] or as questions regarding general knowledge[26]. Another type of items consisted of showing the child pictures of objects (for example, a bucket), and the child had to give a correct name for the object. A further set of questions asked children to perform simple mental arithmetic.

Finally, for the oldest group of children, a very simple (French) literacy test was applied. They were shown a sheet with a written instruction (E.g. "Raise your right hand!"), and they had to perform the respective task without further instructions[27].

As discussed above, the number of repetitions of the same type of item was kept relatively low in order to make the test logistically feasible. Accordingly, none of the scores resulting from a group of exercises of the same item type are supposed to yield

I.e. an instruction to the interviewer would look like "fist/flat hand/fist".

[24]That is, the child was shown a sheet with several images. Interviewers read a sequence of names of objects that appeared on the sheet. The child had to point to the respective images in the correct order.

[25]Interviewers would indicate characteristics of an object, and the child had to name the object. Example: "What has feathers and lays eggs?"

[26]E.g. "What is the name of the President?"

[27]Admittedly, other characteristics of the child such as his physical development may affect results of this test. However, literacy is likely to be the most important determinant of responses to this type of exercise.

Tab. 3.4: Types of items used for assessing cognitive development of children

		Age group								
		3-5 years			6-10 years			10-14 years		
	Varname	Item	Position '08	Position '11	Item	Position '08	Position '11	Item	Position '08	Position '11
Visual processing	recognize	Gestalt Closure		170-172	Gestalt Closure		170-172			
Short Term Memory	repeat-manual	Hand Move-ments		173-178 178	Hand Move-ments		173-176 176			
	repeatoral	Word Order		179-183						
	repeatoral				Number Recall		177-179	Number Recall		170-174
	story	Story Repe-tition	121	191	Story Repe-tition	121	190	Story Repe-tition	121	189
	recall				Object Repe-tition	120	163	Object Repe-tition	120	163
Reasoning	matrix				Raven's Pro-gressive Matrices		180-183 183	Raven's Pro-gressive Matrices		175-180 180
Knowledge	questions	Questions	113-114	184-190	Questions	113-114	184-189	Questions	113-114	181-188
	objects	Naming Objects	115	192-194	Naming Objects		191-194	Naming Objects		190-192
	math				Mental Arith-metic	117-119 119	160-162 162	Mental Arith-metic	117-119 119	160-162 162
Literacy	read							Written Instruc-tions		193-194 194

Note: Columns entitled 'Position' indicate where interviewers recorded the corresponding responses in the two survey questionnaires. Note that most questions and tasks of this section do not appear in the questionnaires due to lack of space. Interviewers were given notebooks containing the material necessary for performing the tests described in the text. Column 'Varname' serves as a guide to finding the corresponding variable in the dataset . The naming pattern is as follows: cog_VARNAME_NUM, where VARNAME should be replaced by the strings indicated in the 'Varname'-column, and NUM stands for the NUM'th repetition of the respective type of item in an age group. Within each age group and item group, counting starts at one and the highest number depends on the number of times a particular type of item is repeated in an age group. For instance, the "Hand movements" item is repeated four times for six to ten-year-old children, resulting in values saved in variables cog_repeatmanual_1 through cog_repeatmanual_4. Variables cog_repeatmanual_5 and cog_repeatmanual_6, however, will have missing values for that age group.

a reliable measure of the respective aspect of knowledge or intelligence. For example, the intention of asking three questions of the ”gestalt closure” item type is not expected to result in a scale which reliably reflects heterogeneity in visual processing.[28] Still, as I consistency check, I computed correlations between subscores corresponding with item types and a measure of schooling (school grade completed minus age plus six years). Results for the two tested age groups who are eligible for primary or secondary school are given in table 3.5. For some of the item types, they show consistently strong and statistically significant correlations with education, particularly for the questions of general knowledge and math (except for 10- to 14-year-old children in 2011), as well as the short literacy test for 10- to 14-year-old children in 2011. This finding seems reasonable, since these items arguably measure aspects of knowledge that are, more than the other items, both enhanced in school and resulting in better performance in school. It is not possible to distinguish whether the scores reflect ability that lead to better performance in school, or whether high scores in the respective items are an outcome of school education. Still, the pattern shown in table 3.5 at least suggest that most elements in the questionnaire module do succeed in grouping children according to their cognitive abilities.

The main goal, however, is rather to combine information from the complete questionnaire module in order to compute one single composite score per child that, hopefully, constitutes a good general measure of knowledge and intelligence. Accordingly, the reliability of the measure should be evaluated taking into account all item measurements contributing to the composite score. For 2011 (2008), the respective Cronbach's alpha[29] is equal to 0.77 (0.47) for 3- to 5-year-old children, 0.75 (0.81) for 6- to 10-year-old children, and 0.66 (0.72) for 11- to 14-year-old children. I conclude that, while results for 3- to 5-year-old children in 2008 (which are based on only three questions) and those for 11- to 14-year-old children in 2011 need to be interpreted carefully, overall the questionnaire module resulted in reasonably reliable measure of cognitive ability and

[28]Accordingly, Cronbach's alpha, the commonly used measure for reliability (Schnell et al. (2008)), is relatively low for some item groups. The measure relies on correlations between responses to different items in order to asses inter-item consistency. It is usually stated that 0.7 is a minimum value for alpha that is desirable. A few of the item groups listed in table 3.4 actually meet this requirement: Literacy for 10- to 14-year-old children in 2011 (0.79), mental arithmetic for all groups except for 10- to 14-year-old children (0.72 to 0.81), as well as questions, word order and hand movement for 3- to 5-year-old children in 2011 (0.70, 0.78, and 0.70, respectively).

[29]See explanation in footnote 28.

Tab. 3.5: Correlation between composite scores from cognitive test item groups and grade for age

		Age group			
		6-10 years		10-14 years	
Varname		2008	2011	2008	2011
recognize	Correlation		0.1055		
	P-value		0.0420		
	N		372		
repeat-manual	Correlation		0.0801		
	P-value		0.1254		
	N		367		
repeatoral	Correlation		0.1041		0.1684
	P-value		0.0507		0.0213
	N		353		187
story	Correlation	0.2095	0.0338	-0.0225	-0.0107
	P-value	0.0000	0.5180	0.7338	0.8827
	N	456	368	231	192
recall	Correlation	0.1935	0.0799	0.1334	0.0700
	P-value	0.0000	0.1209	0.0411	0.3371
	N	459	378	235	190
matrix	Correlation		0.0469		0.2506
	P-value		0.3713		0.0005
	N		365		190
questions	Correlation	0.3448	0.1656	0.3596	0.0883
	P-value	0.0000	0.0025	0.0000	0.2400
	N	467	330	234	179
objects	Correlation		0.1170		0.1973
	P-value		0.0262		0.0068
	N		361		187
math	Correlation	0.4229	0.3116	0.2792	0.1482
	P-value	0.0000	0.0000	0.0000	0.0424
	N	459	375	231	188
read	Correlation				0.3410
	P-value				0.0000
	N				153

Note: Composite scores of item subgroups are defined as deviation of the individuals number of correct responses per item type from the age specific mean of correct responses for an item type.

knowledge. Figure 3.1.3 shows histograms for the composite test scores for all children in 2008 and in 2011.

3.2 Choice and characterization of the studied community

The choice of the community under study was essentially driven by the rare opportunity to evaluate a preschool project, including the ability to cooperate with the NGOs responsible for the project long before the opening of the preschool. In addition, though, the community proved to provide an interesting research environment more generally. Given its economic activities, infrastructure, and its ethnic and religious composition, it can be viewed as somewhat representative for small towns in rural areas of southern West Africa. In fact, the larger geographical area has been a field of study for several other economists before.[30]

The community is situated in the Badou-region of southern Togo, a rural area close to the Ghanaian border. It is the main town of a small geographic area (a so called ”canton”), and its market and secondary schools are of local importance. It receives many secondary school students from all over the ”canton” who either commute, are fostered-in [31] (a phenomenon quite common in West Africa, see Serra (2009), Glewwe and Jacoby (1994)) or rent rooms in the community. 53.4 percent of the households farm, comprising 64.4 percent of the population. Many do so on a subsistence level, some produce cocoa or coffee for export [32]. While the climate is very humid, the mountainous landscape as well as the soil type do not permit the cultivation of large plantations. Other economic activities found are services and a few crafts, industry does not exist. The infrastructure is poor (no lights, no running water, and only main roads are paved). The Badou-region lies in the sphere of the Ewe, a people scattered over southern Togo

[30]For instance, Besley (1995) and Goldstein and Udry (2008) study land usage rights and their impact on agricultural production in two nearby Ghanaian regions.

[31]18.5 percent of all children in the dataset under 15 with at least one living parent report not to be living in the same household with either parent in 2011

[32]29.0 percent of all households had cultivated any cocoa during the last 12 months in 2011, 4.9 had cultivated any coffee.

and south-eastern Ghana. The most important ethnicity in the community is Akposso (39.3 percent of the interviewed population in 2011), who share a cultural similarity with the Ewe. Furthermore, the community has experienced considerable immigration from other parts of Togo and neighboring countries, leading to a mix of ethnicity[33] as well as religion, with Christian churches dominating [34].

It is difficult to find a valid source of information which could help to crosscheck these figures, because, to the best of my knowledge, no comparable data have been collected for a population that comprises the studied community. As an alternative, I compare descriptive statistics from my household survey data with equivalent statistics from a comparable survey dataset collected in neighboring Ghana, the Ghana Living Standards Survey from 1988/1989. For an exemplary comparison, I choose means of completed years of school, grouped by age, sex, and participation in off-farm work. The comparable statistics computed based on GLSS data are available from table 2 in Jolliffe (2004). Table 3.6 compares the respective means from both data sources. Accordingly, the pattern of how education varies among the groups displayed is similar in both datasets: The number of years of completed schooling tends to decrease with age, and it is lower for women; individuals who are participating in off-farm work are better educated. However, the overall level of education is higher in the Togolese data than in the Ghanaian data from the late 1980s. This is consistent with the fact the Ghanaian data were sampled from the whole country, including remote areas, whereas the population in the Togolese community lives in walking range to both primary and secondary schools. Furthermore, the decrease in education with age is less pronounced in the Togolese data. This could be explained by the fact that the Ghanaian data will also include data from cities like Accra which have a University, whereas young individuals from the Togolese community who want to study at a University will have left the community and cannot be part of the sampled population. Overall, even though the choice of characteristics used for the comparison is very limited, these results suggest that the Togolese data capture socio- economic patterns that can be found throughout a larger geographic region, including neighboring countries.

[33]Other mentionable ethnic groups in the community are Ewe (20.7 percent), Kabye (11.0), and Kotokoli (9.3). The latter two ethnic groups originally settled in northern Togo.

[34]According to the 2011 data, 43.4 percent of the population are catholic, 32.5 percent belong to protestant, pentecostal and other Christian churches, 19.5 percent are muslim.

Tab. 3.6: Comparison with GLSS data analyzed in Jolliffe (2004): Years of completed schooling by sex, age, and off-farm participation

Age	Full sample		Fam work only				Off-am work			
			Male		Female		Male		Female	
	Togo	GLSS	Togo	GLSS	Togo	GLSS	Togo	GLSS	Togo	GLSS
All adults (20+)	6.02	4.23	7.10	4.0	4.52	1.5	7.70	6.3	4.89	3.3
20-24	7.33	6.13	8.98	6.3	6.60	3.1	7.50	7.9	5.38	5.2
25-29	6.36	5.89	8.46	5.8	5.52	3.0	6.60	8.0	5.60	4.7
30-34	6.54	5.93	7.03	6.6	4.80	3.1	8.76	7.9	5.59	5.0
35-39	6.02	5.16	6.87	4.6	5.13	2.3	7.42	7.5	4.25	3.9
40-44	6.05	4.48	6.74	4.1	4.00	1.0	8.47	7.3	4.25	2.9
45-49	6.87	3.24	7.12	3.9	3.86	0.6	8.37	5.5	6.35	1.8
50 and older	4.54	1.19	6.01	1.5	2.42	0.2	6.42	2.5	3.07	0.5

Consistent with table 2 in Jolliffe (2004), the sample includes member of all farming households who are 20 years of age and older; 'Off-farm work' includes all individuals who work off the farm, regardless of whether they also work on the farm. N=834.

3.3 Survey implementation: identification of households and realization of interviews

As outlined in the introduction, the main motivation for conducting the household surveys was to allow evaluating a preschool program. Since the number of potential participants in the project was rather small, it was necessary to survey the whole population of households potentially participating in the project in order to insure that the resulting dataset would be large enough to allow statistical analysis. This reasoning suggested to include the community's whole population in the survey. Accordingly, no further decisions regarding a sampling scheme had to be made. A much grater challenge was to ensure that the data would allow matching individual observations from both waves.[35]

Ensuring that the data produced by the survey would constitute a two-wave panel required that individual observations as well as households from both waves be uniquely identifiable. In Togo, as in most African countries, streets are not officially given names, and houses are not numbered. The first measure taken to meet this requirement was to record the respondents' full names in addition to demographic characteristics[36]. Sec-

[35] As the list of datasets in Foster and Rosenzweig (2008) suggests, the number of available panel datasets from developing countries is very limited. Furthermore, the authors point out that these data usually suffer from strong panel attrition. An important reason for both the limited availability and the high attrition might in fact be that linking individuals in developing countries over time requires overcoming severe technical and organizational difficulties.

[36] Linking households and individuals between waves is the only purpose the names were used for.

ondly, before effecting the first survey in 2008, the interviewer team cartographized the community. That is, free-hand maps were drawn that indicated the geographic location of all dwellings. Each dwelling was assigned a unique number which was marked on the maps as well as on the dwellings wall. With the resulting maps at hand, interviewers were able to trace any dwelling when given the respective number. After the completion of the first survey, the free hand maps were digitalized and assembled to a full community map. A copy of that map, fit to two DIN A4 pages is given in appendix 6.3. During the second survey in 2011, interviewers worked with delimited portions of that map, an example of which is given in appendix 6.4.

Based on the cartography of the communities, it was possible to link any household questionnaire to the dwelling a household lives in, i.e. the dwelling numbers were recorded on the questionnaire. Thus, if a household had not moved between 2008 and 2011, the two respective questionnaires from the two surveys could be linked. Moreover, during the second survey, the interviewers took with them household rosters from 2008 for all households that had been living in the dwellings that they were revisiting for second wave interview. Thus, for immobile households, they were able to record the individual 2008 household member identification number for those present in 2011 (question 7 in the 2011 questionnaire). This measure allowed to link individual observations between waves during the preparation of the data for statistical analysis.

For individuals (questions 9 through 12 in the 2011 questionnaire) and households (front page of the 2011 questionnaire) who had moved between 2008 and 2011, it was recorded whether that move had taken place within the community. Since in 2011, all households were interviewed, regardless of whether they had been living in the same dwelling in 2008 or not, movers must be observed in the 2011 data as long as they have not left the community. Out of the pool of households and individuals that had been indicated to have moved within the community, pairs of matching observations were searched after the completion of both surveys making use of the full names and demographic characteristics. For this task, an algorithm was programmed in Stata, consisting of two steps. In a first step, the closest matches between pairs of names found in both waves. This is achieved based on calculating Levenshtein distances between all

Naturally, they will not be included in any published data.

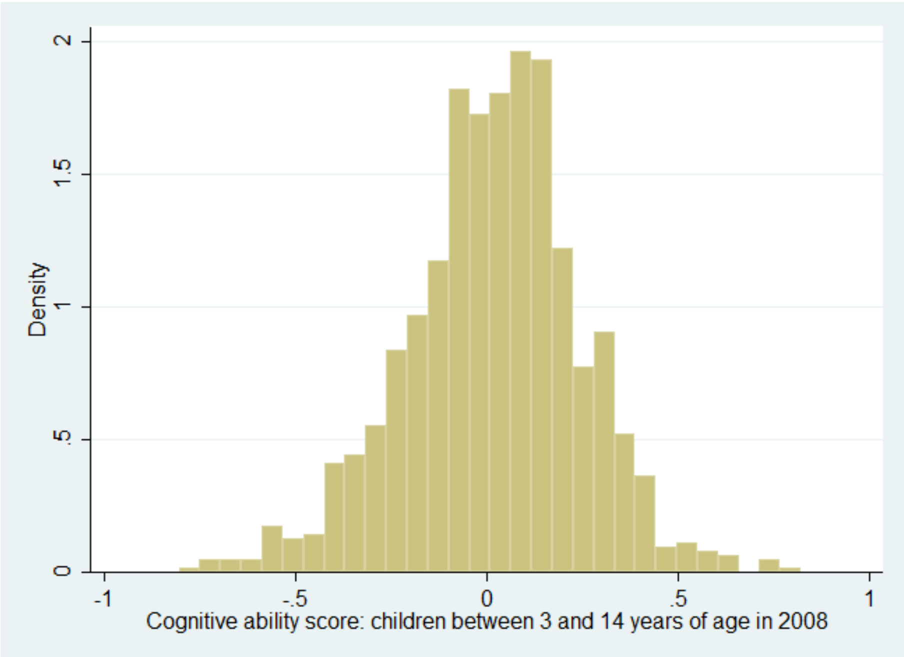
possible pairs of names as a measure of similarity.[37] This information was combined with similarities with respect to demographic characteristics in order to both find sets of potential matches and, in case an observation from one wave has been linked to several concurrent potential matches in the other wave, to decide which pair of observation within the set of potential pairs constitutes the closest match. In the end, out of the 3615 individuals who were interviewed in 2008, 2363 could be recovered in 2011

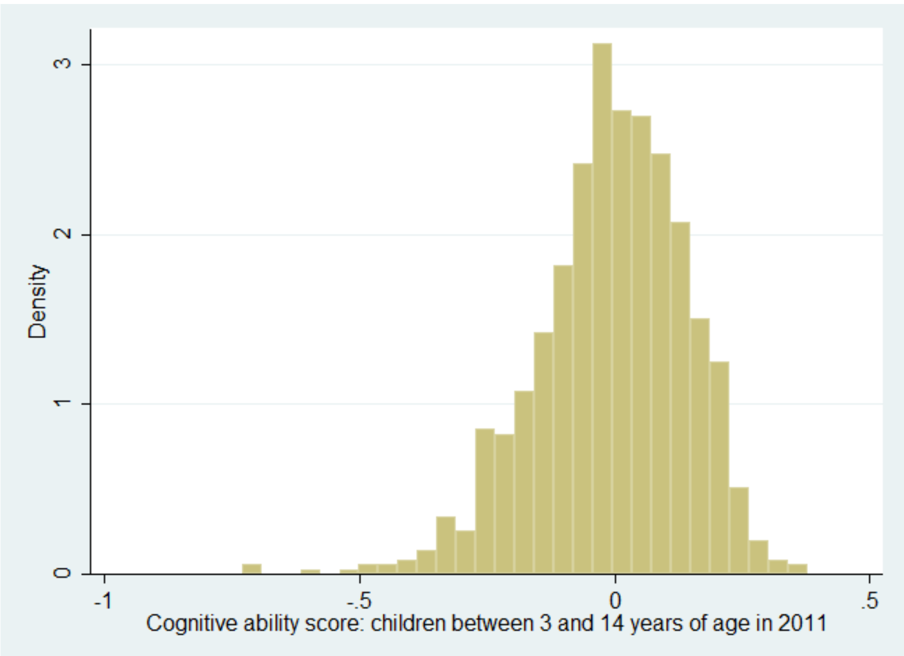
The logistics of realizing the interviews relied on a large staff. I was able to obtain access to Togolese graduate students, university graduates and academics during the preparation period for the 2008 survey. These prospective collaborators, in turn, advertised my search for interviewers and supporting staff on Lomé university campus. At the beginning of the 2008 field trip, I had received enough high quality applications to choose 30 members for the project team. In 2010, a core group of that team became responsible for recruiting additional staff for the second survey in 2011. Out of the 2008 team, 14 members were interested in (and available for) participating in the second survey as well. For the last week of each field trip, the interviewers with the best computer skills were hired for data entry. CSPro software was used to implement a graphical interface for data entry based on the questionnaire structure.[38]. As recommended by Grosh and Glewwe (1998), the risk of errors was reduced by mirroring the skip patterns from the questionnaire and letting the software perform simple plausibility checks during data entry (e.g. alerting data entry operators when out-of-range values are entered). The resulting data base can be stored as a tab separated file and thus easily be imported into statistical software packages like Stata.

[37]For calculating the Levenshtein distances, the name data are first transformed into sequences of numbers. Then, similarity measures are calculated using the sqom command from the Stata module for sequence analysis (Kohler et al. (2006)).

[38]CSPro stands for Census and Survey Processing System, and it is provided by the U.S. Census Bureau (see http://www.census.gov/population/international/software/cspro/).

Fig. 3.4: Composite score of cognitive ability





Note: Composite scores are calculated by first transforming individual results for each item-subgroup (see table 3.4) into deviations from the age-specific mean for the respective item-subgroup. The mean of the resulting deviations is then interpreted as the composite cognitive test score. N=1164 (921) for 2008 (2011).

Chapter 4

Child care and time use of young mothers in developing countries - Experimental evidence from Togo[1]

4.1 Introduction

This paper studies the impact of preschool enrollment on time use of young mothers. While this research question has been studied extensively by economists using data from industrial countries, there exists almost no evidence from the developing world. The issue is of high relevance for policy makers, though, since both the public provision of child care in developing countries currently is at a low level and creating new opportunities for women in these countries to participate in economic activities may be a particularly effective means to reduce poverty in developing countries (Lokshin et al., 2000).

The empirical strategy of this paper is to evaluate the impact of a preschool program in a Togolese community. Before the start of the preschool project in October 2010, young children[2] in the studied community had very limited access to institutionalized care. Using household data collected before and after the introduction of the preschool program, I exploit the variation in enrollment induced by the randomization of access to preschool. Since compliance with the randomization is imperfect, the main strategy is to use the result of the admission experiment to construct an instrumental variable for

[1]Part of this research was supported by the German Research Foundation (DFG) within the project 'Ein Paneldatensatz zur Analyse von Humankapitalinvestitionen im frühen Alter in ländlichen Regionen von Entwicklungsländern', and by the Leibniz Universität Hannover ("Forschungsfond").

[2]Throughout this chapter, the term young children will refer to children younger than six years. Children who are between two and five years old will be called preschool age children.

enrollment. In addition, it is argued that the likelihood of responding to the introduction of the preschool program depended on the age of a mother's youngest child in fall 2010. Consequently, a difference-in-differences approach is applied which estimates the difference of the impact of being admitted to preschool between groups of mothers whose youngest children are from different cohorts.

In order to understand whose time use may potentially be affected the most by any variations in child care necessities, it is important to know which household members spend the most time caring for young children. As documented in Lokshin et al. (2000) for the case of Kenya (also see LeVine et al. (1994)), in African households child care may often be delegated to other household members than the mother.[3] However, as table 4.1 shows, even though grand mothers and aunts of young children are also important providers of child care in the studied community, the children's mothers are by far the most important providers. Other groups of female cohabitants and male cohabitants are negligible regarding their small contribution to child care efforts or due to their group size.[4] Thus, this study will focus on the impact of child care arrangements on the time use of mothers of young children. Given their high level of hours spent caring for children in combination with their relatively high working capacity (compared to grand mothers of young children, for instance), they are plausibly the group most likely to be constrained in their labor supply by child care responsibilities.

Descriptive results in section 4.2 show that for mothers of young children the most important enrollment measure associated with their time use is an indicator for "full enrollment", that is, whether or not all of their young children are enrolled in either preschool or primary school. In case of full enrollment, mothers spend much fewer hours caring for young children, and the time during which child care and other non-work activities overlap is significantly shorter. However, the association with hours of work is weak.

The enrollment of preschool age children is likely to be an endogenous variable in

[3] Stansbury et al. (2000) documents such flexible child care arrangements for rural Ecuador as well.

[4] Table 4.10 in the appendix in the appendix repeats the analysis in a more detailed fashion by using less aggregated categories of time use, and by distinguishing between the first and the second half of the day. The pattern described here is not associated with the timing of activities, i.e. it is equally observed during the morning/noon and the afternoon/evening. An additional result shown in these tables, which is consistent with other empirical studies in developing countries (Malathy (1994)), is that women spend much more time working at home than men do.

models of mothers' time use, because unobserved characteristics of mothers and children can be expected to simultaneously determine both the decision to work and the decision to enroll children. Furthermore, studies using cross sectional data representing a population from a large region or a country will typically be confronted with the complication that the placement of public programs to promote early childhood development is often driven by political and antipoverty considerations, resulting in a correlation between program placement and unobserved community characteristics (Ghuman et al. (2005)).

In order to investigate the relationship between full enrollment and time use in a framework that allows to cope with issues of endogeneity, a randomized experiment was carried out, which was designed to evaluate the preschool project introduced to the studied community. Half a year before the newly constructed preschool started operating in October 2010, access to the first two grades of preschool was determined by separate lotteries for each grade among all children who were signed up for the admission procedure. Unfortunately, compliance with the randomization was poor. However, I argue that the specific circumstances of the admission procedure resulted in the lottery affecting both the admitted children's likelihood of enrollment in preschool and their likelihood of enrollment in *primary* school - taking into account that, in the studied community, primary schools constitute alternative child care institutions. As it turns out, enrollment in *either* preschool *or* in primary school is significantly increased for the children accepted for the first grade of preschool[5] This variation in the likelihood of enrollment of individual children translates into variation in the likelihood of full enrollment (i.e. of all of a mother's young children being enrolled) from the point of view of mothers.

Exploiting this variation, I estimate instrumental variables models of time use of mothers participating in the randomized admission procedure, using an indicator for whether the mother has a child who was accepted for preschool as an instrument for full enrollment.

Furthermore, I identify subpopulations of women who are more likely to respond to the introduction of the preschool than others. More specifically, I argue that the likelihood of full enrollment depends to a large extent on the enrollment status of a

[5]The terms being "accepted for" and "admitted to", and "having access to" preschool are used interchangeably throughout this chapter. They are distinct from actual enrollment in preschool.

mother's youngest child. By the start of the preschool project, a mother's youngest child may have been too young to be eligible for preschool. As a result, their mother's probability of having all her young children enrolled is much less likely to increase in response to the preschool project. Accordingly, mothers whose youngest child is two or three years old are more likely to respond to treatment than mothers whose youngest child is younger. I exploit this variation in the likelihood of responding to the introduction of the preschool by comparing the impact of admission to preschool between mothers whose youngest child is at least two years old, and mothers whose youngest child is younger than two years in a difference-in-differences framework.

Proximity to the preschool construction site is discussed as an alternative source of variation. Since for very young children the distance to school may constitute a constraint to enrollment, mothers living close to the preschool are more likely to be affected by it in the sense that it reduces the distance to the closest institution available for their young children - again, accounting for the fact that primary schools also accept children of preschool age. In fact, variation in the indicator of whether the preschool constitutes the closest institution translates into variation in the likelihood of full enrollment, but this relationship is not estimated to be statistically significant.

For developing countries, the relationship between child care and female labor supply has not been investigated empirically so far (Lokshin et al. (2000), which shows a positive relation between early childhood development programs and female employment in Kenya, is, to the best of my knowledge, the only exception). A few studies investigate the relationship for emerging economies (see Connelly et al. (1996) for the case of Brazil, Berlinski and Galiani (2007) for the case of Argentina, and Wong and Levine (1992) for a study using data from Mexico). A few studies have focused on how institutional child care in developing countries affects the cognitive development of the children (Mwaura et al. (2008)).

A major limitation to investigating the issue has been the still very low investments into institutionalized child care in developing countries (UNESCO (2007); until only a few years ago, many African countries did not have any public preschools, as documented for Botswana in Taiwo and Tyolo (2002)), which imply that opportunities to conduct field experiments are rare. Furthermore, it is difficult to conduct observational studies on the issue using existing survey data from developing countries (e.g. Demographic and Health Surveys), because these data sets usually do not include information on enrolment status of children younger then primary school age.

In industrial countries, on the other hand, a strong link between child care and mothers' labor supply is generally accepted and has manifested itself in numerous research papers. Unfortunately, among them, there are only very few experimental or quasi-experimental studies.[6] Instead of relying on variation in observed household expenditures or area-level averages of prices or expenditures for child care to identify the response in female labor supply, a few studies have tried to exploit natural experiments. An example of a study that uses a natural experiment is Berger and Black (1992) who use women on the waiting list as a comparison group for recipients of child care subsidies and find positive employment effects. Several studies take advantage of expansions of preschool provision or child care subsidy programs. Baker et al. (2008) exploit the expansion of subsidized provision of child care in a Canadian province, and they find a positive effect of child care use on maternal labor supply for married mothers. Cascio (2009) studies the impact of the introduction of preschool subsidies in the U.S. during the 1960s and 1970s. She finds that kindergarten attendance has an effect only on single mothers whose youngest child is five years old. Schlosser (2005) studies the impact on labor supply of the gradual implementation of compulsory preschool laws in Arab towns in Israel. She finds that preschool provision increases maternal labor supply. Finally, Berlinski and Galiani (2007) find a positive impact of a large-scale construction program of preschools in Argentina.

I find that, even though estimation results consistently show that full enrollment reduces the amount of time mothers spend caring for children, mothers whose children of preschool age children are all enrolled do not work significantly more hours.

The remainder of this chapter is structured as follows: Section 4.2 provides a descriptive analysis of the Togolese data, looking at associations between child care arrangements and enrollment of children of preschool age. Motivated by the concerns regarding problems of statistical endogeneity which may be present in such a descrip-

[6] Numerous studies use US- or Canadian data to simulate child care price elasticities of female labor supply on the basis of parameter estimates of structural models (see Michalopoulos et al. (1992), for instance), and they typically find a negative elasticity although there exists a large range of estimates, and the results are ambiguous for single mothers. Ribar (1992) and Anderson and Levine (1999) include surveys of that literature and related studies from the US; see Michalopoulos and Robins (2002) as an example for a study using Canadian data

Tab. 4.1: Average time use of individuals cohabiting with young children

	Mothers of young children	Grand mothers, aunts	Sisters older than 5	Other female relatives	Non-related female	M. child older than	Cohab. male adult
Hours of care	10.02	5.68	0.97	1.78	7.18	50.26	0.53
	(5.27)	*(6.08)*	*(2.90)*	*(4.28)*	*(6.56)*	*(1.24)*	*(2.01)*
Hours of work	4.94	4.63	1.05	4.83	6.50	0.32	5.34
	(4.03)	*(4.08)*	*(2.6)*	*(4.67)*	*(4.27)*	*(0.95)*	*(4.45)*
Care/work overlapping	3.09	1.70	0.19	0.55	3.73	0.04	0.15
	(3.64)	*(3.05)*	*(1.09)*	*(1.54)*	*(4.58)*	*(0.50)*	*(0.92)*
Observations	264	116	189	20	20	196	349

Sample: Calculations are based on Sample B (see table 4.9 in the appendix for details), and each column of the table refers to a subsample of Sample B. Note that some of these categories are not mutually exclusive. For instance, a women may be both the mother of one cohabiting child and the aunt of another. In addition, in few cases, the relationship between household members is not deducible due to missing data, which is why the sum of observations in this table differs slightly from the number of observations in Sample B (standard deviations in parentheses).

tive analysis, section 4.3 describes the randomized preschool program introduced in the studied community and how that project induced variation in enrollment of young children. The empirical strategy exploiting that variation in enrollment is explained in section 4.4 which also describes the sample used for the analysis. Section 5.4 discusses the empirical results for the impact of full enrollment on time use of mothers, and section 4.6 concludes.

4.2 Descriptive analysis: associations between child care arrangements and enrollment of preschool age children

The objective of this study is to investigate the causal effect of increased enrollment of young children on their mothers' labor supply, i.e. whether increased enrollment both reduces the number of hours mothers spend caring for children and increases their hours of work. After a short discussion of patterns in time use among women of preschool age children, I first associations between young children's enrollment and the different uses of time of their mothers, for now ignoring concerns regarding causality.

The data used for both the descriptive analysis discussed in this section as well as for the evaluation of the preschool project (sections 4.3 through 5.4) are from a household survey conducted in the studied community in 2008 and in 2011, which has been described in detail in chapter 3. Since the objective is to investigate the impact of enrollment of preschool age children on mothers' time use, the population of interest throughout the remainder of this chapter consists of mothers of children in the studied community who are eligible for preschool, that is, children who are between two and five years old. Table 5.6 in the appendix shows descriptive statistics for all time use variables as well as all explanatory variables used for the analysis. The columns of the table correspond with different samples, including the full sample of mothers of preschool age children as well as various subsamples which are defined in table 4.9 in the appendix.

To lay ground for a descriptive analysis of time use it is useful to consider the occupational structure of the population of interest.[7] Table 4.12 in the appendix shows average time use separately for the most important occupations of women with young children in the studied community. Taken together, these five categories comprise 89 percent of these women. The number of women in each category is indicated at the bottom of the table. Agriculture and small commerce constitute the most important occupations for young mothers. In addition, a significant number of women is working as sewers, and 18 percent of the women (36 women) indicate not to have a proper occupation. Regarding average time use, some patterns in the data suggest consistency between indicated occupations and responses to time use questions. For instance, women in agriculture work by far the most time on a field compared to women in other categories. In turn, they are less likely to spend time working in a work shop or at home than women in commerce or sewers. Furthermore, time uses associated with education or apprenticeship are only relevant to women without an occupation.

Regarding the average time spent caring for young children as a function of occupation, there does not appear to be a very clear pattern. However, women in commerce do spend somewhat more time caring for young children, which appears to be driven by more hours of overlap between child care and work. It seems reasonable that women working in a shop or a sales stall can more easily supervise children than women leaving the village to go work on a field.

Another characteristic that is likely to affect time use of young mothers is the num-

[7]

ber of young children that they have. For instance, looking out for children while simultaneously working may be much more feasible when there is only a single young child rather than two or more. Since, in addition, fertility and enrollment decisions are usually assumed to be affected by common unobserved characteristics, taking into account fertility outcomes when investigating the impact of child care arrangements is important. However, as the 9th and 10th column in table 4.11 in the appendix show, mothers with more than one young child do not differ from mothers with just one young child as clearly as might have been expected. In particular, they do not work systematically fewer hours, although they spent somewhat less time working in work shops. Furthermore, they spend only about two hours per day more caring for children than mothers with only one child. Interestingly, the average overlap between child care and work is almost identical for the two groups of women.

Turning now to the association between time use and enrollment of young children, table 4.11 in the appendix documents that an indicator for whether or not all of a woman's young children are enrolled (either in primary school or in preschool) helps dividing mothers into two groups that differ substantially with respect to the amount of time they spend caring for children (7th and 8th column). The difference in average time spent caring for children is equal to about more than 3.5 hours during the first half of the day and more than 2.5 hours during the second half of the day. The fact that this difference is more pronounced during the first half of the day is consistent with the fact that the larger share of class time in schools takes place before noon. Strikingly, however, the sharp difference regarding child care between these two groups of women does by no means correspond with any equally pronounced differences in the time spent working.

Summing up the results for the bivariate analysis of time use survey data discussed so far, while hours of care are strongly associated with whether a mother's young children are all enrolled in school, her labor supply is not associated with that enrollment measure. Furthermore, variables that would usually be expected to affect time use, such as occupation and the number of children, are only weakly correlated with hours of child care. Turning now to a multivariate analysis that takes into account several of the variables discussed so far at once, the upper panel of table 4.2 presents results from regressing different categories of time use on various variables capturing the number of children in different age groups a mother has as well as how many young children are enrolled in school.

The first column shows that higher enrollment of young children is associated with cohabiting mothers spending fewer hours per weekday caring for young children. Controlling for the total number of children in the age groups of 0 years, 1 year and 2 to 5 years, they spend, on average, 2.8 fewer hours taking care of a child younger than six.

However, as the second column indicates, the fewer hours spent caring for young children are not mirrored by longer working hours of these mothers. An explanation for this observation could be that higher enrollment rather reduces the number of hours that mothers spend working while looking after children at the same time. In fact, as the third column of table 4.2 shows, the overlap between hours spent taking care of a child and hours doing other things is negatively associated with the enrollment of young children. According to column four, though, an additional child enrolled in school is associated with only about one hour less spent both caring and working.[8]

The analysis so far has not yet taken into account that women may well care for several children at once, and increasing the number of children a mother has to care for from, say, one to two would not necessarily mean that the number of hours she spends caring for young children increases as well. Consequently, in terms of time use, the more relevant factor may be whether or not *any* young children remain at home and need to be taken care of. The lower panel in table 4.2 thus adds an indicator for whether or not all of the mother's children below 6 years of age are enrolled in (pre-)school as an explanatory variable (this indicator will be called "full enrollment" in what follows). The results confirm that this indicator really is the variable driving the association between enrollment status of young children and time use of cohabiting mothers: while full enrollment is significantly associated with a large reduction in hours spent with child care, the coefficient for the number of children enrolled is small and insignificant.

Full enrollment is associated with a statistically insignificant and small increase in hours of work of 1.2. The coefficient for the overlap with other activities is statistically significant and negative, and, according to the rightmost column, the largest part of that

[8]As the last two columns indicate, that reduction in the total overlap between child care and other activities evenly over work activities and non-work activities. The categories captured by the dependent variables in columns four and five are mutually exclusive, and taken together, they are equal to the total overlap used for column three.

Tab. 4.2: Enrollment status of preschool age children and time use of their cohibiting mothers

	Hours of care	Dependent variable: Hours working	Total overlap	Overlap with work	Overlap with oth. activities
# 0-5 year-olds in school	-2.84***	0.52	-1.42***	-0.91*	-0.51
	(0.66)	*(0.53)*	*(0.55)*	*(0.48)*	*(0.49)*
# 0-year-olds in household	2.60**	1.02	1.39	0.59	0.80
-	*(1.01)*	*(0.81)*	*(0.85)*	*(0.74)*	*(0.76)*
# 1-year-olds in household	3.47***	-0.78	1.42*	0.88	0.54
	(0.95)	*(0.76)*	*(0.80)*	*(0.70)*	*(0.71)*
# 2-5 year-olds in household	0.83	-0.34	-0.10	0.22	-0.32
	(0.82)	*(0.66)*	*(0.69)*	*(0.60)*	*(0.61)*
Controlling for whether all young children are enrolled					
All <6 years are enrolled	-5.69***	1.18	-3.14***	-0.88	-2.26**
	(1.20)	*(1.01)*	*(1.04)*	*(0.93)*	*(0.94)*
# 0-5 year-olds in school	-0.27	-0.01	-0.01	-0.52	0.51
	(0.83)	*(0.70)*	*(0.72)*	*(0.64)*	*(0.64)*
# 0-year-olds in household	0.83	-0.66	0.41	0.32	0.09
	(1.03)	*(0.87)*	*(0.89)*	*(0.80)*	*(0.80)*
# 1-year-olds in household	1.56	-0.38	0.37	0.59	-0.22
	(0.98)	*(0.83)*	*(0.85)*	*(0.76)*	*(0.77)*
# 2-5 year-olds in household	-0.66	-0.04	-0.92	-0.01	-0.91
	(0.84)	*(0.71)*	*(0.73)*	*(0.65)*	*(0.65)*

Sample: All mothers with at least one child between two and five years of age (Sample E, see table 4.9 in the appendix for details), N=197. Additional control variables: number of 6-12-year-old children, age of the individual Standard errors given in parentheses.. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$..

association is attributable to fewer hours of overlap between child care and activities other than work. After controlling for full enrollment, the coefficient for the number of young children enrolled is insignificant for all outcomes considered, confirming that full enrollment is a measure much more relevant to the time use of mothers than other enrollment measures. Thus, the remainder of this paper will focus on full enrollment as the explanatory variable of interest. Accordingly, in the following sections, it will be of primary interest how the evaluated preschool project not only affected the enrollment status of individual young children, but also how it affected the likelihood of their mothers to have all of their young children enrolled in (pre-)school.

In order to account for at least some of the individual heterogeneity that may affect both enrollment and time use decisions, I re-estimate the simple time use models dis-

Tab. 4.3: Enrollment status of preschool age children and time use of cohabiting mothers
Using data from both waves: first differences.

	Dependent variable:				
	Hours of care	Hours working	Total overlap	Overlap with work	Overlap with oth. activities
All <6 years are enrolled	-9.51***	2.09	-4.76***	-1.12	-3.64***
	(1.66)	*(1.32)*	*(1.42)*	*(1.26)*	*(1.18)*
# 0-5 year-olds in school	0.80	-1.52*	0.18	-0.58	0.76
	(1.06)	*(0.85)*	*(0.91)*	*(0.81)*	*(0.76)*
# 0-year-olds in household	0.74	1.25	1.80*	1.16	0.64
	(1.23)	*(0.98)*	*(1.06)*	*(0.94)*	*(0.88)*
# 1-year-olds in household	-1.25	1.62	0.27	0.51	-0.24
	(1.29)	*(1.03)*	*(1.10)*	*(0.98)*	*(0.92)*
# 2-5 year-olds in household	-2.16*	1.85**	-0.81	0.44	-1.25
	(1.15)	*(0.92)*	*(0.99)*	*(0.88)*	*(0.82)*

Sample: the analysis used observations for all women in sample E (see 4.9 in the appendix for details) who are also found in the 2008 data and for whom all variables used in the analysis have no missing values. The sample size is equal to 248, i.e. observations for 124 women are included. Additional control variables: number of 6-12-year-old children, age of the individual Standard errors given in parentheses.. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

played in the lower panel of table 4.2 using observations for mothers who are observed in both waves of the household survey conducted in the community in 2008 and 2011 in order to add individual fixed effects. The results are shown in table 4.3. Qualitatively, the results shown in the two tables are almost identical. Sizes of some coefficient and standard error estimates differ in a few cases. For instance, the negative coefficient for full enrollment in all models is larger.

Taken together, the descriptive results in this section suggest that mothers whose young children are all enrolled spend much less time caring for young children than mothers who have at least one young child remaining at home. However, the association of full enrollment with hours of work is small. Rather, hours spent with both child care and other, non-work activities are reduced by full enrollment. Furthermore, since the additional hours of work and the reduced hours of overlap between care and other activities do not add up to the reduction in hours of care, the residual category (leisure) must be positively associated with full enrollment.

Decisions regarding fertility, child care arrangements, and labor supply might well be affected by common unobserved factors that are not sufficiently accounted for by taking first differences between observations from two points in time. Tables 4.21 and

4.22 inspect whether survey questions regarding opinions on child care arrangements, female labor supply, etc., capture any such individual heterogeneity that is both relevant to enrollment decisions to mother's time use. If enrollment decisions are not correlated with opinions that are also relevant for time use decisions, then enrollment is more plausibly an exogenous variable in models of times use.

Regarding time use decisions, several significant associations are observed. A mother of 2- to 5-year-old children who indicates that, if she are caring for a young child, the child would have to be relatively old before she can start working again, tends to both spend less time caring for a child and to work fewer hours. Those who indicate that they would want to spend as much time with their child as possible tend to spent fewer hours with caring for children. Mothers of 2- to 5-year-old children who tend to attribute a relatively high value to (pre-)school education of children spent more hours with caring for children.[9] On the other hand, these opinions are generally not significantly associated with full enrollment, es can be seen from table 4.22. There is one exception, however. Mothers who to attribute a relatively low value to education relative to other family needs actually have a higher probability of full enrollment. Thus, some mothers are both spending fewer hours with child care and more likely to enroll all their young children due to unobserved characteristics manifesting itself in the opinions just described.

Although these latter effects are not very significant in terms of their size, this coarse look into opinions affecting both enrollment and time use decisions illustrates that simple regressions of time use on enrollment may, potentially, not be appropriately taking into account all characteristics which affect both of these variables. Thus, in order to obtain a credibly exogenous source of variation in enrollment, an experiment was carried out in the studied community where access to a preschool program has been randomized. The following section describes the experiment and how it affected child care arrangements. Section 4.4 shows how the variation in school accessability caused by the experiment can be exploited in order to estimate the causal effect of child care arrangements on female labor supply.

[9] Furthermore, knowledge of the NGO and the details of the preschool project further described in section 4.3 are strongly negatively associated with hours of child care, and it is positively associated with hours of work, but, particularly with respect to child care, this association may also be the result of the preschool project.

4.3 Randomized preschool admission: compliance and enrolment status of young children

The preschool program was initiated by a small NGO based in the studied community. During group discussions with the population, members of the association established that the population considered the introduction of institutionalized child care as the community's need with the highest priority. The NGO searched for external funding for such a project, and a small German NGO agreed to support the construction of a new preschool as well as the training of preschool staff. Given the uncertainty regarding the actual demand for preschool enrollment, the participating associations decided to take relatively conservative projections as a starting point. They planned for a maximum of one class per cohort with with class size not exceeding 40 children - even though cohort sizes of children in the relevant age range in the community typically exceed 100. In case where actual demand would exceed 40 children per cohort, the NGOs agreed to randomize access in order to give all signed up children equal chances of access.

In April 2010, while the construction was still ongoing, parents were asked to sign up children for the new school year, scheduled to start in late September of that year. At the same time, parents were informed about the rules of the inscription process, i.e. that some signed-up children might not be accepted in case a randomization would become necessary. The preschool accepted applications for children who would be between 2 and 5 years of age in fall 2010. Applicants were divided into three age groups: the group of the youngest (children born in 2007 and later, called "first grade of preschool" in what follows), a second group of children born in 2006 (called "second grade of preschool"), and a third group of children born earlier than 2006 . If the number of applicants exceeded 40 in an age group, a lottery was carried out in order to determine which of the applying children would be accepted. For the group of the oldest children, the number of applications did not exceed 40. For the middle group and the group of the youngest, the number of applicants was 61 and 86, respectively. Consequently, a randomization was carried out for both the first and the second grade of preschool.

The randomization procedure was carried out in public. The town chief, directors of all primary schools as well as parents representing the town's quarters attended it upon invitation. I was able to control the details of the procedure. For each preschool grade, I printed out a list of random numbers which were drawn by generating, separately for each grade of preschool, uniformly distributed random integers over [1,K] in Stata, where K denotes the number of applications for a grade. I asked a member of the audience to arbitrarily choose a starting point in a list (each list included 500 integers). Another member of the audience was then asked to read out loud one random number after another. On the lists of signed-up children, rows were ordered according to when the respective child had been signed up. If a random number that was read out loud matched a row number on the list, the child was marked as accepted. If the same number appeared again, nothing was changed. This process was stopped as soon as the target of 40 children per class was reached. In section 4.4, which compares mothers of accepted children to mothers of children not accepted for preschool, it will be discussed whether this randomization succeeded in balancing out differences between admitted and non-admitted children with respect to observable characteristics.

Until the start of the preschool project in October 2010, the studied community did not have access to institutionalized child care. This did not mean, however, that all children younger than the standard school entry age of six years had to stay at home. Since they faced pressure by young mothers to accept children younger than six years for the first grade of primary school (as a substitute for daycare), it was common in the community's four primary schools to find children of 5 years of age, and even 4- and 3-year-old children. This phenomenon is in line with an increase in demand for preschool education in Africa, which, as perceived by Mwaura et al. (2008), is partly attributable to a combination of stronger economic pressure on women to work and a shift away from traditional extended family networks, while the supply of preschool admission opportunities remains low. However, the likelihood of being accepted at a primary school dropped dramatically the younger a child was.[10] Thus, the community's parents' demand for access to any institution caring for their young children by far exceeded the few accessible spots in primary school.

Table 4.4 shows the status of applying children regarding residency and enrolment

[10] In 2008, 5.9 percent of 2-year-old children were enrolled, in addition to 11 percent of 3-year-old children, 45.4 percent of 4-year-old children, 65.3 percent of 5-year-old children, and 84.9 percent of 6-year-old children. In 2011, the figures for these same age groups were, respectively, 0, 14.6, 32.9, 62.9, and 72.9 percent.

Tab. 4.4: Compliance with the randomization of preschool admission

	Accepted	Not accepted
	Children signed up for first grade	
staying at home	11	22
enrolled in primary school	7	2
enrolled in preschool	12	9
moved away	2	2
died	1	0
household not interviewed	7	11
total	40	46
	Children signed up for second grade	
staying at home	10	1
enrolled in primary school	17	12
enrolled in preschool	4	1
moved away	1	2
died	0	0
household not interviewed	8	5
total	40	21

Sample: All children participating in the randomized preschool admission procedure in April 2010.

status as of January 2011. As can be seen from the table, out of the total of 147 children who participated in a lottery, a significant number was not part of a household interviewed during the 2011 survey. 31 children were from households that did not participate in the survey at all. These households have either moved out of the community between April 2010 (when the inscription process and randomization took place) and January 2011 (the survey period), or they could not be contacted during the survey (usually due to temporary absence, and, in very few cases, refusal). Furthermore, 8 children used to belong to a household which was interviewed but were no longer present in January 2011. Usually, these children had left in order to live with a different part of the same family but outside the studied community (as indicated by the remaining household members during the interview; numbers not shown). Pooling both lotteries, the likelihood of a child showing up in the household data is 76.3 percent in the group of accepted children versus 70.1 percent in the group of children who were not accepted for preschool. I will assume that the likelihood of being accepted is not correlated with unobserved factors that drive selection into the household survey sample.

Focussing on children who were present in households interviewed during the 2011 survey, it becomes apparent that compliance with the randomization was far from perfect. There are accepted children who were eventually not sent to preschool, and there are also children who were not accepted during the randomized admission procedure but who were eventually enrolled in preschool anyway. Table 4.19 provides a first impression regarding potential causes for the imperfect compliance with randomization. Households with eligible children where asked in January 2011 whether they send their child to preschool, and they were asked to indicate reasons for not doing so in case they did not. The table shows frequencies for responses by those households who did not send their child to preschool even though their child was admitted to preschool. The main reasons indicated are that they preferred to send the child to primary school, that the child was considered to be too young, and that the distance to preschool was considered too far. The latter two answers indicate that a problem may have been that households were not informed well enough, despite the NGOs efforts to reach the whole population through information campaigns surrounding the admission procedure in April 2010. Had they been informed well enough, parents of children participating in the admission procedure should have been able to judge their child's readiness for preschool and the distance to the preschool before deciding to sign up for admission. Lack of information as a major explanation for poor compliance is consistent with another odd result shown in table 4.19: out of the households with admitted children, five apparently thought that their child was *not* accepted for preschool.

However, even though compliance was imperfect, the randomization did create some variation in enrollment status of the children in the household survey sample. For children signed up for the first grade of preschool, the likelihood of actually attending preschool amounts to 40.0 percent for accepted children as opposed to 27.3 percent for children who were not accepted based on the randomization. For children signed up for the second grade of preschool such a comparison is not warranted since there is only a total of 5 children in that group who attend preschool.

This variation in preschool enrollment is definitely insufficient to identify the impact on time use of other household members. However, as discussed in the following section, the preschool program also caused variation in *primary* school enrolment which is just as relevant for the analysis, since preschool and primary school enrollment of all of a mother's young children together determine full enrollment, which, as motivated in section 4.2, is my enrollment measure of interest. Thus, the following section discusses how the preschool experiment induced variation in full enrollment, and how that variation can be exploited empirically in order to estimate the effect on mothers' time use.

4.4 Empirical strategy

Although, as discussed in the previous section, compliance with the randomization of preschool access was poor such that it did not cause significant variation in actual preschool enrolment, the introduction of the preschool program is likely to have affected enrolment decisions more broadly. As argued in this section, being accepted for preschool may have also increased the likelihood of enrollment in *primary* school, which is, in fact, observed for children signed up for the first grade of preschool. As discussed below, both effects together (the increase in preschool enrollment and the increase in primary school enrollment) translate into variation in full enrollment, that is, the likelihood that all of a mother's young children do not have to be cared for at home during the day increases. The main empirical strategy will consist of exploiting this variation in order to implement an instrumental variables model of the effect of full enrollment on time use of mothers. Furthermore, the strength of the instrumental variable is likely to interact with the age of a mother's youngest child, which is shown to imply a variant of the estimation strategy.

In order to better understand how the preschool program affected both preschool and primary school enrolment of all participants in the inscription process, it is necessary to recapitulate the specific conditions under which affected families were to make their enrolment decisions during the decisive period between April and October 2010. In April 2010, the randomization was carried out. Then, in September 2010, parents learned that there would be a delay in the preschool project. Finally, it was only after inscription procedures for primary schools had been completed that the preschool started operating by the end of October 2010.

While parents of accepted children had easier access to preschool than others, some of them, upon learning about the delayed start of the preschool project, may have tried to enroll their child in primary school instead. However, assuming that before the introduction of the preschool, admissions to primary school were sufficiently below actual demand, then the enrollment of accepted children in any institution (either preschool or primary school) can be expected to be higher than it would have been in the absence of the preschool.

For accepted children, enrollment may have also increased due to a reduction in uncertainty caused by the experiment. Primary school enrolment procedures usually take place during the first week of a new school year. Since primary schools, as described in section 4.3 were more reluctant to accept a child the younger a child was, before the start of the preschool project parents of very young unenrolled children faced a great deal of uncertainty regarding their child's future enrollment status. Since parents of accepted children knew with certainty that their child would have access to some institution (i.e. upon learning about the randomization results in April 2010), they were probably more likely than they otherwise would have been to make arrangements that depended on their child being enrolled. For example, the reduced uncertainty may have induced them to search for employment opportunities, save for school tuition and equipment, etc.

In fact, as the data show, enrollment in both preschool *and* primary school actually is higher for accepted children among the children who were signed up for the first grade of preschool (according to table 4.4), because accepted children are more likely to be either enrolled in preschool or in primary school. Taken together, whether or not a child is enrolled in preschool or in primary school determine whether a child stays at home during the day. According to the data, the likelihood of staying at home is equal to 33.3 percent for accepted children versus 66.6 percent for children not accepted for preschool.

For children who were signed up for the second grade of preschool, however, the impact of randomly gaining preschool access on enrollment in preschool or in primary school taken together is reversed. In particular, accepted children are less likely to be enrolled in primary school. For these children, the likelihood of staying at home is equal to 32.2 percent for accepted children versus 7.1 percent for children who were not accepted. One could think of reasons why even a negative effect of being accepted for preschool might in fact reasonable[11]. However, given the small sample size for children

[11]A situation where the impact of being accepted for preschool on the likelihood of total enrollment is ambiguous might accur when the introduction of the preschool program is likely to have positively affected enrollment rates not only for accepted children but also for remaining participants. As long as the size of these effects cannot be determined theretically it is not clear a priori for which of the two

signed up for the second grade of preschool, such differences between randomly chosen groups may as well be the result of sampling variation. In fact, as will be seen in section 5.4, when focussing on mothers of children signed up for second grade of preschool, the impact of having a child accepted for preschool on enrollment becomes insignificant when controlling for characteristics of the mother and the mother's household. As regards access to first grade of preschool, on the other hand, it will be shown that the positive effect on enrollment is robust, even though, for the first grade experiment, the sample size is still relatively small.

Since the focus of the analysis is the time use of mothers, it is necessary to link the sample of students participating in the randomized preschool admission procedure to the corresponding sample of mothers. Out of the 63 children who were signed up for the first grade of preschool and who were recovered in the household data, 60 children live with their mother. Two of these children are of the same mother, so the sample of mothers has a size of 59. Out of the 45 children who were signed up for the second grade of preschool and who were recovered in the household data, 36 children live with their mother [12] , and none of these children are of the same mother. As discussed in section 4.3, whether or not a child signed up for first grade was accepted for preschool can be considered random. Consequently, for the mothers in the sample, indicator of whether or not their child has been accepted, is a random variable.[13]

Translating the effect of randomly gaining preschool access on mother's time use into an empirical mothers, for each of the two lotteries, observations for all mothers of participating children can be used to implement a simple comparison between mothers

groups groups the impact has been stronger. For individuals not accepted for preschool, accessability of primary school may have risen relative to a counterfactual situation without the preschool program. For instance, participating in the preschool inscription process may have increased their awareness of the benefits of enrolling their child. Furthermore, the notification of not being accepted for preschool may have encouraged them to seek enrollment for their children in an alternative institution (i.e. primary school). In addition, after the completion of primary school enrollment procedures, parents of "left over" children from the group of children not accepted for preschool may have been more likely than they otherwise would have been to succeed in enrolling their child in preschool because they could argue that their child's name can be found on "some list".

[12]In West Africa, it is very common to observe that children are fostered-out to live with relatives other than their parents, and the likelihood for a young child to be fostered-out rises with its age (Akresh (2009), Serra (2009), Glewwe and Jacoby (1994)).

[13]For the one mother who signed up two 3-year-old children the indicator for being accepted for preschool is given a value of 0 since none of the two children has been admitted.

of accepted children and mothers of children not accepted for preschool:

$$y_i = \sigma_P P_i + \beta X_i + \epsilon_i \tag{4.1}$$

where y_i is the dependent variable of interest, i.e. a measure of time use. P_i is a dummy variable indicating whether a woman has a child accepted for preschool. X_i includes additional control variables. Since the values of P_i have been determined randomly, σ_P should identify the causal effect of being accepted for preschool on the time use of mothers.[14]

The focus of this study is to investigate how a mother's child care obligations affect her time use, so the treatment variable of interest must be some measure of (pre-)school enrollment of young children, which, in turn, determines how many hours of care these children need at home. As outlined in section 4.2, a good candidate for such a variable would be an indicator for full enrollment which measures whether or not all of a mother's young children are enrolled an any institution (preschool or primary school). For several reasons, this measure suggests itself for being used to define treatment in the context of this study.

First, full enrollment is a measure that is likely to map into effective changes in child care responsibilities, as suggested by the descriptive results from section 4.2. The fact that it appears to capture actual variation in the child care duties better than other enrollment measures is consistent with the notion that, in an environment where many mothers have several young children, enrollment of a single child can be expected to reduce the number of hours a mother needs to stay at home to a very limited extent as long as the remaining young children are not enrolled.

Second, is a measure that is significantly affected by the introduction of and participation in the preschool project, such that the random variation created by the preschool project can be exploited for the identification of treatment effects. In other words, the effect of the preschool experiment on enrollment in preschool and in primary school de-

[14]If treatment were defined as having a child attending preschool, then σ_P could be interpreted as the intention-to-treat effect of having a child attending preschool, given that compliance with the randomized admission procedure is imperfect. However, as discussed below, regarding the particular setting of this study, it is convenient to define "treatment" whether or not all of a mother's young children are enrolled.

scribed above translates into variation of the indicator of full enrollment for the sample of mothers of children signed up for preschool admission. According to a comparison of means displayed in table 4.5, as of January 2011, right after the start of the preschool project, the difference in the likelihood of full enrollment between mothers of accepted children and mothers of children who were not admitted is equal to 30 percentage points for the grade 1 experiment.[15] For the grade 2 experiment, the difference is equal to -19 percentage points. As mentioned above, the results presented in section 5.4 will show that this negative effect for children signed up for second grade is not robust to the inclusion of control variables. For the first grade experiment, however, the relationship is robust. Thus for mothers who have a child signed up for first grade of preschool, the effect of being accepted on full enrollment provides a first stage relationship for instrumenting full enrollment.

Defining full enrollment as the treatment variable and using an indicator for being accepted for preschool as an instrumental variable for full enrollment, the relationship of interest can be captured by the following model:

$$y_i = \rho E_i + \beta X_i + \epsilon_i \tag{4.2}$$

where y_i and X_i are defined as before, and E_i denotes full enrollment. The hypothesis is that treatment reduces hours of child care, and that it potentially increases the time mothers spend with other activities such as work. However, since y_i and E_i can be expected to be determined by common unobserved factors, OLS estimates of ρ would be subject to endogeneity bias, so an instrumental variables strategy is preferred. The following equation constitutes the first stage relationship for instrumenting E_i in equation 4.2:

$$E_i = \delta P_i + \gamma X_i + \mu_i \tag{4.3}$$

[15]The number of observations used for computing the mans in table 4.5 is smaller than 59 (the number of mothers of children in the data who were signed up for preschool admission and who live with their mother) because it excludes observations for which variables used for the analysis described in section 5.4 are missing (see table 4.9 in the appendix for details), and because it uses only observations for women observed in both waves of the survey data.

Tab. 4.5: Having a child accepted for preschool and means of full enrollment

Participants in randomization, preschool grade 1

	All mothers			Youngest child>1			Youngest child<=1		
		2008	2011		2008	2011		2008	2011
Child accepted	N=18	.06	.39	N= 9	0	.78	N=9	.11	0
Child not accepted	N=22	.09	.09	N=13	.15	.15	N=9	0	0

Participants in randomization, preschool grade 2

	All mothers			Youngest child>1			Youngest child<=1		
		2008	2011		2008	2011		2008	2011
Child accepted	N=16	.19	.31	N=13	.23	.38	N=3	0	0
Child not accepted	N=10	.1	.5	N= 6	.17	.83	N=4	0	0

Sample: For the upper panel, calculations are based on all observations from sample F where the respective variables are observed in both waves. For the lower panel, calculations are based on all observations from sample G where the respective variables are observed in both waves. See table 4.9 in the appendix for details.

where all variables are defined as above and P_i, the indicator for having a child accepted for preschool, is the instrumental variable.

In order to assess how balanced mothers of children accepted for first grade of preschool and mothers of non-admitted children are with respect to observed characteristics, columns I and J in table 5.6 in the appendix compare means of various socio-demographic variables for these two groups of mothers. No sharp differences are observable. However, mothers of children not accepted for preschool tend to live in households with fewer adult cohabitants (who are, accordingly, accumulating somewhat less household income), and, on average, their adult cohabitants score lower in tests of cognitive ability. A strategy in section 5.4 will be the stepwise inclusion and exclusion of most of the variables mentioned in table 5.6 in the appendix in order to check whether empirical results are sensitive to such changes in specification.

As another view on whether randomization was successful in the sense that there are no significant differences between the groups prior to the randomization table 4.5 compares mothers of accepted children to those whose children were not accepted by indicating averages of full enrollment in 2008, two years prior to the start of the preschool program. Mothers whose children would later be accepted for grade 1 of preschool had a somewhat smaller likelihood of full enrollment in 2008 than mothers of children who would not be accepted. For the grade 2 experiment, the reverse holds true. Even though the existence of these relatively small differences prior to the start of the preschool program are likely to be attributable to the small sample sizes, a strategy to account for such heterogeneity will be to include values of the outcome variable in 2008 in X_i as additional controls. Note that, according to table 4.5, the likelihood of full enrollment increased between 2008 and 2011 for all groups of mothers. This is most likely due to the fact that most of the children signed up for preschool in April 2010 had been infants in fall 2008, which, for respective mothers, would automatically imply that E_i takes on the value zero.

This latter point illustrates that, when analyzing full enrollment, it is important to take into account the interaction between the likelihood of treatment affecting the amount of time a mother needs to devote to child care and the age of children. In particular, since school accessability is still increasing with age for young children even after the introduction of the preschool[16], and because children younger than two are not eligible for preschool, the youngest child is, on average, the least likely to attend school. Thus, changes in the enrollment status of the youngest child are, more likely to directly affect full enrollment. For similar reasons, the age of the youngest child has frequently been the explanatory variable of focus in studies investigating associations between fertility and labor supply of mothers (e.g. Lehrer (1992)).

Accordingly, the age of the youngest child should have a strong effect in regressions such as model 4.3, and including it in the model may increase the precision of the coefficient estimate of the instrumental variable, thereby increasing the instrument's strength. More precisely, the age of the youngest child will be taken into account by including a woman's number of children who were younger than two years in January 2011 as a control variable for all regressions.

In addition, employing a variant of equation 4.3, the information regarding the age of the youngest child can be exploited by differentiating the effect of admission to preschool between the impact it has on mothers whose youngest child is at least two years old and the impact it has for mothers whose youngest child is slightly younger. Since for the former mothers, obtaining access to preschool is much more likely to actually result in full enrollment, the coefficient for P_i in the first stage regression should be much larger for this subgroup of mothers than for mothers of younger children. In other words, the first stage relationship can be reformulated as the following difference-in-differences

[16] See footnote 10

model:

$$E_i = \sigma_P P_i + \sigma_A A_i + \delta_P (P_i \times A_i) + \beta X_i + \epsilon_i \tag{4.4}$$

where A_i is a dummy variable indicating whether a mother belongs to the subpopulation hypothesized to be more likely to be affected by the introduction of the preschool program (in the sense that for them, the preschool program may have effectively increased the likelihood of full enrollment), which is the group of mothers whose youngest child is at least two years old. δ_P, the coefficient for the interaction between this indicator and the dummy variable indicating admission to preschool, is the respective difference-in-differences estimator. X_i includes additional control variables. $(P_i \times A_i)$ serves as instrumental variable.

This latter strategy essentially groups mothers into two different cohorts according to A_i, where cohort membership is defined according to whether a mother's youngest child was born in a specific year. The effect of randomized preschool admission as in equation 4.3 is then computed within each cohort, and afterwards, the difference between these two estimates is interpreted as the impact of having access to preschool for a child on full enrollment.

The means of full enrollment reported in table 4.5 already suggest that mothers whose youngest child is at least two years old in 2011 constitute a group for which the effect of admission to preschool on full enrollment is much more pronounced. Focusing on mothers of children signed up for the first grade of preschool, the respective difference in means of full enrollment is equal to 0.63 whereas mothers of younger children always have at least one child that is not enrolled, regardless of whether one of their children was accepted for preschool or not.

The age of the youngest child may itself by, of course, an endogenous variable. For instance, according to the simple model in Leibowitz et al. (1992), a mother's probability of (re-)entering the labor force rises with increasing age of her child, and that result is driven by two assumptions. Firstly, the costs of child care are assumed to decline with the age of the child. That is the mechanism that I want to exploit by comparing mothers whose youngest child is at least two years old to mothers of younger children;

since children younger than two years are not eligible for institutionalized care, child care costs fall discontinuously when a child turns two.[17] In addition, however, Leibowitz et al. (1992) assume that the mother's reservation wage decline with the age of the child.[18] The authors do not discuss exactly why this should be the case, but one can come up with a few suggestions: For instance, when the child grows older, providing care for it could generally become an easier task, decreasing the value of a woman's time in home production. In addition, women may worry about the appropriateness of specific child care arrangements for children under a certain age. On the other hand, there appears to be consensus among researchers that for children above the age of two, out-of-home care where the child interacts with other children is generally more appropriate than being cared for at home completely (see Leibowitz et al. (1988) and the psychological literature cited therein).

In order to rule out that systematic differences with respect to unobservable characteristics between the groups defined by A_i drive the results, observations for the same women from the survey prior to the introduction of the preschool program can be taken into account in order to formulate the first stage as a triple differences model:

$$E_{it} = \rho(P_i \times A_i \times T_t) + \sigma_P P_i + \gamma T_t + \delta_P (P_i \times T_t) + \sigma_A A_i + \delta_A (A_i \times T_t) + \eta (P_i \times A_i) + \beta X_{it} + \epsilon_{it} \tag{4.5}$$

where A_i and P_i are defined as above. ρ, the coefficient for the interaction between P_i, A_i, and the period indicator T_t is the respective triple differences estimator. X_{it} includes additional control variables which can now be time varying.

[17]For this reason, empirical labor supply models of mothers estimated in earlier studies typically account for child age by controlling for whether a mother's child falls into one of two age intervals, which are usually chosen at 0-2-years and 3-5-years (Duleep and Sanders (1994)), or they simply control for whether a mother has any preschool age children (Lehrer (1992)).

[18]In addition, many studies have focussed on taking into account that female labor supply and fertility may be the results of simultaneous decisions (Angrist and Evans (1998); see Agüero and Marks (2011) for a study using data from developing countries.). However, in this study, identification is motivated by the fact that for one cohort, the preschool project was already accessible, whereas the other cohort was till too young. It is unlikely that women in the studied community took into account the timing of the beginning of the preschool project when making decisions that affected their fertility outcomes.

As a variation of equation 4.5, individual fixed effects can be included in the model:

$$E_{it} = \rho(P_i \times P_i \times T_t) + \gamma T_t + \delta_P(P_i \times T_t) + \delta_A(A_i \times T_t) + \beta X_{it} + \tau_i + \epsilon_{it} \qquad (4.6)$$

where τ_i is the individual fixed effect which absorbs the variables P_i, A_i, and $(P_i \times A_i)$. X_{it} can now only include time varying covariates.

A concern regarding the implementation of the empirical strategy outlined in this section could be that it does not rely on information on all mothers of children of preschool age in the studied community but rather on a subsample of mothers who chose to sign up their child for admission to preschool in April 2010. It could be argued that these mothers differ from average mothers in aspects such as labor force attachment or in how well they are informed about the preschool project. Table 5.6 in the appendix confronts the issue by comparing means of a range of characteristics used throughout the analysis between participating mothers (in other words, those whose child was part of the randomized admission procedure), and non-participants (columns F vs. H). The general impression from inspecting these descriptive statistics (excluding the first four rows of the table, which show means for full enrollment as well as the dependent variables) is that the groups do not differ systematically with respect to observable socio-demographic characteristics.

In addition, table 4.20 documents how participants differ from non-participants with respect to various measures of opinions regarding child care arrangements, female labor supply, the preschool project and different social groups. In general, there are only a few statistically significant associations after controlling for other characteristics. There are a few exceptions, though. Surprisingly, mothers with high labor force attachment tend to be *less* likely to sign up a child for first grade of preschool. Moreover, mothers who value preschool education are *less* likely to participate. Not surprisingly, correct knowledge of the organizational details of the preschool project (as measured by the knowledge of the correct school fee) has a strong correlation with signing up a child.[19] However, regarding the interpretability of these results, since the respective responses

[19]surprisingly, the likelihood of signing up a child for second grade of preschool is negatively associated with having ever heard of the NGO, but this could be due to low sample size/few observations of women not having heard of the NGO.

were recorded in January 2011, they may simply reflect an outcome of the participating in the preschool project.

4.4.1 Proximity to the preschool construction site as a source of variation in the accessability of (pre-)school

As discussed above, preschool attendance is only partially determined by the result of the randomized admission procedure. Instead, 11 children who participated in the randomization and who were not accepted eventually ended up attending preschool. Furthermore, 7 children who did not participate in the admission procedure at all were attending preschool in January 2011 anyway.[20] Particularly regarding this latter group of children it becomes evident that the introduction of the preschool not only affected the subpopulation of participants in the randomization but rather the community's population as a whole. Accordingly, there must be additional determinants of the likelihood of enrolling a child in preschool which could potentially serve as instruments for full enrollment. A distinction of such a strategy from the one discussed in the previous section will be that the focus shifts from the population of mother who signed up their children for admission to preschool to the population of all mothers in the community who have children of preschool age. Among them, again, the group of mothers whose children attend preschool is likely to be selective. That is, had there not been a preschool program introduced to the community, it is still very likely that mothers whose children are attending preschool today would have differed systematically from other mothers with respect to their enrollment behavior and their time use.

The households' distance to the preschool construction site provides an alternative source of variation in the likelihood of preschool enrollment (which translates into variation in full enrollment) that is independent from having participated in the randomization but still motivated by the introduction of the preschool program. That measure is likely to be associated with how strongly the introduction of the preschool program increased accessability of preschool for households with young children.

[20] In addition, 12 of the children attending preschool can not not be traced in the survey data. Overall, enrollment in preschool fell short of projections, particularly because so many 4- to 5-year-old children were enrolled in primary school rather than preschool, in spite of commitments made by primary school principals to abstain from this practice.

Note that, as mentioned above, at the beginning of the school year 2010/2011, primary schools still accepted children of preschool age. The studied community is relatively densely populated and hosts four primary schools. Accordingly, at least one school is in close walking distance (not more than 20-30 minutes) to almost any household in the sample. In addition, evidence regarding the impact of distance to school on enrollment of primary school age children in poor countries has been mixed (see Filmer (2007) for an overview), and the distances observed in the studied community would have to be regarded relatively low compared to distances found elsewhere. Thus, regarding primary school access, the distance to school does not constitute a significant constraint. However, this may not hold true for the youngest among the children of preschool age (i.e. 2- to 4-year-old children) before the introduction of the preschool. While older children may probably walk to school on their own, the very young children are more likely to need an accompanying older child or an adult. This implies that for parents of 2- to 4-year-old children, schools were less accessible when they lived further away from any school in 2008 than for other mothers.

Based on this reasoning, the relationship that may be expected to be observable between enrollment behavior and a household's distance to the preschool is relatively complex. Ceteris paribus, the introduction of the preschools increased the set of enrollment options particularly for those households, which live closer to the preschool than to any of the primary schools. In 2011, households were asked to indicate the walking distance in minutes from their home to each of the four primary schools and to preschool. Exploiting this information, the following difference-in-differences model can be estimated for women with 2- to 4-year-old children:

$$E_{it} = \sigma_C C_i + \gamma T_t + \delta_C (C_i \times T_t) + \beta X_{it} + \epsilon_{it} \tag{4.7}$$

where C_i is a dummy variable indicating whether a woman lives closer to the preschool construction site than to any of the primary schools in 2011. T_t is equal to 1 for observations from the survey after the introduction of the preschool program, and 0 if they are from the 2008 survey. δ_C, the coefficient for the interaction between these two indicators, is the respective difference-in-differences estimator. X_{it} includes additional control variables. Similar to the discussion in the preceding subsection, equation 4.7 could be employed as a first stage regression where $(C_i \times T_t)$ serves as instrumental variable for E_{it}. Alternatively, by replacing the dependent variable with a measure of time use y_{it}, the difference-in-difference estimate could be interpreted as the reduced form effect of improved access to preschool on time use of mothers. However, es can be seen from inspecting the coefficient estimates presented in table 4.18 in the appendix, even though the difference-in-difference estimate on full enrollment is positive and relatively robust to the inclusion of control variables it is estimated with very low precision. In the sample of mothers of 2- to 4-year-old children δ_C is identified by only 11 mothers who indicated to live closer to preschool than to any of the primary schools. Accordingly, the variation in C_i that can be observed in the data is not strong enough in order to reliably identify the effect of improved access to preschool.

One explanation for this result could simply be that distance is not a relevant criterion for enrollment decisions in the studied community. However, other aspects may be relevant as well. First, the distance to preschool may be measured with error, because some respondents were probably not yet very familiar with the exact location of the preschool. Second, in light of the discussion in section 4.3, the relationship that can be expected between distance to preschool and full enrollment is probably even more complicated. As outlined, enrolling 2- to 4-year-old children in the particular school of choice may have been more difficult than for 5-year-old children, because primary schools were more reluctant to accept too young children the younger a child was. Consequently, for them, the closest *accessible* school may not always have been the school that is also the closest one geographically.

4.5 Results: enrollment status of young children and time use of cohabiting women

Turning first to the results from estimating equation 4.1 for participants in the first grade randomization (table 4.15 in the appendix), which shows the effect of being admitted to preschool on mothers' time use, the direction of the effect on hours of care is consistent with the expectation that being accepted for preschool reduces child care responsibilities on average. The small size of the coefficient (less than one hour for six out of seven specifications[21]) can be explained by low compliance with the randomization. Since, in addition, standard errors of the coefficient estimates are large due to the small sample size, in none of the specifications the estimate is found to be statistically significant. The estimates of both the effects on hours of work and on the overlap between time spent caring for children and time working are all very small and statistically insignificant; the effect on hours of overlap between work and child care is, unexpectedly, estimated to be positive. Although the small and insignificant effects on work may also be partly due to low compliance with the randomization, they suggest that the true effect of a reduction in child care responsibilities in response to the preschool program on work is in fact very small or even absent.

Repeating the same analysis for participants in the second grade randomization (rows 2 to 4 of table 4.16 in the appendix) yields a few surprising results. Being accepted for preschool is estimated to have a very large and *positive* effect on hours of care and hours of overlap. The results for hours of work are inconclusive. However, both the size of coefficient estimates and the size of estimated standard errors vary across specifications, particularly comparing specification (5) to the remaining columns. Furthermore, as seen in the first row of the same table, using the full enrollment indicator as dependent variable yields very small and statistically insignificant effects of being admitted to preschool for most sets of control variables. I thus conclude that, even though admission to the second grade of preschool was randomized, mothers of admitted children differ systematically from other mothers with respect to characteristics that affected both their likelihood of enrolling young children and their time use decisions. Consequently, the second grade experiment did not result in variation in enrollment behavior that could be exploited in order to evaluate the effect of enrollment on time use decisions of mothers, and the remaining analysis will no longer use observations from the second grade experiment.

Focusing on the first grade experiment, table 4.6 summarizes the results from esti-

[21]For all estimated equations, results from several specifications are shown in order to assess the robustness of coefficient estimates. Results in the first column show raw effects without additional controls. For the result in the second column, the number of children younger than two has been added as a control variable. The remaining specifications add varying combinations of further socio-demographic controls. Further details are given in the tables.

mating the instrumental variables model described by equations 4.2 and 4.3. The first stage coefficient estimates of the indicator for being accepted for preschool (first row in table 4.6) indicate that a first stage relationship does exist, however, due to the limited compliance with the randomization, the first stage relationship is not very strong. Conditional on covariates, a mother's likelihood of having all young children enrolled rises by 18 to 21 percentage points if her child has been accepted for preschool, depending on the specification. As hypothesized in section 4.4, controlling for the number of children younger than two years increases the precision of the coefficient estimate for the first stage effect of the instrument, although only marginally. At the same time, the size of the coefficient estimate increases a little. Due to both these effects, the first stage relationship is statistically significant only if at least the number of children younger than two years are controlled for. Even then, however, the first stage relationship is still relatively weak, as indicated by the respective F-statistics indicated in the second row of table 4.6, which vary between 3.0 and 4.2 conditional on varying sets of control variables. Consequently, resulting IV-estimates of the effect of full enrollment on time use should be interpreted with care since they are, potentially, subject to a weak instrument problem. Regarding the validity of the instrument, I consider the first stage coefficient estimates displayed in columns (2) through (7) of the first row in table 4.6 to indicate robustness to the inclusion of varying sets of control variables. Given that the sample size is small, minor fluctuations in estimated effect sizes should not contradict the assumption that the instrumental variable is exogenous.

The IV-estimates for the effect of full enrollment on three different categories of time use are displayed in rows 3, 4, and 5 of table 4.6. They confirm the basic result of table 4.15 in the appendix which was based on estimating the reduced form effect of the instrument. The effect on hours of care for young children is consistently negative, i.e. mothers who enrolled all of their young children in response to the improved school access induced by the preschool experiment spend systematically fewer hours caring for children. However, the effect is not statistically significant. Conditional on covariates, the estimated effect varies between -3.4 and -6.0 hours. For both time use categories related to work, estimated coefficients are generally much smaller (usually less than one hour), and they are never statistically significant.

In order to further account for heterogeneity relevant to the time use decisions inves-

Tab. 4.6: Instrumental variable estimates using admission to first grade of preschool as an instrument, women observed in 2011

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First stage effect of instrument	0.16	0.20*	0.20**	0.20**	0.18*	0.18**	0.21**
	(0.12)	*(0.11)*	*(0.10)*	*(0.10)*	*(0.10)*	*(0.09)*	*(0.10)*
First stage F-statistic	1.62	3.18	4.18	3.94	2.96	3.80	4.24
IV-estimate: hours of care	-1.56	-4.36	-4.54	-4.48	-5.10	-3.36	-6.03
	(9.71)	*(6.36)*	*(6.33)*	*(5.81)*	*(7.07)*	*(6.38)*	*(5.98)*
IV-estimate: hours of work	0.87	0.90	0.30	0.58	-1.12	0.37	-0.43
	(6.42)	*(5.18)*	*(5.04)*	*(4.75)*	*(5.92)*	*(5.47)*	*(4.91)*
IV-estimate: hours of overlap	0.84	3.67	0.88	0.49	0.90	1.50	0.49
	(5.17)	*(8.88)*	*(5.05)*	*(4.72)*	*(5.84)*	*(5.50)*	*(4.85)*

Sample: Observations from Sample F (see table 4.9 in the appendix for details; N=47). *Specifications:* see explanations for table 4.15. The "First stage F-statistic" is based on a test of significance of the instrumental variable in the first stage. Standard errors given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

tigated, I estimated the IV model controlling for individual 2008 values of the respective dependent variable as well as for the number of young children in 2008. This reduces the sample size from 47 to 40, because some of the women in the original sample are only observed in 2011. Table 4.17 in the appendix presents results from estimating the IV model for that subsample, but not yet controlling for previous outcomes. Evidently, the women observed in both data waves are a selective sample. For them, the first stage relationship is also very robust, and the size of the estimated impact of the instrumental variable is larger. Accordingly, the first stage F-statistic becomes larger. A more pronounced first stage relationship for these women could be due to the fact that they, presumably, have been living in the community for a longer time and thus may be more familiar with the communities institutions, providing them with generally better school access, and amplifying the effect of the preschool program on enrollment behavior. The second stage results regarding the impact of full enrollment on time use are comparable with those presented in table 4.6. However, the absolute size of the effect on child care tends to be smaller, and the effect on hours of work is still small but consistently *negative*.

Adding 2008 values of the respective outcome variable as well as the number of young children in 2008 results in coefficient estimates presented in table 4.7. In columns (3) through (7), which add various sets of additional controls, the size of the effect of the instrument in the first stage is even further increased, resulting in first stage F-

Tab. 4.7: Instrumental variable estimates using admission to preschool as an instrument, controlling for outcomes in 2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First stage	0.29**	0.29**	0.41***	0.43***	0.40***	0.37***	0.41***
	(0.13)	*(0.12)*	*(0.12)*	*(0.12)*	*(0.13)*	*(0.11)*	*(0.12)*
First stage F-statistic	4.89	6.36	11.38	11.81	10.50	10.20	9.86
IV-estimate: hours of care	-3.32	-3.53	-3.99	-2.56	-4.65	-3.12	-4.07
	(5.47)	*(4.81)*	*(3.78)*	*(3.57)*	*(3.82)*	*(3.88)*	*(3.85)*
First stage	0.29**	0.30***	0.40***	0.41***	0.40***	0.37***	0.39***
	(0.13)	*(0.11)*	*(0.11)*	*(0.12)*	*(0.13)*	*(0.11)*	*(0.12)*
First stage F-statistic	5.32	6.97	10.39	9.99	10.57	9.56	9.13
IV-estimate: hours of work	-0.12	-0.07	-0.78	-0.11	-1.44	-0.75	-0.88
	(3.93)	*(3.79)*	*(3.04)*	*(2.81)*	*(3.03)*	*(3.10)*	*(3.08)*
First stage	0.30**	0.31***	0.43***	0.45***	0.42***	0.39***	0.44***
	(0.13)	*(0.12)*	*(0.11)*	*(0.12)*	*(0.13)*	*(0.11)*	*(0.12)*
First stage F-statistic	5.20	6.89	12.08	12.38	11.26	10.47	10.47
IV-estimate: hours of overlap	1.11	0.66	0.91	1.48	-0.01	0.91	0.92
	(4.54)	*(3.72)*	*(3.35)*	*(3.23)*	*(3.26)*	*(3.51)*	*(3.46)*

Sample: See explanations for results in table 4.17 (N=40). *Specifications:* see explanations for table 4.15. In addition, all IV regressions control for the 2008 value of the outcome variable, i.e. the measure of time use, and they include the number of young children in 2008 as a control variable. Standard errors given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

statistic of around 10. The remaining estimates confirm the results discussed above. Full enrollment leads to consistently fewer hours of child care, with the reduction usually being estimated to be at least three hours. The effect is still not statistically significant, though. The impact of full enrollment on time uses associated with work is usually estimated to be less than one hour, and it is always statistically insignificant.

Table 4.8 shows results from estimating equations 4.4 and 4.5 where the idea is to focus on subpopulations that are more likely to be affected by the preschool program. The first two columns of the upper panel display coefficient estimates form implementing a difference-in-differences model which compares the difference in the likelihood of full enrollment between mothers of accepted children and mothers of non-admitted children for mothers whose youngest child is at least two years old to the respective difference for mothers whose youngest child is younger than two years. Looking at the raw difference in means (column 1), the strategy of the model is successful in the sense that the estimated impact of being accepted for preschool on full enrollment actually is estimated to be larger for this subpopulation. However, the effect is not robust to the inclusion of control variables (column 2). Taking observations from 2008 into account by estimating the triple difference model 4.5 even further increases the estimated effect of being accepted for preschool. When adding control variables, the change in the likelihood of full enrollment induced by admission to preschool between 2008 and 2011 is estimated to be 86 percentage points higher for mothers whose youngest child is at least two years old in 2011 in comparison to mothers whose youngest child is younger than two. The lower part of table 4.8 shows coefficient estimates for models that take a measure of time use as the dependent variable. The reduced form effect of the instrument is the coefficient of δ_P in equation 4.4 when replacing the dependent variable with a time use measure (columns 1 and 2), or the coefficient of the ρ in equation 4.5 when replacing the dependent variable with a time use measure (columns 3 and 4). The IV-estimates result from a model where full enrollment is instrumented by either using model 4.4 (columns 1 and 2) or model 4.5 (columns 3 and 4) as the first stage. Results indicate that in models where the first stage relationship is reasonably strong (columns 1, 3 and 4), full enrollment is estimated to have a negative effect on hours of child care. The effect on hours of work is again estimated to be very small and statistically insignificant. In contrast with results shown in tables 4.6 through 4.7, the effect on hours of care is estimated to be marginally statistically significant. This is due to a large (and probably unreasonable) increase in the size of the coefficient estimate which more than counterbalances the increase in standard errors. The increase in standard errors is due to the fact that, when narrowing down the investigation to mothers whose youngest child is at least two years old, the subpopulation of mothers who can possibly comply with the instrument is, inevitably, smaller.

Thus, based on the first grade experiment, which created the stronger and more credible variation in enrollment behavior than the randomized admission to second grade of preschool, I interpret the results as consistently showing that (in spite of the coefficient estimate's large standard errors) full enrollment reduces the time mothers cares for young children by at least three to four hours. However, this is neither accompanied with an increase in the number of hours worked, nor with an unambiguous reduction in hours of care overlapping with other activities.

4.6 Conclusions

Based on a field experiment conducted in a Togolese community, I exploited variation in enrollment behavior induced by randomized access to preschool in order to identify

Tab. 4.8: Triple difference estimates (models 4.5 and 4.6) for mothers who signed up their child for the first grade of preschool

	Dependent variable: all young children enrolled			
	no controls	controls	no controls	controls
Accepted · second wave · youngest>1			0.89***	0.86***
			(0.26)	*(0.26)*
Accepted · youngest>1	0.62***	0.36*	-0.26	-0.38*
	(0.19)	*(0.20)*	*(0.18)*	*(0.20)*
Accepted · second wave			-0.11	-0.09
			(0.19)	*(0.19)*
Second wave			0.00	0.00
			(0.13)	*(0.13)*
Accepted	0.00	0.15	0.11	0.11
	(0.14)	*(0.14)*	*(0.13)*	*(0.14)*
Second wave · Youngest>1			0.00	0.00
			(0.17)	*(0.17)*
Youngest>2	0.15	0.31**	0.15	0.26**
	(0.13)	*(0.14)*	*(0.12)*	*(0.13)*
First stage F-statistic	10.52	3.09	11.89	11.09
	Dependent variable: hours of care			
	no controls	controls	no controls	controls
Reduced form effect of instrument	-4.84*	-4.45	-9.36**	-8.78**
	(2.76)	*(3.46)*	*(3.81)*	*(3.67)*
IV-estimate: effect of full enrollment	-7.75*	-12.44	-10.53**	-10.18**
	(4.53)	*(9.67)*	*(4.68)*	*(4.28)*
	Dependent variable: hours of work			
	no controls	controls	no controls	controls
Reduced form effect of instrument	-0.94	0.19	-2.21	-1.54
	(2.40)	*(2.98)*	*(3.43)*	*(3.46)*
IV-estimate: effect of full enrollment	-1.51	0.53	-2.48	-1.78
	(3.75)	*(7.04)*	*(3.74)*	*(3.67)*
	Dependent variable: hours of overlap			
	no controls	controls	no controls	controls
Reduced form effect of instrument	-0.86	-1.91	-4.07	-3.88
	(2.24)	*(2.74)*	*(3.05)*	*(3.02)*
IV-estimate: effect of full enrollment	-1.38	-5.34	-4.58	-4.50
	(3.45)	*(6.97)*	*(3.44)*	*(3.30)*

Sample: For the first two columns, see explanations for results in table 4.17 (N=40). The third and forth column include the same sample of women, and they add observations from 2008 for these same women. *Specifications:* In columns 2 and 4, the following control variables were added: a mother's number of children in three different age groups (0 through 1 year, 2 through 5 years, and 6 through 12 years), the number of adults per household, age of the mother, dummy variables for the three most common religious denominations, and the number of adult cohabitants who work in agriculture. Standard errors given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the impact on mothers' labor supply. The studied community did not have access to institutionalized child care before the start of the program. Given that demand originally exceeded supply, access to the first two grades of preschool was determined by a lottery. Compliance with the randomization was poor. However, in particular institutional environment, primary schools also accept preschool age children, and, due to a short delay in the preschool program, the randomized preschool access also affected the likelihood of enrollment in primary school. As a result, for one of the two lotteries carried out (the one for the first grade of primary school), the randomization induced significant and robust variation in the likelihood of full enrollment, which is an indicator for whether all of a mother's young children are enrolled in either preschool or primary school. Full enrollment is argued to be more relevant to time use decisions of mothers, given that, if there are several young children present, enrolling one single child while the others remain at home may not effectively reduce child care responsibilities of the mother.

The variation induced by the first grade experiment is used to construct an instrumental variable for full enrollment. Results consistently show that full enrollment reduces the time mothers cares for young children by at least three to four hours, although, due to the small sample size, this effect is not found to be statistically significant. The systematic reduction in hours of child care is neither mirrored by an increase in the number of hours worked, nor are hours of care overlapping with work significantly reduced. These results are, qualitatively, consistent with OLS estimates of the effect of full enrollment on time use of mothers for the whole population of mothers of young children in the community (regardless of whether they participated in the randomization or not).

A potential explanation for the results is that they capture the effect of changes in enrollment behavior induced by the preschool program in the short run. The program started in October 2010, and individuals were surveyed in January 2011. This time span may have been too short for many women to adjust their schedule regarding economic activities. Since most of the women work on their household's farms or in small family enterprizes (e.g. sewer), increasing the amount of work might require to be accompanied by additional investments (such as land or raw material) which affected families may not have been able to make yet. In addition, it could be argued that participants in the randomized preschool admission procedure are selective, and that they differ systematically from other mothers with respect to unobservable characteristics which affect their time use decisions (as partly suggested by results from comparisons for these two groups of answers to survey questions regarding opinions on labor supply and child care arrangements). On the other hand, the descriptive results, which are not restricted to participants in the randomization, and which are driven not only by the short-run variation in enrollment induced by the experiment, lead to very similar results This suggests that the preschool program would not necessarily lead to different effects if evaluated after a longer time delay of if it were expanded to the expanded to cover the full population.

Thus, the results suggest that expanding the provision of public care for preschool age children would not lead to an increase in female labor supply. This would not allow to conclude, of course, not to invest in public preschools. A full evaluation of the benefits of early childhood education in a developing country like Togo would, naturally, have to take into account the direct effects on the children's cognitive and non-cognitive development. It has been argued that the economic importance of evaluating early childhood development programs in poor countries lies in the long-term consequences such programs may have on education, labor market outcomes, health, and poverty of affected children (Ghuman et al. (2005)). While such benefits of preschool programs in industrial countries (e.g. Head Start) have been well documented, the evidence from developing countries is limited to a small number of studies in educational research. Mwaura et al. (2008) found positive effects on cognitive development of children even within 1.5 years of time for a large East African preschool program. A study from Botswana showed that having attended preschool is associated with better subsequent achievement in primary school (Taiwo and Tyolo (2002)). Berlinski et al. (2009) find a similar result when evaluating the impact of a large expansion of the public provision of preschool in Argentina. Other studies have been concerned with whether child arrangements affect the physical development of young children in developing countries (Stansbury et al. (2000), Glewwe et al. (2001)). Usually, these studies are not exploiting credibly exogenous variation in enrollment, so the issue still provides interesting opportunities for (quasi-)experimental research. Not that, when evaluating the impact of preschool on child development in poor countries, variation in the quality of preschool

Tab. 4.9: Sample selection

<table>
<tr><td colspan="5">Sample A: individuals older than five years living in households with young children in 2011 in studied community, N=1280</td></tr>
<tr><td colspan="5">Sample B: individuals in Sample A with non-missing time-use data, N=1164</td></tr>
<tr><td colspan="5">Sample C: individuals in Sample B who are mothers of young children (younger than six), N=264</td></tr>
<tr><td colspan="5">Sample D: individuals in Sample C without missing values for control variables used in analysis, N=255</td></tr>
<tr><td colspan="5">Sample E: individuals in Sample D who have children of preschool age (two to five years), N=197</td></tr>
<tr><td colspan="2">Sample F: Participants in 1st grade randomization, N=47</td><td colspan="2">Sample G: Participants in 2nd grade randomization, N=31</td><td rowspan="2">Sample H: Not participating in any randomization, N=120</td></tr>
<tr><td>Sample I: Accepted for preschool, N=22</td><td>Sample J: Not Accepted for preschool, N=25</td><td>Sample K: Accepted for preschool, N=21</td><td>Sample L: Not Accepted for preschool, N=10</td></tr>
<tr><td colspan="5">Sample M: individuals in Sample E who have 2- to 4-year-old children, N=153</td></tr>
<tr><td colspan="5">Sample N: individuals in Sample M who have non-missing data on distance to (pre-)school, N=110</td></tr>
</table>

Note that one single observation belongs to both Sample F and Sample G such that adding up the number of observations for samples F, G, and H exceeds the number of observations in sample E by one.

should be taken into account (Moore et al. (2008)). Particularly for the studied community, and for Togo as a whole, the quality of preschool is likely to be very low on average, given that, as described in this study, many preschool age children actually end up in the first grade of primary school, where curriculum, equipment and teacher training are most likely not suitable for their needs.

The results regarding the impact of preschool enrollment on labor supply contradicts findings from most earlier studies. However, previous results have been obtained using data from richer countries than Togo. Accordingly, more research regarding the effect of child care arrangements in developing countries is necessary. In this respect, another lesson from this study is that researchers should evaluate programs that affect, for a subpopulation large enough in order to be able to estimate precise effects, the full enrollment of mothers' young children rather than only the enrollment status of single children.

4.7 Appendix: additional tables

Tab. 4.10: Average time use of individuals cohibiting with young children

	Mothers of young children	Grand moth-ers and aunts	Sisters older than 5	Other female relatives	Non-related female	Cohabiting male child older than 5	Cohabiting male adult
				Morning/noon			
Attending school	0.21	0.41	3.38	1.13	1.00	4.23	0.78
	(1.00)	*(1.28)*	*(1.86)*	*(2.06)*	*(1.88)*	*(1.10)*	*(1.75)*
Doing school homework	0.00	0.06	0.02	0.00	0.00	0.02	0.01
	(0.00)	*(0.42)*	*(0.16)*	*(0.00)*	*(0.00)*	*(0.15)*	*(0.16)*
Working as an apprentice	0.24	0.11	0.17	0.75	0.30	0.03	0.11
	(1.05)	*(0.69)*	*(0.95)*	*(1.83)*	*(1.34)*	*(0.36)*	*(0.74)*
Doing household work	1.09	0.57	0.24	0.28	0.30	0.03	0.02
	(1.96)	*(1.52)*	*(0.94)*	*(0.68)*	*(1.34)*	*(0.23)*	*(0.23)*
Working on a field	0.65	0.59	0.13	0.55	0.70	0.05	1.55
	(1.47)	*(1.35)*	*(0.66)*	*(1.70)*	*(1.55)*	*(0.35)*	*(2.23)*
Working in a shop/work shop/etc.	1.20	1.17	0.18	1.00	2.35	0.00	1.28
	(2.05)	*(2.13)*	*(0.95)*	*(2.05)*	*(2.79)*	*(0.00)*	*(2.26)*
Working at home	0.69	0.68	0.09	0.55	0.53	0.02	0.24
	(1.65)	*(1.60)*	*(0.52)*	*(1.70)*	*(1.62)*	*(0.19)*	*(1.01)*
Running errands	0.09	0.13	0.05	0.00	0.00	0.00	0.14
	(0.50)	*(0.71)*	*(0.46)*	*(0.00)*	*(0.00)*	*(0.00)*	*(0.63)*
Doing nothing	0.15	0.65	0.14	0.60	0.00	0.12	0.50
	(0.90)	*(1.91)*	*(0.76)*	*(1.88)*	*(0.00)*	*(0.70)*	*(1.62)*
Other activity	0.16	0.34	0.05	0.10	0.00	0.03	0.28
	(0.84)	*(1.31)*	*(0.49)*	*(0.45)*	*(0.00)*	*(0.25)*	*(1.12)*
Caring for a child	4.91	2.79	0.44	0.80	3.25	0.09	0.25
	(2.80)	*(3.08)*	*(1.44)*	*(2.14)*	*(3.51)*	*(0.56)*	*(1.04)*
Caring for a child while working	1.73	0.93	0.11	0.50	2.00	0.03	0.09
	(2.13)	*(1.77)*	*(0.68)*	*(1.54)*	*(2.69)*	*(0.36)*	*(0.58)*
				Afternoon/evening			
Attending school	0.10	0.15	1.56	0.35	0.48	1.95	0.34
	(0.53)	*(0.52)*	*(0.95)*	*(0.67)*	*(1.07)*	*(0.82)*	*(0.83)*
Doing school homework	0.00	0.01	0.02	0.00	0.00	0.04	0.03
	(0.06)	*(0.09)*	*(0.15)*	*(0.00)*	*(0.00)*	*(0.19)*	*(0.26)*
Working as an apprentice	0.14	0.04	0.08	0.60	0.15	0.01	0.07
	(0.68)	*(0.25)*	*(0.48)*	*(1.57)*	*(0.67)*	*(0.14)*	*(0.55)*
Doing household work	1.19	0.64	0.27	0.40	0.53	0.03	0.04
	(2.05)	*(1.55)*	*(1.01)*	*(0.70)*	*(1.46)*	*(0.17)*	*(0.31)*
Working on a field	0.29	0.33	0.03	0.15	0.00	0.02	0.78
	(0.89)	*(0.94)*	*(0.25)*	*(0.67)*	*(0.00)*	*(0.19)*	*(1.52)*
Working in a shop/work shop/etc.	0.95	0.91	0.17	0.45	1.70	0.02	1.07
	(1.84)	*(1.88)*	*(0.96)*	*(1.39)*	*(2.12)*	*(0.19)*	*(2.07)*
Working at home	0.58	0.62	0.11	0.53	0.57	0.02	0.19
	(1.45)	*(1.44)*	*(0.62)*	*(1.52)*	*(1.18)*	*(0.15)*	*(0.90)*
Running errands	0.12	0.13	0.02	0.00	0.00	0.00	0.19
	(0.52)	*(0.68)*	*(0.17)*	*(0.00)*	*(0.00)*	*(0.00)*	*(0.63)*
Doing nothing	0.23	0.76	0.13	0.70	0.13	0.19	0.65
	(0.96)	*(1.95)*	*(0.70)*	*(2.15)*	*(0.56)*	*(0.91)*	*(1.80)*
Other activity	0.16	0.23	0.05	0.13	0.00	0.05	0.28
	(0.82)	*(1.11)*	*(0.43)*	*(0.56)*	*(0.00)*	*(0.33)*	*(1.06)*
Caring for a child	5.11	2.89	0.54	0.98	3.93	0.17	0.28
	(2.66)	*(3.15)*	*(1.55)*	*(2.16)*	*(3.31)*	*(0.83)*	*(1.06)*
Caring for a child while working	1.36	0.78	0.08	0.05	1.73	0.01	0.06
	(1.88)	*(1.62)*	*(0.48)*	*(0.22)*	*(2.09)*	*(0.14)*	*(0.39)*
Observations	264	116	189	20	20	196	349

Sample: see description for table 4.1 (standard deviations in parentheses).

Tab. 4.11: Average time use of subpupulations of mothers cohabiting with young children

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Participants	Non-participants	Admitted (1st grade)	Not admitted (1st grade)	Youngest child younger than 2	Youngest child at least 2	All young children enrolled	Not all young children enrolled	One young child	More than one young child
					Morning/noon					
Attending school	0.11	0.23	0.23	0.00	0.14	0.18	0.26	0.14	0.22	0.10
	(0.73)	*(1.02)*	*(1.07)*	*(0.00)*	*(0.83)*	*(0.90)*	*(1.12)*	*(0.81)*	*(1.01)*	*(0.72)*
Doing homework	0.00	0.00	0.00	0.00	0.00	0.00	0 00	0.00	0.00	0.00
	(0.00)	*(0.00)*	*(0.00)*	*(0.00)*	*(0.00)*	*(0.00)*	*(0 00)*	*(0.00)*	*(0.00)*	*(0.00)*
Working: apprentice	0.05	0.29	0.11	0.00	0.22	0.28	0 00	0.32	0.19	0.33
	(0.36)	*(1.14)*	*(0.53)*	*(0.00)*	*(1.05)*	*(1.07)*	*(0 00)*	*(1.17)*	*(0.89)*	*(1.21)*
Household work	0.94	1.13	0.80	1.06	1.31	0.89	0.91	1.07	0.91	1.18
	(1.89)	*(1.99)*	*(1.88)*	*(1.93)*	*(2.11)*	*(1.79)*	*(1.88)*	*(1.93)*	*(1.84)*	*(2.00)*
Working on a field	0.61	0.70	0.52	0.68	0.64	0.64	0.63	0.64	0.67	0.61
	(1.33)	*(1.55)*	*(1.35)*	*(1.35)*	*(1.43)*	*(1.50)*	*(1.60)*	*(1.44)*	*(1.56)*	*(1.37)*
Working in a shop	1.62	0.89	1.86	1.40	0.86	1.33	1.62	1.05	1.36	0.95
	(2.13)	*(1.85)*	*(2.25)*	*(2.05)*	*(1.76)*	*(2.18)*	*(2.43)*	*(1.93)*	*(2.18)*	*(1.89)*
Working at home	0.71	0.83	0.59	0.82	0.85	0.81	1.17	0.73	0.92	0.72
	(1.52)	*(1.72)*	*(1.49)*	*(1.57)*	*(1.77)*	*(1.75)*	*(2.34)*	*(1.57)*	*(1.90)*	*(1.60)*
Running errands	0.00	0.14	0.00	0 00	0.02	0.17	0.12	0.11	0.12	0.10
	(0.00)	*(0.50)*	*(0.00)*	*(0 00)*	*(0.18)*	*(0.71)*	*(0.51)*	*(0.60)*	*(0.47)*	*(0.68)*
Doing nothing	0.06	0.08	0.14	0 00	0.06	0.12	0.09	0.10	0.06	0.15
	(0.44)	*(0.47)*	*(0.64)*	*(0 00)*	*(0.40)*	*(0.76)*	*(0.56)*	*(0.67)*	*(0.43)*	*(0.82)*
Other activity	0.23	0.08	0.41	0.08	0.17	0.10	0.18	0.11	0.13	0.13
	(0.96)	*(0.61)*	*(1.33)*	*(0.40)*	*(0.83)*	*(0.71)*	*(0.85)*	*(0.73)*	*(0.80)*	*(0.71)*
Caring for a child	4.82	4.93	4.73	4.90	5.98	4.07	1.88	5.47	4.09	5.46
	(2.87)	*(2.68)*	*(3.13)*	*(2.67)*	*(1.84)*	*(3.04)*	*(2.59)*	*(2.40)*	*(3.06)*	*(2.36)*
Caring + working	1.76	1.71	1.82	1.70	2.17	1.44	0.82	1.92	1.52	1.90
	(1.92)	*(2.13)*	*(2.08)*	*(1.80)*	*(2.18)*	*(1.99)*	*(1.73)*	*(2.11)*	*(2.06)*	*(2.11)*
					Afternoon/evening					
Attending school	0.04	0.10	0.09	0.00	0.06	0.08	0.10	0.07	0.09	0.05
	(0.29)	*(0.47)*	*(0.43)*	*(0.00)*	*(0.33)*	*(0.43)*	*(0.45)*	*(0.39)*	*(0.47)*	*(0.30)*
Doing homework	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	*(0.00)*	*(0.00)*	*(0.00)*	*(0.00)*	*(0.00)*	*(0.00)*	*(0.00)*	*(0.00)*	*(0.00)*
Working: apprentice	0.06	0.14	0.14	0.00	0.15	0.13	0.00	0.17	0.07	0.20
	(0.31)	*(0.61)*	*(0.44)*	*(0.00)*	*(0.69)*	*(0.52)*	*(0.00)*	*(0.65)*	*(0.37)*	*(0.75)*
Household work	1.02	1.20	1.05	1.00	1.37	1.00	1.35	1.08	0.97	1.31
	(2.00)	*(2.02)*	*(2.13)*	*(1.93)*	*(2.07)*	*(1.92)*	*(2.29)*	*(1.90)*	*(1.90)*	*(2.06)*
Working on a field	0.29	0.29	0.27	0.30	0.16	0.35	0.28	0.28	0.39	0.17
	(0.71)	*(0.87)*	*(0.77)*	*(0.68)*	*(0.57)*	*(0.97)*	*(1.01)*	*(0.81)*	*(1.06)*	*(0.53)*
Working in a shop	0.85	0.80	0.68	1.00	0.59	1.06	1.14	0.83	1.08	0.69
	(1.47)	*(1.77)*	*(1.13)*	*(1.73)*	*(1.41)*	*(1.98)*	*(1.95)*	*(1.77)*	*(1.97)*	*(1.60)*
Working at home	0.81	0.70	0.80	0.82	0.68	0.75	1.17	0.61	0.84	0.60
	(1.73)	*(1.54)*	*(1.97)*	*(1.53)*	*(1.46)*	*(1.71)*	*(2.29)*	*(1.39)*	*(1.81)*	*(1.39)*
Running errands	0.18	0.13	0.18	0.18	0.05	0.20	0.24	0.12	0.22	0.07
	(0.71)	*(0.50)*	*(0.85)*	*(0.58)*	*(0.24)*	*(0.68)*	*(0.86)*	*(0.47)*	*(0.70)*	*(0.37)*
Doing nothing	0.06	0.20	0.14	0.00	0.18	0.20	0.09	0.22	0.09	0.30
	(0.44)	*(0.77)*	*(0.64)*	*(0.00)*	*(0.83)*	*(0.83)*	*(0.56)*	*(0.88)*	*(0.45)*	*(1.09)*
Other activity	0.16	0.08	0.18	0.14	0.17	0.08	0.06	0.12	0.08	0.14
	(0.69)	*(0.58)*	*(0.85)*	*(0.53)*	*(0.71)*	*(0.58)*	*(0.40)*	*(0.68)*	*(0.64)*	*(0.62)*
Caring for a child	4.93	5.30	4.89	4.96	5.92	4.65	2.92	5.65	4.70	5.54
	(2.91)	*(2.46)*	*(3.04)*	*(2.85)*	*(1.95)*	*(2.84)*	*(2.78)*	*(2.29)*	*(2.84)*	*(2.31)*
Caring + working	1.35	1.45	1.30	1.40	1.35	1.45	1.18	1.47	1.54	1.27
	(1.68)	*(2.05)*	*(1.72)*	*(1.68)*	*(1.74)*	*(2.04)*	*(1.93)*	*(1.93)*	*(2.09)*	*(1.75)*
Observations	47	120	22	25	71	126	39	158	101	96

Samples: Results in column 1 were computed based on sample F, those in column 2 based on sample H, those in column 3 basee on sample I, and those in column 4 based on sample J. For each of the remaining pairs of columns (5/6, 7/8, and 9/10), calculations are based on sample E, where the sample is split into two subsamples (one subsample corresponding to one column) based on the respective characteristic indicated in the column head. For more details, see table 4.9 in the appendix (standard deviations in parentheses).

Tab. 4.12: Average time use of subpupulations mothers cohibiting with young children: different occupations

	Agriculture	Alimentary commerce	Other small commerce	Sewery	No occupation
			Morning/noon		
Attending school	0.00 *(0.00)*	0.06 *(0.38)*	0.00 *(0.00)*	0.00 *(0.00)*	0.42 *(1.40)*
Doing school homework	0.00 *(0.00)*	0.00 *(0.00)*	0.00 *(0.00)*	0.00 *(0.00)*	0.00 *(0.00)*
Working as an apprentice	0.14 *(0.75)*	0.00 *(0.00)*	0.00 *(0.00)*	0.26 *(1.15)*	1.08 *(1.98)*
Doing household work	1.04 *(1.78)*	0.97 *(1.95)*	1.07 *(1.95)*	0.42 *(1.43)*	1.89 *(2.41)*
Working on a field	1.82 *(2.05)*	0.08 *(0.53)*	0.24 *(0.79)*	0.00 *(0.00)*	0.51 *(1.38)*
Working in a shop/work shop/etc.	0.10 *(0.50)*	1.86 *(2.35)*	1.71 *(2.14)*	1.92 *(2.19)*	0.14 *(0.83)*
Working at home	0.51 *(1.33)*	1.26 *(2.12)*	0.81 *(1.76)*	1.21 *(2.26)*	0.57 *(1.59)*
Running errands	0.10 *(0.41)*	0.20 *(0.66)*	0.05 *(0.28)*	0.32 *(1.38)*	0.03 *(0.17)*
Doing nothing	0.20 *(1.07)*	0.06 *(0.38)*	0.16 *(0.61)*	0.18 *(0.80)*	0 00 *(0 00)*
Other activity	0.23 *(1.12)*	0.20 *(0.95)*	0.10 *(0.56)*	0.11 *(0.46)*	0 00 *(0 00)*
Caring for a child	5.20 *(2.54)*	4.94 *(2.86)*	4.88 *(2.89)*	3.66 *(3.09)*	4.28 *(2.81)*
Caring for a child while working	1.61 *(1.85)*	2.06 *(2.14)*	1.62 *(2.11)*	1.53 *(1.86)*	1.01 *(1.95)*
			Afternoon/evening		
Attending school	0.00 *(0.00)*	0.00 *(0.00)*	0.03 *(0.19)*	0.00 *(0.00)*	0.21 *(0.71)*
Doing school homework	0.00 *(0.00)*	0.00 *(0.00)*	0.00 *(0.00)*	0.00 *(0.00)*	0.00 *(0.00)*
Working as an apprentice	0.09 *(0.48)*	0.00 *(0.00)*	0.00 *(0.00)*	0.11 *(0.46)*	0.57 *(1.12)*
Doing household work	1.26 *(1.73)*	1.30 *(2.26)*	1.29 *(2.18)*	0.55 *(1.30)*	1.54 *(2.41)*
Working on a field	0.81 *(1.36)*	0.14 *(0.64)*	0.03 *(0.19)*	0.00 *(0.00)*	0.18 *(0.50)*
Working in a shop/work shop/etc.	0.06 *(0.30)*	1.16 *(1.84)*	1.29 *(2.12)*	1.18 *(1.33)*	0.28 *(1.26)*
Working at home	0.50 *(1.20)*	1.22 *(2.19)*	0.93 *(1.82)*	0.97 *(1.98)*	0.28 *(0.91)*
Running errands	0.16 *(0.54)*	0.25 *(0.87)*	0.07 *(0.26)*	0.18 *(0.69)*	0.04 *(0.18)*
Doing nothing	0.21 *(1.07)*	0.09 *(0.43)*	0.14 *(0.58)*	0.34 *(1.03)*	0.31 *(1.09)*
Other activity	0.26 *(1.05)*	0.09 *(0.60)*	0.05 *(0.28)*	0.18 *(0.61)*	0.00 *(0.00)*
Caring for a child	5.06 *(2.65)*	5.52 *(2.44)*	5.05 *(2.62)*	4.32 *(3.10)*	4.78 *(2.66)*
Caring for a child while working	0.95 *(1.41)*	1.83 *(2.09)*	1.47 *(2)*	1.29 *(1.75)*	0.76 *(1.50)*
Observations	48	44	29	19	36

Sample: Calculations are based on sample E (see table 4.9 in the appendix for details). The means desplayed in each column refer to a subsample defined by the occupation indicated in the column head. 18 observations from sample E are not taken into account because they are grouped into occupational categories of 5 observations and less (standard deviations in parentheses).

Tab. 4.13: Means and standard deviation of variables used throughout the analysis for different subsamples

	Sample					
	E	F	I	J	F'	H
All children younger than six enrolled	0.20	0.23	0.32	0.16	0.22	0.14
Hours of care	9.87	9.74	9.61	9.86	10.05	10.23
	(5.28)	*(5.64)*	*(6.13)*	*(5.28)*	*(5.44)*	*(4.98)*
Hours of work	5.13	5.40	5.48	5.34	5.52	4.75
	(3.92)	*(3.53)*	*(3.56)*	*(3.57)*	*(3.64)*	*(3.71)*
Hours of overlap between care and work	5.38	5.03	5.34	4.76	5.27	5.60
	(4.21)	*(4.08)*	*(4.23)*	*(4.01)*	*(4.10)*	*(4.23)*
Number of 0- to 1-year-old children	0.33	0.40	0.45	0.36	0.39	0.33
	(0.47)	*(0.50)*	*(0.51)*	*(0.49)*	*(0.49)*	*(0.47)*
Number of 2- to 5-year-old children	1.17	1.15	1.09	1.20	1.15	1.14
	(0.45)	*(0.55)*	*(0.29)*	*(0.71)*	*(0.57)*	*(0.37)*
Age	30.33	30.47	29.68	31.16	31.15	30.1
	(7.34)	*(6.96)*	*(5.63)*	*(7.99)*	*(7.11)*	*(7.18)*
Is wife of household head	0.75	0.79	0.77	0.80	0.80	0.72
Number of children ever gave birth to	3.81	4.04	3.95	4.12	4.22	3.84
	(2.16)	*(2.36)*	*(2.34)*	*(2.42)*	*(2.42)*	*(2.20)*
Ever went to school	0.82	0.81	0.86	0.76	0.78	0.84
Protestant	0.14	0.17	0.18	0.16	0.20	0.12
Muslim	0.21	0.28	0.23	0.32	0.30	0.19
Number of adults in household	3.65	3.21	3.68	2.80	3.17	3.94
	(2.52)	*(1.83)*	*(2.03)*	*(1.55)*	*(1.80)*	*(2.87)*
Number of adult cohabitants working on farm	0.65	0.66	0.82	0.52	0.61	0.66
	(0.74)	*(0.73)*	*(0.80)*	*(0.65)*	*(0.70)*	*(0.78)*
Number of adult coh. working in sales shop	0.24	0.19	0.27	0.12	0.20	0.27
	(0.55)	*(0.54)*	*(0.70)*	*(0.33)*	*(0.56)*	*(0.55)*
Number of adult coh. without occupation	0.26	0.19	0.27	0.12	0.20	0.32
	(0.66)	*(0.45)*	*(0.55)*	*(0.33)*	*(0.46)*	*(0.76)*
Total of adult cohabitants' salaries (Thousand)	12.22	10.36	9.02	11.53	11.26	13.88
	(23.73)	*(18.91)*	*(13.65)*	*(22.79)*	*(20.40)*	*(27.09)*
Total value of adult cohabitants' farm output (Th.)	344.41	337.00	404.55	277.56	330.02	344.72
	(819.10)	*(648.55)*	*(645.82)*	*(658.31)*	*(654.38)*	*(805.90)*
Number of rooms of dwelling	3.21	3.36	3.95	2.84	3.34	3.26
	(2.48)	*(2.64)*	*(3.36)*	*(1.70)*	*(2.76)*	*(2.59)*
Dwelling is owned by a household member	0.51	0.43	0.45	0.40	0.44	0.53
Av. cogn. test score of children (3-5) in household	-0.02	-0.02	-0.02	-0.03	-0.02	-0.02
	(0.16)	*(0.16)*	*(0.17)*	*(0.16)*	*(0.17)*	*(0.15)*
Average cognitive test score of adults in household	6.11	6.33	6.96	5.70	6.40	6.14
	(2.33)	*(2.35)*	*(2.24)*	*(2.34)*	*(2.37)*	*(2.35)*
Number of observations	197	47	22	25	40	120

Sample: the samples used to compute the means vary by column, and column heads indicate the respective sample used. For definitions of these samples see table 4.9. Sample F' includes all observations from sample F for which dependent variables from 2008 are not missing (i.e. the sample used for the estimations for which table 4.7 displays the results). For the two rows of means for cognitive test scores, sample sizes are smaller, because there are some missing values for test scores, particularly for young children. The respective sample sizes for child test score are (in the order of the table's columns): 121, 24, 13, 11, 22, and 76. For adult test scores, sample sizes are: 162, 40, 20, 20, 33, and 98 (standard deviations in parentheses).

Tab. 4.14: Opinions regarding ability to work/child development conditional on different child care arrangements. Relative frequencies of responses to survey questions.

Response: Child must be...	Age of child when mother can start working if child is cared for by mother	Age of child when mother can start working if child is cared for someone else	Age of child when no longer harmful if it is cared for by someone else
1 year old	23.91	14.67	20.11
2 years old	17.93	10.87	20.65
3 years old	14.67	33.70	23.37
4 years old	14.13	19.57	13.04
5 years old	17.39	14.13	14.67
6 years old	6.52	3.80	3.80
at least 7 years old	5.43	3.26	4.35

Sample: Calculations are based on all observations in sample E (see table 4.9 in the appendix for details) for which responses to all three survey questions considered here are non-missing (N=184).

Tab. 4.15: Impact of having a child accepted for first grade of preschool on time use of mothers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hours of care	-0.25	-0.86	-0.91	-0.91	-0.92	-0.62	-1.25
	(1.67)	*(1.39)*	*(1.52)*	*(1.48)*	*(1.57)*	*(1.39)*	*(1.52)*
Hours of work	0.14	0.18	0.06	0.12	-0.20	0.07	-0.09
	(1.04)	*(1.06)*	*(1.15)*	*(1.15)*	*(1.23)*	*(1.17)*	*(1.18)*
Hours of overlap	0.58	0.17	0.18	0.10	0.16	0.28	0.10
	(1.20)	*(1.03)*	*(1.12)*	*(1.12)*	*(1.21)*	*(1.14)*	*(1.16)*

Sample: Observations from Sample F (see table 4.9 in the appendix for details; N=47). *Specifications:* Model 1 gives the raw effect without additional controls. Model 2 adds the number of children younger than two. Models 3 through 7 all add at least the following control variables: a mother's number of children in three different age groups (0 through 1 year, 2 through 5 years, and 6 through 12 years), the number of adults per household, age of the mother, dummy variables for the three most common religious denominations, and the number of adult cohabitants who work in agriculture. In addition, models 4 through 7 include further groups of control variables in a non-nested manner (i.e. the additional controls from model 4 are not included in either of models 5 through 7, etc.). Model 4 adds an indicator for whether the mother is the household head's wife, the total number of children she has ever given birth to, and whether she ever went to school. Model 5 adds the number of adult cohabitants who own shops, the number of adult cohabitants who work as drivers, and the number of cohabiting adults who have no occupation. Model 6 adds the number of rooms per household and a dummy variable indicating whether the household's dwelling is owned by a household member. Model 7 adds the total of salaries of cohabiting adults and the total of agricultural output produced by cohabiting adults Standard errors given in parentheses..

Tab. 4.16: Impact of having a child accepted for second grade of preschool on time use of mothers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Full enrollment	-0.21	-0.29*	-0.02	-0.02	0.16	-0.08	-0.03
	(0.19)	*(0.17)*	*(0.21)*	*(0.21)*	*(0.28)*	*(0.20)*	*(0.22)*
Hours of care	4.22**	5.05**	4.38*	4.22*	1.89	4.92*	4.25
	(2.13)	*(2.04)*	*(2.56)*	*(2.39)*	*(3.46)*	*(2.60)*	*(2.78)*
Hours of work	-0.08	-0.27	-1.48	-1.20	2.95	-1.98	-1.30
	(1.96)	*(2.02)*	*(2.73)*	*(2.87)*	*(3.41)*	*(2.83)*	*(2.94)*
Hours of overlap	3.25**	3.50**	4.80**	4.56**	3.80	5.14**	4.77**
	(1.58)	*(1.61)*	*(2.05)*	*(1.87)*	*(2.77)*	*(2.10)*	*(2.19)*

Sample: Observations from Sample G (see table 4.9 in the appendix for details; N=31). *Specifications:* see explanations for table 4.15. Standard errors given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Tab. 4.17: Instrumental variable estimates using admission to first grade of preschool as an instrument, women observed in both 2008 and 2011

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First stage effect of instrument	0.30**	0.31***	0.33***	0.34***	0.30**	0.31***	0.32***
	(0.13)	*(0.11)*	*(0.11)*	*(0.12)*	*(0.12)*	*(0.11)*	*(0.12)*
First stage F-statistic	5.48	7.16	8.57	8.11	6.20	7.78	7.80
IV-estimate: hours of care	-3.59	-3.95	-3.86	-2.00	-3.19	-2.50	-3.94
	(5.17)	*(4.53)*	*(4.61)*	*(4.24)*	*(5.13)*	*(4.68)*	*(4.51)*
IV-estimate: hours of work	-0.13	-0.08	-1.79	-1.30	-2.65	-1.79	-1.87
	(3.84)	*(3.71)*	*(3.88)*	*(3.63)*	*(4.43)*	*(4.15)*	*(3.91)*
IV-estimate: hours of overlap	0.44	0.80	0.52	1.43	0.51	0.84	0.25
	(3.69)	*(4.44)*	*(3.73)*	*(3.58)*	*(4.21)*	*(4.00)*	*(3.75)*

Sample: Observations from Sample F (see table 4.9 in the appendix for details) for women who appear in both the 2008 and the 2011 household data and for whom data is not missing (N=40; the same sample has been used for the computation of means displayed in the upper panel of table 4.5). *Specifications:* see explanations for table 4.15. Standard errors given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Tab. 4.18: Households for which the preschool is the closest institution for young children vs. other housholds: difference in differences

	Dependent variable: all young children enrolled	
	No controls	Controls
Preschool is closest · second wave	0.21	0.15
	(0.18)	*(0.17)*
Preschool is closest	-0.09	0.06
	(0.13)	*(0.12)*
Second wave	-0.04	0.09
	(0.06)	*(0.07)*

Sample: all observations on women in sample N (see table 4.9 in the appendix for details) who are also observed in 2008, and for whom all other variables used for the analysis for both years are not missing. The final sample includes 81 women, i.e. 162 observations. Standard errors given in parentheses.

Tab. 4.19: Reasons indicated for not sending young children to preschool

Don't know	5
Preferred to send child to primary school	12
Preferred to have child at home	0
Preschool too far away	17
Quality of preschool is uncertain	1
Preschool is too expensive	0
Child is still too young	9
Neighboring child was not admitted	1
Child was not admitted	5
Other reason	18
Number of children	65

Sample: households with children who were admitted to preschool but who were not enrolled in preschool. Multiple responses were allowed

Tab. 4.20: Opinions and the likelihood of signing up a child for preschool

	1st grade, 3-year-olds			2nd grade, 4-year-olds		
	N	(1)	(2)	N	(1)	(2)
Child care arrangements, mother's work, and child development						
Age of child when mother can start working if child is cared for by mother	39	-0.11**	-0.08	33	0.07	0.09
		(0.04)	*(0.05)*		*(0.06)*	*(0.07)*
Age of child when mother can start working if ch. is cared for someone else	39	-0.11**	-0.07	32	0.02	0.04
		(0.05)	*(0.06)*		*(0.06)*	*(0.08)*
Age of child when no longer harmful if it is cared for by someone else	39	-0.04	-0.03	33	-0.05	-0.03
		(0.06)	*(0.07)*		*(0.07)*	*(0.09)*
Young children are harmed when cared for by someone else	39	-0.05	0.01	33	0.06	-0.01
		(0.06)	*(0.08)*		*(0.06)*	*(0.10)*
Could easily find care for young child	39	0.01	0.02	33	-0.02	-0.03
		(0.06)	*(0.06)*		*(0.06)*	*(0.09)*
Whether women/mothers should work						
Want to spend as much time with young child as possible	39	0.06	0.00	33	0.08	0.03
		(0.08)	*(0.11)*		*(0.07)*	*(0.09)*
Women should not work outside household	39	0.08	0.05	33	0.01	0.05
		(0.06)	*(0.08)*		*(0.08)*	*(0.11)*
Mothers of young children should not work outside household	39	0.08	0.04	33	0.04	0.06
		(0.06)	*(0.08)*		*(0.07)*	*(0.09)*
Importance of education						
Our children must attend school	39	-0.29	-0.39	33	0.11	-0.15
		(0.27)	*(0.32)*		*(0.37)*	*(0.50)*
Sometimes family needs are more important than education for children	39	-0.01	0.04	33	0.11*	0.21***
		(0.07)	*(0.07)*		*(0.06)*	*(0.08)*
Children should attend preschool	39	-0.27*	-0.34*	33	0.14	0.22
		(0.16)	*(0.18)*		*(0.15)*	*(0.22)*
Labor force attachment						
Need to fulfill my needs by means of work	39	-0.33*	-0.35*	33	0.30*	0.31
		(0.17)	*(0.20)*		*(0.18)*	*(0.23)*
Like working	39	-0.16	-0.14	33	-0.07	-0.10
		(0.16)	*(0.21)*		*(0.23)*	*(0.30)*
Would want to work more if I had the chance	39	-0.34**	-0.38**	33	-0.07	-0.66
		(0.16)	*(0.18)*		*(0.31)*	*(0.62)*
Knowledge of NGO/preschool project						
Have heard of NGO	40	0.19	0.22	33	-0.48**	-0.68*
		(0.18)	*(0.25)*		*(0.23)*	*(0.35)*
Number of NGO's projects known	40	0.06	0.12	33	-0.08	-0.10
		(0.06)	*(0.08)*		*(0.06)*	*(0.08)*
Gave correct preschool fee	37	0.57***	0.54***	30	0.33*	0.35
		(0.14)	*(0.16)*		*(0.17)*	*(0.22)*
Trust						
Can generally confide in people	39	-0.02	0.03	32	0.14	0.31
		(0.17)	*(0.19)*		*(0.19)*	*(0.28)*
People take advantage of me	39	0.02	-0.02	32	-0.01	0.26**
		(0.06)	*(0.08)*		*(0.07)*	*(0.11)*
Children can marry anyone	39	0.02	0.05	32	-0.10	0.14
		(0.17)	*(0.23)*		*(0.18)*	*(0.24)*
Children should not marry Christian	39	-0.21	0.02	32	-0.05	-0.40
		(0.23)	*(0.38)*		*(0.30)*	*(0.49)*
Children should not marry someone of different relig. denomination	39	0.10	0.01	32	-0.05	-0.18
		(0.25)	*(0.30)*		*(0.30)*	*(0.48)*
Children should not marry Muslim	39	-0.02	-0.15	32	0.07	-0.10
		(0.23)	*(0.26)*		*(0.21)*	*(0.29)*

Sample: two subgroups out of sample M (see table 4.9) which are not mutually exclusive - women with at least one 3-year-old child for the left half of the table, and women with at least one 4-year-old child for the right half. Since woman would often have a missing value for one response and non-missing values for all others, I chose not to construct one sample consistently used for all computations. Instead, the sample used for each row depends on the sample of women for whom one particular opinion measure is not missing; resulting sample sizes are indicated. Columns labeled with (1) display coefficient estimates from regressing an indicator for whether a woman signed up a child for admission to preschool on the respective opinion measure and a constant. For the results in columns labeled with (2), the following control variables have been added: a mother's number of children in three different age groups (0-1 year, 2-5 years, and 6-12 years), a dummy variable for whether she is muslim, the numbers of adult cohabitants who work in agriculture, own shops, and have no occupation, the number of rooms per household, a dummy variable indicating whether the household's dwelling is owned by a household member, an indicator for whether the mother is the household head's wife, whether she ever went to school, the total of salaries of cohabiting adults and the total of agricultural output produced by cohabiting adults. Standard errors given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Tab. 4.21: Correlation between opinions and time use of mothers of 2 to 5-year-olds: are opinions relevant to outcomes?

	N	Hours of care (1)	Hours of care (2)	Working hours (1)	Working hours (2)
Child care arrangements, mother's work, and child development					
Age of child when mother can start working if child is cared for by mother	191	-0.47** (0.21)	-0.49** (0.21)	-0.28* (0.15)	-0.29* (0.16)
Age of child when mother can start working if ch. is cared for someone else	190	-0.04 (0.26)	0.01 (0.26)	0.20 (0.19)	0.12 (0.20)
Age of child when no longer harmful if it is cared for by someone else	185	0.13 (0.23)	0.23 (0.24)	-0.12 (0.18)	-0.18 (0.19)
Young children are harmed when cared for by someone else	191	0.05 (0.31)	0.17 (0.32)	0.06 (0.23)	0.03 (0.25)
Could easily find care for young child	191	-0.01 (0.27)	0.02 (0.28)	-0.32 (0.20)	-0.30 (0.22)
Whether women/mothers should work					
Want to spend as much time with young child as possible	191	-0.66** (0.32)	-0.61* (0.33)	0.33 (0.24)	0.34 (0.25)
Women should not work outside household	191	0.03 (0.29)	0.16 (0.30)	0.23 (0.22)	0.24 (0.23)
Mothers of young children should not work outside household	191	-0.29 (0.30)	-0.31 (0.30)	-0.15 (0.22)	-0.15 (0.23)
Importance of education					
Our children must attend school	190	0.61 (1.30)	0.72 (1.30)	-0.50 (0.96)	-0.71 (1.00)
Sometimes family needs are more important than education for children	191	-0.58* (0.32)	-0.61* (0.32)	-0.01 (0.24)	-0.01 (0.25)
Children should attend preschool	191	1.05* (0.57)	1.07* (0.57)	-0.66 (0.42)	-0.65 (0.44)
Labor force attachment					
Need to fulfill my needs by means of work	191	-0.95 (0.61)	-1.16* (0.61)	-0.63 (0.45)	-0.68 (0.47)
Like working	191	-0.61 (0.72)	-0.51 (0.73)	-0.13 (0.53)	-0.21 (0.56)
Would want to work more if I had the chance	191	-0.76 (0.56)	-0.97 (0.62)	-0.60 (0.42)	-0.54 (0.48)
Knowledge of NGO/preschool project					
Have heard of NGO	191	-1.95** (0.83)	-1.79** (0.90)	1.69*** (0.61)	1.82*** (0.69)
Number of NGO's projects known	191	-0.27 (0.28)	-0.14 (0.30)	0.53*** (0.21)	0.53** (0.23)
Gave correct preschool fee	175	-1.85** (0.83)	-1.71** (0.85)	-0.04 (0.62)	-0.26 (0.66)
Trust					
Can generally confide in people	186	2.29*** (0.83)	2.28*** (0.84)	-0.04 (0.63)	-0.08 (0.66)
People take advantage of me	182	0.48 (0.29)	0.33 (0.31)	-0.14 (0.22)	-0.12 (0.24)
Children can marry anyone	184	-0.50 (0.80)	-0.49 (0.85)	-0.16 (0.59)	-0.21 (0.65)
Children should not marry Christian	184	1.04 (1.23)	0.40 (1.53)	-0.21 (0.91)	-0.35 (1.17)
Children should not marry someone of different relig. denomination	184	1.11 (1.28)	0.86 (1.34)	0.13 (0.95)	0.20 (1.03)
Children should not marry Muslim	184	-0.81 (0.95)	-0.67 (0.99)	0.63 (0.70)	0.84 (0.76)

Sample: observations include women in sample E (see table 4.9 in the appendix for details). For further explanations regarding sample choice and specifications, see the explenations given in table 4.20. Standard errors given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Tab. 4.22: Correlation between opinions and likelihood of full enrollment for mothers of 2 to 5-year-olds

	N	(1)	(2)
Child care arrangements, mother's work, and child development			
Age of child when mother can start working if child is cared for by mother	191	-0.02	-0.01
		(0.02)	*(0.02)*
Age of child when mother can start working if ch. is cared for someone else	190	0.01	0.00
		(0.02)	*(0.02)*
Age of child when no longer harmful if it is cared for by someone else	185	-0.02	-0.04**
		(0.02)	*(0.02)*
Young children are harmed when cared for by someone else	191	0.01	-0.01
		(0.02)	*(0.02)*
Could easily find care for young child	191	0.00	-0.01
		(0.02)	*(0.02)*
Whether women/mothers should work			
Want to spend as much time with young child as possible	191	0.01	0.01
		(0.02)	*(0.02)*
Women should not work outside household	191	0.04*	0.02
		(0.02)	*(0.02)*
Mothers of young children should not work outside household	191	0.06***	0.06***
		(0.02)	*(0.02)*
Importance of education			
Our children must attend school	190	-0.07	-0.07
		(0.10)	*(0.09)*
Sometimes family needs are more important than education for children	191	0.06**	0.05**
		(0.02)	*(0.02)*
Children should attend preschool	191	-0.04	-0.04
		(0.04)	*(0.04)*
Labor force attachment			
Need to fulfill my needs by means of work	191	-0.04	-0.03
		(0.05)	*(0.04)*
Like working	191	0.00	-0.02
		(0.05)	*(0.05)*
Would want to work more if I had the chance	191	-0.05	-0.03
		(0.04)	*(0.04)*
Knowledge of NGO/preschool project			
Have heard of NGO	191	0.11*	0.08
		(0.06)	*(0.07)*
Number of NGO's projects known	191	0.06***	0.04**
		(0.02)	*(0.02)*
Gave correct preschool fee	175	0.27***	0.26***
		(0.06)	*(0.06)*
Trust			
Can generally confide in people	186	-0.02	-0.01
		(0.07)	*(0.06)*
People take advantage of me	182	0.01	0.03
		(0.02)	*(0.02)*
Children can marry anyone	184	0.07	0.07
		(0.06)	*(0.06)*
Children should not marry Christian	184	-0.18*	-0.09
		(0.09)	*(0.11)*
Children should not marry someone of different relig. denomination	184	-0.05	0.03
		(0.10)	*(0.10)*
Children should not marry Muslim	184	0.06	0.01
		(0.07)	*(0.07)*

Sample: observations include women in sample E (see table 4.9 in the appendix for details). For further explanations regarding sample choice and specifications, see the explenations given in table 4.20. Standard errors given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Chapter 5

Grade retention and peer effects[1]

5.1 Introduction

This paper investigates how grade repetition[2] causes spillovers, or peer effects, in educational production. Economists have long been interested in peer effects in the context of education, because of their potentially important implications for policy; regarding, for instance, the potential efficiency gains from reallocating students by reorganizing classroom composition. The bulk of evidence on peer effects in education has been obtained by studying data from industrialized countries. The present study, instead, uses administrative student data from four primary schools in one Togolese community. It focuses on grade retention status as the relevant peer characteristic, which has, with the exception of Lavy et al. (2008), not yet been studied in the context of peer effects. Identification relies on variation in the share of repeating classmates within schools while using a value added approach, i.e. controlling for earlier test scores. I argue that, given the primarily merit-based retention policy in the studied schools, the estimated models appropriately account for selection into classes. The identifying variation in the share of repeaters per class comes from, essentially, comparing different cohorts of students within schools. Togolese primary schools provide a good setting for investigating the issue, because class repetitions are very common, and, in addition, the Togolese school system can be considered as a typical example of education systems in Francophone

[1]Part of this research was supported by the German Research Foundation (DFG) within the project 'Ein Paneldatensatz zur Analyse von Humankapitalinvestitionen im frühen Alter in ländlichen Regionen von Entwicklungsländern', and by the Leibniz Universität Hannover ("Forschungsfond").

[2]The terms grade repetition and retention are used interchangeably throughout this chapter.

sub-Saharan Africa, which are all strongly and homogenously influenced by the French colonial administration (Frölich and Michaelowa (2005)).

In many countries (such as the US, see Jacob and Lefgren (2004) and Eide and Showalter (2001)), requiring students to repeat a grade if they fail to meet specific achievement standards, is a widespread policy. Grade repetition has been seen as a means to help low achieving students improve their learning by giving them more time to acquire the skills associated with their current grade. In addition, extremely high repetition rates in many developing countries have long been recognized as potentially indicating a waste of resources, since letting a student repeat a grade requires roughly the same resources as enrolling an additional child for one year (Gomes-Neto and Hanushek (1994), Mahjoub (2008), Manacorda (2008), Glick and Sahn (2010)). Regarding the prevalence of grade repetition, the studied community is not an exception. The average share of repeaters per class in the data used for the analysis equals 22.7 percent, a figure that is very similar to those found for other developing countries, particularly in sub-Saharan Africa.

Regarding the evaluation of the impact grade retention policies, studies have focused on assessing whether repeating a class enhances learning for affected students, yielding mixed results. Gomes-Neto and Hanushek (1994) found that having repeated a grade is associated with enhanced learning for Brazilian primary school students. However, Jacob and Lefgren (2004) summarize results from correlational studies to indicate that repeaters are negatively affected through decreased self-esteem, lower achievement and higher dropout rates. These studies may not take into account that repeaters are a selective sample who tend to be low achieving students before repetition.

For a few quasi-experimental studies, which do attempt to account for the fact that repeaters tend to be low-achievers, the direction of the effect of grade repetition on achievement of affected students depends on whether these studies use data from rich or from poor countries. As regards industrialized countries, Jacob and Lefgren (2004) evaluate grade repetitions in Chicago Public Schools by exploiting the fact that the retention decision is an (almost) discontinuous function of prior test scores in order to implement a regression discontinuity design. Mahjoub (2008) employs an instrumental variable strategy. He uses quarter of birth as an instrument, arguing that relatively old students are promoted more often. For French high school students, he finds a positive effect of repetition. Likewise, Eide and Showalter (2001) argue that students who are relatively young compared to their classmates repeat more often, and their instrumental variable is given by a child's school entry age relative to the statutorily determined kindergarten entry date which varies across US states. They find negative but statistically insignificant effects of grade repetition on the likelihood of dropping out of high school, and positive but statistically insignificant effects on later earnings.

For developing countries, comparable studies tend to find opposing results. Manacorda (2008) exploits a discontinuity in retention probability induced by a minimum attendance requirement in Uruguayan junior high schools to implement a regression discontinuity design. He finds a negative effect of grade retention on educational attainment, i.e. students complete fewer grades if they have repeated a grade. Glick and Sahn (2010) obtained a similar result for students in Senegal. Exploiting variation across schools in test score thresholds for promotion they find that repeating second grade reduces the likelihood of completing primary school.

Thus, while there exists some (albeit inconclusive) evidence regarding the impact of grade repetition on repeaters, Jacob and Lefgren (2004) explicitly point out that a full evaluation of the costs and benefits of grade retention would require to take into account spillover effects, which is the focus of this study. Regarding the direction of peer effects caused by grade retention, it may depend on whether grade repetition actually enhances learning of repeaters or not - more knowledgable peers may positively affect classmates, and less knowledgable may negatively affect classmates. Thus, the fact that studies from developing countries mentioned above tend not to find beneficial effects of grade repetition on repeating individuals leads to suspect negative peer effects as well. Moreover, even if learning effects for affected students were not negative, it has been argued that grade repetition may have psychological effects such as lower self esteem (Gomes-Neto and Hanushek (1994)) which could worsen classroom behavior and thereby negatively affect educational achievement of classmates.

In order to avoid simultaneity bias arising from including contemporaneous outcomes in the empirical model, studies of peer effects usually focus on peer group characteristics that have been determined before the formation of peer groups as the explanatory variable of interest. The respective coefficient is then interpreted as a reduced form peer effect associated with the group characteristic, which is a mixture of both a direct effect of the characteristic on peers, and an indirect effect working through changes in behavior during the current period of observation caused by the respective characteristic.[3] Accordingly, studies of peer effects focus on a great variety of group characteristics which all have in common that, chronologically, they have been determined before the formation of peer groups. Lavy and Schlosser (2011) investigate the impact of peer group gender composition; Hoxby (2000) also looks at gender composition, and at racial composition of student cohorts. Sacerdote (2001) and Carrell et al. (2009) use SAT scores as a measure for background characteristics of peers in college.[4] The present study also follows this reduced form approach by focusing on the retention status of peers, a variable that is determined during the previous school year.

Even if a study of peer effects does not attempt to measure peer effects of contemporaneous group behavior, the identification of peer effects is still complicated due to usually non-random peer group formation. The strongest concerns regarding endogeneity due to selection into peer groups concern school choice.[5] Accordingly, a large part of the literature chooses to control for school fixed effects, arguing that, given the respective institutional background, variation in the peer measure within schools can be considered exogenous[6]. Lavy and Schlosser (2011), for instance, compare cohorts within schools. Ammermueller and Pischke (2009) rely an variation in class composi-

[3]See Lyle (2007) for one of the few recent attempts to separate between contemporaneous (or "endogenous") peer effects and "exogenous" peer effects (i.e. the spillovers caused by preexisting behavior and attributes of peers net of contemporaneous changes in behavior) within a randomized research design.

[4]Other examples include Ding and Lehrer (2007) who use exam results prior to assignment to schools as the relevant peer characteristic. Ammermueller and Pischke (2009) use the number of books in student's household as a proxy for peer quality. Neidell and Waldfogel (2010) look at the spillovers during Kindergarten resulting from additional peers having attended preschool prior to Kindergarten. Angrist and Lang (2004) evaluate peer effects by exploiting variation in the racial composition of classes induced by Boston's Metco Program.

[5]This is what would be called correlated effects in Manski's terms (Manski (1993) as cited in most of the studies discussed in this section).

[6]Other papers have addressed the issue of selection into peer groups by exploiting scenarios in which individuals are randomly assigned to their peer groups. Carrell et al. (2009) consider random assignment of US Air Force Academy students to squadrons. Sacerdote (2001) exploits random assignment of Freshmen at Dartmouth college to roommates and dorms. These studies tend to find positive effects of high peer quality. Effect sizes in Carrell et al. (2009) are larger than in Sacerdote (2001), which the authors attribute to the fact that their peer variable is measured at the level of a larger group than in other studies (which focus on roommates, for instance). Studying randomly formed groups within the US Military Academy, Lyle (2007) finds significant peer effects on subsequent decisions such as choice of academic major, but not for academic performance. A few studies rely on a large set of control variables that are argued to sufficiently capture student, teacher, and school characteristics in order to adequately take into account the process of selection into schools and classes (Ding and Lehrer (2007), Frölich and Michaelowa (2005))

tion within European primary schools, comparing classrooms of the same grade for the same cohort of students; in their study, the introduction of school fixed effects reduces the size of estimated peer effects. Other studies that rely on including school fixed effects for identification of peer effects include Hoxby (2000), Neidell and Waldfogel (2010), and Lavy et al. (2008). In my study, variation in class level variables within schools can only result from variation across cohorts, either by comparing students in the same grade between different cohorts, or by comparing different cohorts (grades) observed in the same calendar year.

The estimates presented in this chapter consistently show that test scores are significantly negatively affected by increases in the share of repeaters. A one standard deviation increase in the share of repeaters per class is estimated to reduce individual test scores by about 13 percent of a standard deviation. Estimates are robust to changes in model specification associated with different sources of variation in the share of repeaters. The effect is heterogenous across subjects taught, and it is particularly pronounced for mental arithmetic and math word problems. I also show that the estimates can be interpreted as showing the impact of actual changes in class composition rather than changes in class size.

The study proceeds as follows: Section 5.2 discusses institutional background, potential determinants of the likelihood of grade retention, and arguments regarding the direction of a potential peer effect associated with grade repetition. Section 5.3 describes the empirical strategy and the models estimated as well as the dataset used for the analysis. Section 5.4 presents results, and robustness checks and analysis regarding heterogenous effects across subjects are discussed in section 5.5. Section 5.6 concludes.

5.2 Background

The direction of peer effects caused by grade repetition can be expected to depend on how repeaters respond to the fact that they are retained, as well as on predetermined characteristics of repeaters, who are, presumably, low achievers Lavy et al. (2008). Potential mechanisms may include both cognitive and behavioral aspects. As far as learning is actually enhanced through grade repetition for affected students, spillover effects may also be positive. More knowledgable peers are assumed to be beneficial for a student's learning because his motivation increases, he has to compete with betters students, or because his peers share knowledge with him.[7] However, in turn, if the causal effect of repetition on repeating students actually is negative, resulting externalities may also consist of lower achievement of other students in the same class. Manacorda (2008) claims that repeaters may do worse in school because they are faced with lower expectations, they have lower expectations themselves, or because they need to adjust to a new class. In addition to learning effects, repetition might also have negative consequences for classroom behavior of repeaters. For instance, it has been argued that grade repetition may lower self-esteem of repeating students (e.g. Gomes-Neto and Hanushek (1994), Manacorda (2008)). If, as Lazear (2001) argues, worse behaving students require teachers to shift teaching time to disrupting students, such deteriorating behavior of repeaters would impose negative effects on learning of classmates.

Recent research focusing on the importance of non-cognitive skills also supports the notion that classroom behavior may constitute a mechanism through which grade repetitions impose negative peer effects. Aizer (2008) for instance, investigating the example of ADD, finds that programs which alter student behavior can reduce negative peer effects. Segal (2008) has found that institutional settings such as school rules may, to some extent, affect classroom behavior such as tardiness, inattentiveness, disruptiveness, homework completion, and absenteism (although he concludes that such behavior is not as malleable has might be desired). Accordingly, classes with many repeaters may constitute an environment where teachers find it harder to impose school rules, resulting in worse non-cognitive skills of students which, in turn, may lower achievement. The psychological literature has also been concerned with peer effects operating through classroom behavior. For example, studying language achievement of preschool children, Mashburn et al. (2009) stress that the quality of social interactions within the classroom are important determinants of students' success. They find that language achievement improves if teachers succeed in managing social interactions. In an environment with many repeaters, teachers may have a harder time in managing fruitful interactions between students within the classroom.

[7]A more general positive impact of grade repetition policies can be that merit based retention decision rules can provide an incentive for students to study harder in order not to have to repeat a grade (Manacorda (2008)). Testing this hypothesis is not a focus of this study.

According to these considerations, the sign of a potential peer effect of grade repetitions is a priori unclear. However, given that studies mentioned in the introduction have found that in developing countries, repeaters tend to do worse in school in response to repetition, and in light of the arguments made regarding negative behavioral consequences of grade repetition, any peer effect observed for students in the studied Togolese community would be expected to be negative. The objective of this study is to test this expectation empirically.

When estimating peer effects of grade retention, the reasons leading to grade retentions (and thereby causing variation in the share of repeaters per class) need to be well understood, and I now discuss them for the remainder of this section.

I study peer effects of grade repetition using data on students from all primary schools in a community in the Southwest of Togo. Data from the same community has been used to evaluate the preschool program in chapter 4, and a description of the community is given in chapter 3. In the studied community, children can choose between four different primary schools (commuting to another community to attend a school there is not an option). Among students attending these schools, repeating a class is extremely common. In the data[8], out of the 1460 first through sixth graders for whom exam scores are available, 22.7 percent are currently repeating a class. This figure is comparable to those that have been found in other developing countries, and in particular, it is very close to repetition rates in other sub-Saharan African countries. Manacorda (2008) reports that, according to UNESCO data, the average repetition rate in sub-saharan African primary schools is 20 percent. The sample means given by Frölich and Michaelowa (2005), who use data for fifth graders from several Francophone Sub-Saharan African countries, imply that grades one through four exhibit an average repetition rate of 17.5 percent. In Latin America, repetition rates have been found to be even higher. Gomes-Neto and Hanushek (1994) cite studies which estimate repetition rates of 30 to 55 percent for the first grade in Brazilian primary schools during the 1980s; based on data from northeast Brazil from the 1980s which the authors use for their own analysis they calculate repetition rates of 65, 45, 37, and 32 percent for first, second, third, and forth grade, respectively. According to the data Manacorda (2008) uses for his own analysis, the repetition rate equals 30 percent in Uruguayan junior

[8] The data are further described at the end of section 5.3

high schools. In the sample of Senegalese students used by Glick and Sahn (2010), 76 percent of students report having repeated at least one grade during primary school.

Given that, as discussed above, grade repetition is unlikely to be beneficial for affected students in the studied community, it may be somewhat surprising that schools adhere to this policy. One explanation for the high incidence of grade repetitions in the Togolese community may be that primary schools often accept children who are actually too young to attend primary school.[9] This leads to an unusual heterogenity among first graders with respect to their age. According to the survey data introduced in chapter 3, in 2011, out of all first graders in the community who had just started attending school in fall 2010, 4 percent were two years old, 11 percent were three years old, 16 percent four years old, 36 percent five years old, 18 percent six years old, and 15 percent were seven years old or older. A likely consequence of this phenomenon is that in first grade classes children are very heterogenous with respect to their school readiness. Given the sparse resources at hand, teachers may deem themselves incapable of preparing the least ready children for promotion to the next grade. Consequently, schools might prefer to simply wait until these children grow old enough, before they start promoting them. However, according to table 5.6 in the appendix, the first grade classes in the sample are not the ones exhibiting the highest share of repeaters, which demonstrates that there must also be other reasons for the high incidence of grade repetition than heterogeneity in school readiness among first graders.

While the average share of repeaters in the sample is very significant it also varies strongly between classes. As figure 5.1 in the appendix shows, observed values for that fraction range from 0 to 46 percent. Most of this variation in the share of repeaters per class is within the four schools. The total variance is 129.4, and the within schools and between schools components are 110.7 and 18.7, respectively.[10] Given that the four

[9]This phenomenon has been discussed in more detail in chapter 4

[10]This calculation is based on the following formula, also used by Ammermueller and Pischke (2009):

$$1/C\sum_{s=1}^{S}\sum_{c=1}^{C_S}(x_{CS}-\bar{x})^2 = 1/C\sum_{s=1}^{S}\sum_{c=1}^{C_S}(x_{CS}-\bar{x_S})^2 + 1/C\sum_{s=1}^{S}C_S(\bar{x_S}-\bar{x})^2 \tag{5.1}$$

where the left hand side is equal to the total variance in the share of repeaters per class (x_{CS}), and the right hand side shows the within and between schools components. C is equal to the number of classes in the sample, C_S is the number of classes in school S. $\bar{x_S}$ is the average share of repeaters per class in school S, and $\bar{x}$ equals the average share of repeaters per class in the sample.

schools are located in the same community it appears plausible that their retention policies do not differ much.

Even though the variation in the share or repeaters per class between schools is relatively small, regressions will control for school choice by including school dummy variables. Since school choice might be influenced by factors that are also correlated with a school's average grade retention probability, not accounting for it may bias results.

The central assumption of the empirical analysis will be that, conditional on school choice, first trimester scores, and individual retention status, the average retention status of class mates can be seen as a predetermined variable from the point of view of the individual student. In order to assess the plausibility of that assumption, it is necessary to discuss all potential determinants of the share of repeaters per class. Within schools, the class a child is taught in when it is first enrolled is not subject to the student's or parents' choice, because each school only comprises one class per grade.[11] The retention status of classmates depends on the exam results of students who attended the same grade during the previous calendar year and on how the retention decision rule is enforced.

Formally, a student needs to reach a target regarding his total score during the last trimester of a school year in order to be admitted to the following grade. Accordingly, conditional on the number of non-repeating classmates, the share of repeaters in the current class depends on the achievement of students who attended the same grade during the previous calendar year. Thus, the share of repeaters increases if last year's students obtained low scores because they learned less. In an education production framework, the current share of repeaters would thus be seen as determined by last year's education production inputs, i.e. student level factors and teacher quality.[12]

Strong ties between the likelihood of grade repetition and exam results in the previous school year are consistent with the explanations given by other studies for the high

[11] The situation would be more complex if students switched schools frequently. The following discussion would then have to elaborate on how to take into account individual determinants of the likelihood of choosing a particular school at the beginning of each school year. However, out of the 1331 individual observations used for the estimations presented in section 5.4, only 4 correspond to students who are observed to have attended a different school during the previous school year. Accordingly, the problem of students switching between schools is neglected for this study.

[12] In addition, the share of repeaters might also be high if grading standards during the previous year have been particularly high. This issue is further discussed in section 5.3.

incidence of grade repetition in many countries. Gomes-Neto and Hanushek (1994) refer to potential causes of low achievement which may result in a higher probability of repetition if retention decisions are primarily merit-based. For instance, low achievement may be caused by poverty and bad health. Causes on the school level would include low teacher quality and low quality and quantity of other resources such as textbooks. In their study for Brazil, they find limited scope for overruling merit-based retention decisions. For instance, family background is found to be unimportant, implying that parents do not significantly influence the retention decision. Manacorda (2008) summarizes studies from industrialized countries as indicating that family socio-economic status, educational inputs, and early childhood interventions affect the likelihood of grade repetition, and he refers to two studies from developing countries indicating that cash transfers increase the likelihood of grade promotion. Glick and Sahn (2010) show that for Senegalese primary school students test scores at the end of second grade are the most important determinant of the likelihood of repeating that grade. Among the household background variables only a measure for household wealth had a significant effect when controlling for test scores. Other important determinants were classroom supplies as well as whether the teacher was female (particularly relevant for girls).

Given that the studied schools also have a merit based retention policy, the share of repeaters per classroom can be expected to be determined to a large extent by factors in the educational production for the same grade in the same school during the previous school year. However, those past inputs are not the only determinants of the share of repeaters per class in the current year if the retention rule described above is not strictly enforced.

Table 5.1 documents how the individual likelihood of repeating a grade varies with a student's third trimester exam results. The exam results are standardized such that they indicate how many percent of the grade specific target points a student has reached[13]. Thus, a score of 100 or more would indicate that a student is formally entitled to continue with the subsequent grade. The figures indicate that there is a strong negative relationship between a student's third trimester exam results and his likelihood of re-

[13]The relevant thresholds have been obtained by interviewing the principals of the four schools. They vary by grade, and they are identical for all four schools. They are equal to 30 for first grade, 35 for second grade, 50 for third and fourth grade, and 70 points for fifth and sixth grade.

peating a class. The vast majority of repeaters have missed the grade specific target. The likelihood of compliance rises for very low exam results. Note that the observation of the likelihood of retention being strongly related to prior individual achievement is consistent with the notion that repeaters constitute a sample of students who actually do need more time to master the grade's skill level than other students. Thus, it is possible that grade repetition may actually enhance learning of affected students (Gomes-Neto and Hanushek (1994)). However, according to table 5.1 there is no sharp drop in the likelihood of repeating at the threshold. Apparently, many students do not comply with the retention rule.[14]

The deviations from the formal retention rule as documented in table 5.1 imply that teachers or other school staff frequently overrule the test results. A likely explanation is that other student characteristics can make up for low test scores. For instance, even a low scoring student might be considered mature enough to be promoted to the next grade. Accordingly, variation across classes in aspects of personality and cognitive development that are not sufficiently captured by exam results will also translate into variation in the share of repeaters per class.

Moreover, the share of repeaters per class might be affected by variation in the likelihood of disregarding the formal retention rule. A class has many repeaters if the enforcement of the retention rule for the same grade in the previous year was particularly strict. Within schools, enforcement intensities might vary due to changes in teacher assignment to classes. In addition, schools/teachers might adapt enforcement intensities to "shocks" like variations in cohort size, etc..

Table 5.8 in the appendix examines the determinants of the share of repeaters by estimating linear regression models at the class level. Both the number of failures in the same grade last year and the previous class size are associated with the share of repeaters today (with the exception of model 5.4 for previous class size), and they explain a significant portion of the variation in the share of repeaters (as can be seen by comparing the R^2 of the restricted model, which includes neither the number of failures, nor previous class size, with an unrestricted model's R^2, which includes either

[14]Accordingly, the institutional background of the studied schools is not suitable for applying a regression discontinuity design similar to the study by Jacob and Lefgren (2004) in order to determine the causal effect of grade retention on repeating individuals.

Tab. 5.1: Share of students who repeat or drop out, grouped by third trimester exam results

Third trimester results	Repeats	Drops out	N
[0, 20[	0.62	0.29	21
[20, 30[	0.53	0.36	36
[30, 40[	0.75	0.19	52
[40, 50[	0.61	0.26	80
[50, 60[	0.54	0.33	114
[60, 70[	0.43	0.26	157
[70, 80[	0.21	0.31	137
[80, 90[	0.27	0.23	142
[90, 100[	0.12	0.25	143
[100, 110[	0.04	0.24	197
[110, 120[	0.05	0.22	190
[120, 130[	0.02	0.24	155
[130, 140[	0.05	0.27	130
[140, 150[	0.01	0.21	91
[150, 160[	0.03	0.15	62
[160, 170[	0.02	0.23	48
[170, 180[	0.06	0.29	35
>=180	0.06	0.59	34
Total	0.20	0.26	1824

Sample: all students for whom the subsequent grade and the same grade are observed in the following school year. Note that this ”forward looking” sample differs from the one used to calculate the total share of repeaters at the beginning of section 5.2 and for the estimations in the following sections which is ”backward looking” in the sense that it requires the same grade to be observed during the previous school year. This precludes using observations from the earliest school year within a grade-school combination for which classes are observed for several school years in a row. The reverse is true for the ”forward looking” sample, however, the last school year for which classes are observed generally is the school year 2010/2011, but that year is also excluded from the ”backward looking” sample, because it only includes observations for the first trimester - which explains the difference in sample sizes.

the number of failures or previous class size). Note how the number of failures translates almost one to one into repeaters when looking at variation within groups of classes from several years within the same grade within the same school (model 5.4). These results show that for the student level estimates discussed in section 5.4 a very large part of the identifying variation in the share or repeaters stems from variation in the number of students who failed the same grade one year earlier. Particularly when focusing on different cohorts of students within the same grade and school, achievement of the preceding class of the same grade will be the most important determinant of variation in the share of repeaters for a current class.

Referring to table 5.1 again, also note that dropout rates are very high[15], but they are apparently unaffected by third trimester exam results. In other words, students do not appear to drop out of school in response to missing the grade point target. This is an important result, because it suggests that factors driving dropout decisions are different from those determining retention probability. If this were not the case, empirically modeling the process of individual selection into classes would potentially become more complicated.

5.3 Empirical strategy and data

Given the institutional background described in the previous section, I estimate regression models of individual student achievement (as measured by third trimester total scores) which control for first trimester scores as well as the number of non-repeating students per class and dummy variables indicating the primary school attended. Thus, I am investigating the association between the change in individual achievement within one school year and the share of repeaters among classmates, where the variation in the share of repeaters only comes from within-school variation in the number of repeaters (rather than within-school variation in the number of non-repeaters). Similar to earlier studies (Neidell and Waldfogel (2010), Ammermueller and Pischke (2009), Lavy and Schlosser (2011), Lavy et al. (2008)), I argue that the estimates from such a value added model of achievement, conditional on school effects, identify the reduced form

[15]Based on the data at hand, I define dropped out students as those for whom observations cannot be linked to an observation in the subsequent school year.

peer effect of the share of repeaters per classroom. My main model is defined by the following equation:

$$\begin{aligned} y_{itcs,trimester=3} = \; & \alpha + \beta_1 SR_{-i,tcs} + \beta_2 N^{NR}_{tcs} + \beta_3 y_{itcs,trimester=1} + \beta_4 R_{itcs} \\ & + \beta_5 S^B_{tcs} + \beta_6 S^C_{tcs} + \beta_7 S^D_{tcs} + \gamma' X_{tcs} + \epsilon_{itcs} \end{aligned} \tag{5.2}$$

where $y_{itcs,trimester=3}$ indicates the third trimester exam results of student i in year t in class c, and in school s. $SR_{-i,tcs}$ denotes the share of a student's classmates who are repeaters while controlling for the number of non-repeaters (N^{NR}_{tcs}). S^k_{tcs} is a dummy variable indicating whether an observation is from school k, and R_{itcs} denotes the individual's current retention status. X_{tcs} includes additional class level controls which are either grade dummy variables, school year dummies, or both. Exam results from the first trimester, $y_{itcs,trimester=1}$, are also added as a control variable. Since first trimester exam results are measured relatively shortly after peer group formation, they can be assumed to reflect peer influences to a very limited extent. Neidell and Waldfogel (2010) employ a similar strategy. When investigating Kindergarten students they estimate the impact of peer characteristics on test scores in spring while controlling for test scores from the preceding fall in the same school year. When relying on such a value added specification, the intention is to use previous test scores as control for all previous inputs to educational production. It requires the assumption that the effects of all inputs in the production process relevant to the the first trimester score have developed (i.e. decayed) until the third trimester at the same rate β_3 (Hanushek et al. (2003), Ding and Lehrer (2007)). In other words, model 5.2 assumes that the change in test scores during the current school year is unrelated to, for instance, teacher behavior in the previous school year as well as to all other inputs in an individual student's educational production during the previous year, which implies a specific form of the education production function.

In model 5.2, identification of β_1 relies on variation within the four schools. As noted in the introduction, including school fixed effects has been a standard approach in the literature in order to take into account selective school choice. The reasoning underlying the concern that school choice introduces endogeneity is that school choice is correlated with background characteristics such as the type of neighborhood (Ammermueller and Pischke (2009)). However, in this study, all four observed schools are located in the same community, and differences in background of students between the schools can be assumed to be smaller than in other studies which sample students from different schools across a country. Still, school fixed effects are included in order to account for any heterogeneity across schools that may be present.

The identification strategy must avoid that unobserved selection effects, i.e. factors which affect the likelihood of ending up in a class with a high or low share of repeaters, are still included in the error term of equation 5.2.[16] However, since schools in the community only include one class per grade, many of the practical concerns regarding selective group formation within schools as mentioned in other studies (e.g. grouping of students within the same grade into separate classes according to their abilities) are not relevant to the scenario investigated here. Instead, it is known exactly how the individual likelihood of ending up in a particular class is determined: it depends on a child's entry cohort, it's probability of dropping out of school before reaching a particular grade, and on the number of grades it has repeated.

As seen above, the likelihood of dropping out appears to be driven by other factors than those determining the share of repeaters in a class, so I do not consider it as a threat to the identification strategy.

Regarding individual grade repetitions, it is evident that factors determining the likelihood that a child currently repeats a grade are also correlated with it's current classes' share of repeaters. Since the regressions control for the number of non-repeating students per class, this potential source of bias only concerns repeaters. As far as the retention decision is merit based and the assumptions underlying value added specifications are met, taking into account first trimester exam results should already control for the determinants of individual retention status. However, as discussed in the previous section, factors other than previous test scores also seem to affect the likelihood of grade repetition. Thus, all regressions include a dummy for whether a student currently repeats a grade as additional explanatory variable.

[16]Apart from selective peer group formation, the share of repeaters per class could, technically, be correlated with the error term due to common shocks during the current school year. However, contemporaneous shocks are unlikely to be associated with predetermined peer characteristics (Lyle (2007)).

Accordingly, I argue that estimates of β_1 rely on exogenous variation in the share of repeaters which reflects variation in the likelihood of retention within schools from cohort to cohort, grade to grade, and year to year. As discussed in section 5.2, the variation in the share or repeaters reflects, to a large extent, variation in achievement across classes within the same grade in the previous year, and other sources of variation include student characteristics not reflected in test scores as well as the strictness with which the merit-based retention rule is enforced. All estimates shown in section 5.4 will report standard errors clustered at the class level.

In order to assess the robustness of the results obtained by estimating model 5.2, I also estimate additional specifications that add dummy variables for smaller units than schools. Determining how the peer effect coefficient changes in response to such alternations in model specification allows to observe whether the size or even the sign of the peer effect depends the particular source of variation that is exploited for its estimation. In the related literature, this point is often made to motivate comparisons between estimates resulting from models with and without school fixed effects (Neidell and Waldfogel (2010)). Model 5.3 is specified as

$$\begin{aligned} y_{itcs,trimester=3} = \; & \alpha + \beta_1 SR_{-i,tcs} + \beta_2 N^{NR}_{tcs} + \beta_3 y_{itcs,trimester=1} + \beta_4 R_{itcs} \\ & + \textstyle\sum_{t=2}^{T} \sum_{s=1}^{S} \beta_{t,s} \left(T^t_{tcs} \times S^s_{tcs}\right) + \gamma' X_{tcs} + \epsilon_{itcs} \end{aligned} \tag{5.3}$$

where the T^t_{tcs} denote school year dummies (counting from 2, which is the second school year in the sample i.e. 2005/2006 to T, the last year in the sample, which is 2009/2010). Model 5.3 exploits variation within each school-year combination. Thus, looking at students who attended one school in a particular year, different grades are compared.

However, in models 5.2 and 5.3, endogeneity could arise if controlling for first trimester scores does not adequately account for educational inputs and teacher behavior during the previous school year. Such problems are particularly likely to arise for classes in the same school and grade, and even more so for classes taught by the same teacher. For instance, rather than being randomly assigned to classes each year, teachers in the studied community tend to be responsible for the same grade for several years in a row. If one grade in a school is taught by a particularly bad teacher for two years in a row, then the share of repeaters in the second year might be high and average student achievement might be low simply due to low teacher quality.

In order to verify whether coefficient estimates change when I focus on variation in the share of repeaters within groups of observations that are likely to be exposed to some common educational production inputs and similar teacher behavior, I also estimate the following two specification:

$$\begin{aligned} y_{itcs,trimester=3} = \quad & \alpha + \beta_1 SR_{-i,tcs} + \beta_2 N_{tcs}^{NR} + \beta_3 y_{itcs,trimester=1} + \beta_4 R_{itcs} \\ & + \textstyle\sum_{c=1}^{C} \sum_{s=1}^{S} \beta_{c,s} \left(G_{tcs}^{t} \times S_{tcs}^{s} \right) + \gamma' X_{tcs} + \epsilon_{itcs} \end{aligned} \tag{5.4}$$

where G_{tcs}^{t} are grade dummies. Model 5.4 exploits variation within each school-grade combination. Thus, looking at all students who ever attended one grade in a particular school, the specification compares the different years (i.e. cohorts of students), which makes the strategy similar to the one applied by Hoxby (2000) who compares adjacent student cohorts within the same grade within the same school; also see Hanushek et al. (2003) for a similar strategy. Information on which teacher was assigned to which class is also available in the data, so replacing G_{tcs}^{t} by teacher dummies would provide an additional useful model. However, since during the period of observation teachers do not switch schools or grades, this also amounts to a comparison between classes of the same grade but between different years. Since, in addition, a single teacher is responsible for teaching all subjects in one class, and, as table 5.7 in the appendix documents, only very few changes in teacher assignment between different years are observed, estimates controlling for teacher dummies would be based on very much the same variation as those obtained from model 5.4. Unsurprisingly, estimation results from both of these specifications turned out to be very similar (results controlling for teacher dummies are not shown).

A concern regarding the interpretation of the results based on models 5.2 through 5.4 may be that the effect on scores does not reflect a change in achievement but rather changes in grading standards. For instance, in response to the presence of many repeaters, a teacher may lower grading standards in order to avoid that too many students would have to repeat yet again. As a result, test scores would measure actual achievement with error. If this were true, this would result in an upward bias in the estimate of the effect on actual achievement, such that the estimate of β_1 could be interpreted as giving upper bounds of the achievement effect. Moreover, controlling for first trimester scores should reduce the potential bias caused by any adjustments in grading standards as long as these affect only the level of a students scores throughout a school year, not the change in scores.

Furthermore, a teacher might take into account a student's status as either a repeater or a non-repeater when judging his performance. For example, he may give a better score to a non-repeater than to a repeater even though their true performance is identical, because he may value the repeaters' achievement lower, as they had to take the class twice in order to arrive at a given skill level. Consequently, grading standards for non-repeaters may be lowered, or standards for repeaters may be increased, or both. The direction of a potential bias in the estimated effect on actual achievement induced by such behavior is ambiguous, because it is not clear whether the average grading standard in a class would increase or decrease in response to additional repeaters. Again, controlling for first trimester scores should reduce the potential bias due to such adjustments in grading standards if they do not affect the change in individual test scores.

The data used for this study were collected parallel to the deployment of the second wave of the household survey described in chapter 3 which was conducted in the same community in January 2011. The principals of all four primary schools allowed access to all administrative student data they could find in their archives. These consisted of hand written notebooks which teachers used to keep track of exam results for all students. The resulting list included each attending student by name and, for each trimester, the students' exam results for each subject as well as the total score. These tables were first scanned, and then transformed into spreadsheets which were assembled to form a single dataset suitable for econometric analysis.

Notebooks that could be recovered ranged from the school year 2003/2004 until the first trimester of the school year 2010/2011. Unfortunately, data from this time period are not complete. Presumably, due to frequent changes in personnel, many notebooks were simply misplaced. Even more mundane reasons for data loss included termite infestation. Table 5.7 in the appendix illustrates for which combinations of school, school year, and grade data is available for the final sample. The original data include 2856 observations with non-missing third trimester exam results (one observation per student) in 90 classes. In order to compute the share of repeaters and the individual retention status, an observation can only be included in the sample if class exam results are also available for the same grade in the same school in the previous school year. This restriction reduces the number of observations to 1585 in 54 classes. In order to estimate value added specifications, the first trimester score needs to be available for a given school year and a given student as well, which further reduces the sample size to 1460 observations in 52 classes.[17] Finally, the estimation of model 5.4 require variation within grades of the same school. Thus, all classes are dropped which constitute the single class observed for one grade in given school. This restriction leads to a final sample of 1331 observations in 48 classes. Note that the way in which estimated models are specified, the data are treated is if they were repeated cross sections. In reality, since some of the cohorts a covered by more than one of the cross sections (as defined by a school year), it is theoretically possible to take into account observations from more than a single school year for an individual. However, given that data for many classes are missing, the resulting individual panel data would be very unbalanced, and it would contain many students who appear in the data only for one school year. Accordingly, panel data methods would hardly improve upon the value added models presented above.

A advantage of the data used for the analysis is that for each class in the data usually the complete class is observed. As a result, I do not have to deal with measurement error in the peer characteristics variable which can result if peer averages are calculated using a subsample of students (Ammermueller and Pischke (2009)). However, the children appearing in the data are still a non-random sample of the population of equally aged children in the studied community. A concern regarding the interpretation of estimates based on these data may be that children attending school are different from other children with respect to learning and their reaction to various types of peers. However, in the studied community, enrollment rates in primary school are very high. Out of the

[17]The loss of student data notebooks as described above included cases were third trimester and first trimester results were documented in different notebooks, and only the former were recovered.

551 children who are between six and twelve years old (the regular primary school age group) and who are observed in the household survey described in chapter 3 in 2011, 93 percent were reported to be currently attending school. Furthermore, if a main policy interest is to learn more about the determinants of school quality, then children currently attending school can themselves be a population of interest.

A major challenge during the process of data preparation was to link individual observations between consecutive school years in order to determine whether a student proceeded regularly, repeated a grade or dropped out of school. This had to be accomplished based on the students' names as indicated in the schools' notebooks. However, the exact spelling of the same name may differ depending on the teacher who writes it down, and even for the same teacher noting the same name at several occasions. In addition, the first name indicated by students may differ from one occasion to another, as most children in Togo have at least two first names which are used interchangeably. In order to solve the problem an algorithm similar to the one that linked observations between the two waves of the household survey as described in section 3.3 was used. The main difference between the two approaches was that the algorithm used for the student data had to take into account that in the student data, the same individual would usually appear more than twice. After this step of data preparation, a student was defined to be a repeater in the current school year if his or her name appears in both the current school year's class list as well as in the same grade and the same school one year earlier.

Table 5.6 in the appendix summarizes descriptive statistics based on the sample used for the analysis discussed in the following sections.

5.4 Results

This section shows the results of estimating models 5.2 through 5.4 using all available student data from the studied Togolese community fulfilling the sample requirements described in the previous section. Table 5.2 presents the main results, displaying coefficient estimates for the share of repeaters per class and for individual first trimester test scores with varying models and specifications. At first sight, estimates of the peer effect are consistently negative, and they are statistically significant for most estimated models.

The rows in table 5.2 correspond to different models estimated, namely a baseline model without fixed effects, and models 5.2 through 5.4 discussed in section 5.3. The columns correspond to variations in these models which differ with respect to which additional control variables are included as indicated at the bottom of the table. These additional controls include first trimester scores (which was a standard control variable in the equations presented in section 5.3, so for models 5.2 through 5.4 columns 5 through 8 are consistent with the presentation in section 5.3), school year dummies, grade dummies, or any combination of these. This setup allows to give a first impression of the robustness of the results.

The effect of first trimester scores on third trimester scores is estimated very precisely. The effect is about 0.6, and it is extremely robust to changes in specification (this includes additional variants of the model which will be discussed in section 5.5). Comparing the columns 1 through 4 with columns 5 through 8 in table 5.2 shows that using a value added specification (i.e. including fist trimester results as a control variable) significantly affects the estimated peer effect only if grade effects are not controlled for. Furthermore, when focussing on the results from the value added models in columns 5 through 8, the size of the estimated peer effect varies within a relatively small range from -.33 to -.56. When exploiting only variation in the share of repeaters within the same grade and same school (model 5.4), coefficient estimates are slightly larger, but overall, there is no clear pattern linking the size of the estimate with the particular source of variation in the share of repeaters exploited for its computation. It is particularly interesting to note that, comparing the baseline model with model 5.2, including school dummies only has a very small impact on the peer effect estimate. Thus, as suspected in section 5.3, non-random selection into different schools is less relevant to this study than in earlier literature. Furthermore, the general robustness of the peer effect estimate as seen from table 5.2 increases confidence in that the identification strategy successfully accounts for all other aspects of peer group formation.

Table 5.3 shows results from estimating a subset of the models without including individual retention status as a control variable. This change in specification hardly affects coefficient estimates at all, even when not controlling for first trimester exam results. This suggests that common determinants of retention status and the share of repeaters per class do not affect current achievement. The estimated coefficient for retention status when it is included in the model as in table 5.2 is not interpreted here.[18]

Regarding the size of the peer effect, the middle of the interval between the lowest and the highest coefficient estimate from value added models presented in table 5.2 is equal to 0.45. In other words, an increase in the share of repeaters by one percentage point reduces standardized test scores by about .45 points.

In terms of standard deviations a one standard deviation increase in the share of repeaters per class reduces test scores by 13 percent of a standard deviation. This figure is slightly above the range between .05 and .10 which Ammermueller and Pischke (2009), summarizing previous studies on peer effects, indicate to be the range within which the largest part of estimates fall. However, larger effects have been found as well. The mean effect according to the analysis in Ammermueller and Pischke (2009) is .17. Hanushek et al. (2003) find an effect of .35. Larger effects have also been found in Hoxby (2000) and McEwan (2003). Naturally, the size of estimates can be expected to depend on the peer characteristic that has been considered and on the particular context of each study.

5.5 Robustness and potential mechanisms

Table 5.4 shows estimates of the effect of the share of repeaters where the dependent variable has been replaced by scores for any of the five subjects that are consistently graded for all schools and grades in the sample: dictation, mental arithmetic, (written) math, math word problems, and drawing. Although all coefficients are estimated with rather low precision, a clear pattern can be observed. The effect of classroom composition on student achievement is not homogenous across subjects, and the subjects most clearly negatively affected are mental arithmetic and math word problems.

The result that peer effects are more relevant to math achievement than to outcomes in other subjects is consistent with similar findings in various contexts. For Israeli primary schools, Lavy and Schlosser (2011) find that gender composition in the classroom only affects math, science and technology test scores, but not language test results.

[18] See Manacorda (2008) and Jacob and Lefgren (2004) for studies which carefully identify the causal effect of retention status on individual achievement of repeaters.

Tab. 5.2: Coefficient estimates for the share of repeating classmates

	Specifaction							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline model							
Share of repeaters	-.97***	-1.11***	-.38	-.47*	-.41*	-.47**	-.33	-.41*
	.31	*.29*	*.25*	*.25*	*.22*	*.22*	*.22*	*.21*
First trimester score					.59***	.6***	.6***	.61***
					.03	*.03*	*.03*	*.03*
	Model 5.2							
Share of repeaters	-.93***	-1.04***	-.29	-.32	-.46*	-.5**	-.37	-.4*
	.3	*.3*	*.24*	*.24*	*.24*	*.24*	*.23*	*.21*
First trimester score					.58***	.59***	.61***	.62***
					.04	*.03*	*.03*	*.03*
	Model 5.3							
Share of repeaters	-1.07***		-.29		-.48*		-.49*	
	.29		*.26*		*.25*		*.26*	
First trimester score					.59***		.63***	
					.03		*.03*	
	Model 5.4							
Share of repeaters	-.38	-.44*			-.51**	-.56***		
	.24	*.24*			*.2*	*.19*		
First trimester score					.62***	.63***		
					.03	*.03*		
Additional controls:								
First trimester score					✓	✓	✓	✓
Year dummies		✓		✓		✓		✓
Grade dummies			✓	✓			✓	✓

All regressions control for number of non-repeating classmates as well as individual retention status. Sample: Observations for all students from classes for which results for the same grade in the same school in the previous school year are available and for which at least one more class in the same school and same grade also fulfills the same requirement; individual observations are dropped for all students form whom first or third trimester total exam results are missing (N=1331). Standard errors of coefficient estimates are given in italic; asterisks indicate statistical significance at the 1-, 5-, and 10-percent significance level, respectively. Standard errors are clustered at the class level.

Tab. 5.3: Coefficient estimates for the share of repeating classmates - not controlling for individual retention status

	Specifaction (1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline model							
Share of repeaters	-.96***	-1.1***	-.37	-.47*	-.41*	-.47**	-.33	-.41**
	.31	*.29*	*.25*	*.25*	*.22*	*.22*	*.22*	*.2*
First trimester score					.59***	.6***	.61***	.61***
					.03	*.03*	*.03*	*.03*
	Model 5.2							
Share of repeaters	-.93***	-1.04***	-.3	-.34	-.46*	-.51**	-.37	-.4*
	.3	*.3*	*.25*	*.25*	*.24*	*.24*	*.23*	*.21*
First trimester score					.58***	.59***	.61***	.62***
					.04	*.03*	*.03*	*.03*
	Model 5.4							
Share of repeaters	-.39	-.47*			-.51**	-.56***		
	.25	*.25*			*.2*	*.19*		
First trimester score					.62***	.63***		
					.03	*.03*		
Additional controls:								
First trimester score					✓	✓	✓	✓
Year dummies		✓		✓		✓		✓
Grade dummies			✓	✓			✓	✓

All regressions control for number of non-repeating classmates. Sample: Observations for all students from classes for which results for the same grade in the same school in the previous school year are available and for which at least one more class in the same school and same grade also fulfills the same requirement; individual observations are dropped for all students form whom first or third trimester total exam results are missing (N=1331). Standard errors of coefficient estimates are given in italic; asterisks indicate statistical significance at the 1-, 5-, and 10-percent significance level, respectively. Standard errors are clustered at the class level.

Tab. 5.4: Effect of share of repeaters on raw scores in different subjects

	Dependent variable: Dictation	Mental arithmetic	Math	Word problems	Drawing
Model 1	-.003	-.022	-.001	-.04	.013
	.022	*.026*	*.027*	*.028*	*.013*
Model 3	-.029	-.039	-.018	-.038*	.017
	.022	*.028*	*.026*	*.02*	*.014*
Number of observations	1328	1329	1328	1327	1328

All regressions control for number of non-repeating classmates, school dummies, year dummies, grade dummies as well as individual retention status. Sample: Observations for all students from classes for which results for the same grade in the same school in the previous school year are available and for which at least one more class in the same school and same grade also fulfills the same requirement; individual observations are dropped for all students form whom first or third trimester results for the respective subject are missing. Note that in cases of missing subject scores, the total trimester score can still be available, because it was recorded by teachers in an extra column. Standard errors of coefficient estimates are given in italic; asterisks indicate statistical significance at the 1-, 5-, and 10-percent significance level, respectively. Standard errors are clustered at the class level.

According to the results in Hoxby (2000), positive peer effects for Texan primary school students associated with a higher share of classmates being female are larger for math than for reading. For students of the US Air Force Academy, Carrell et al. (2009) find that peer quality matters only for math and science achievement as opposed to physical education and foreign languages.

Given the high relevance that has been attributed to math and science education in previous studies, the finding that classroom composition matters particularly for math scores is quite interesting. On an aggregate level, mathematical skills have been found to be associated with economic growth (Hanushek and Woessmann (2008)). For both developing and industrialized countries, it has been shown that the private returns to learning math can be very high (Hanushek and Woessmann (2008), Joensen and Nielsen (2009)).

Another aspect regarding the potential channel of the estimated peer effect is whether the results allow to distinguish between the impact of a change in classroom composition and a change in class size. Since all regressions presented so far control for the number of non-repeating students, an increase in the share of repeating classmates actually means two things: the retention status of the average classmate changes, but the class also increases in size. In an attempt isolate the classroom composition effect, I estimated alternative models that control for total class size rather than the number of non-repeating class mates.

A potential drawback of this approach is that variation in the share of repeaters can now be due to both a change in the number of repeaters and a change in the number of non-repeaters. Accordingly, identification assumptions for these models are stronger than for the models discussed above, since they would include assumptions not only about inputs to educational production for the same grade last year but also for the preceding grade last year. However, as table 5.9 in the appendix shows, the estimates for the effect of the share of repeaters are very similar whether class size is controlled for or the number of non-repeating class mates. Consequently, I interpret the results presented in tables 5.2 and 5.4 as indicating the effect of a change in classroom composition.

Another approach to examining potential mechanisms that contribute to the classroom composition effect is to add specific control variables which further narrow down the type of variation in the share of repeaters identifying the estimated coefficient. Two such variables on the class level which are both available in the data and are intermediate variables in the determination of the share of repeaters are class size and number of failing students in the previous class (of the same grade).

The number of failing students in the previous year should reflect both the ”output” of the previous class’ educational production as well as their teacher’s grading standard. Thus, when controlling for the number of failures from the previous class, the variation in the share of repeaters that is ”left over” should reflect deviations from the retention rule.

The size of the previous class should reflect variations in cohort size, but also educational achievement of even earlier classes (because last year’s class size is in part determined by last year’s number of repeaters). When controlling for the previous class’ size, the remaining variation in the share of repeaters should reflect variation in average achievement in last year’s class, variations in last year’s grading standards as well as deviations from the retention rule.

Table 5.5 presents results for the respective estimations of the effect on individual test scores. In comparison with table 5.2, coefficient estimates of the effect of the share of repeaters tend to be somewhat larger in absolute size. However, neither adding the number of failures nor previous class size significantly affects the main results, even though the process causing the identifying variation in the share of repeaters is quite different. Thus, classroom composition changes due to an increased number of repeaters negatively affect student achievement irrespective of the reason for the increased share of repeaters.

5.6 Conclusion

In order to investigate the peer effects of grade retention, this study used administrative student data from four primary schools in a Togolese community. Identification relied on variation in the share of repeating classmates within schools. I argued that, given the primarily merit-based retention policy in the studied schools, the estimated value added models of student achievement appropriately account for selection into classes when controlling for school choice and individual retention status. The identifying

Tab. 5.5: Coefficient estimates for the share of repeating classmates: controlling for previous classes' size and number of failures

	Controlling for number of failures				Controlling for previous class size			
	(5)	(6)	(7)	(8)	(5)	(6)	(7)	(8)
	Model 5.2							
Share of repeaters	-.61**	-.64***	-.5**	-.54***	-.46*	-.51**	-.38*	-.43**
	.25	*.23*	*.21*	*.2*	*.24*	*.23*	*.23*	*.21*
First trimester score	.6***	.62***	.61***	.62***	.57***	.59***	.6***	.61***
	.03	*.03*	*.03*	*.03*	*.04*	*.03*	*.03*	*.03*
	Model 5.4							
Share of repeaters	-.6***	-.66***			-.51**	-.56***		
	.22	*.23*			*.2*	*.2*		
First trimester score	.62***	.62***			.62***	.63***		
	.03	*.03*			*.03*	*.03*		
Additional controls:								
First trimester score	✓	✓	✓	✓	✓	✓	✓	✓
Year dummies		✓		✓		✓		✓
Grade dummies			✓	✓			✓	✓

All regressions control for number of non-repeating classmates as well as individual retention status. Sample: Observations for all students from classes for which results for the same grade in the same school in the previous school year are available and for which at least one more class in the same school and same grade also fulfills the same requirement; individual observations are dropped for all students form whom first or third trimester total exam results are missing (N=1331). Standard errors of coefficient estimates are given in italic; asterisks indicate statistical significance at the 1-, 5-, and 10-percent significance level, respectively. Standard errors are clustered at the class level.

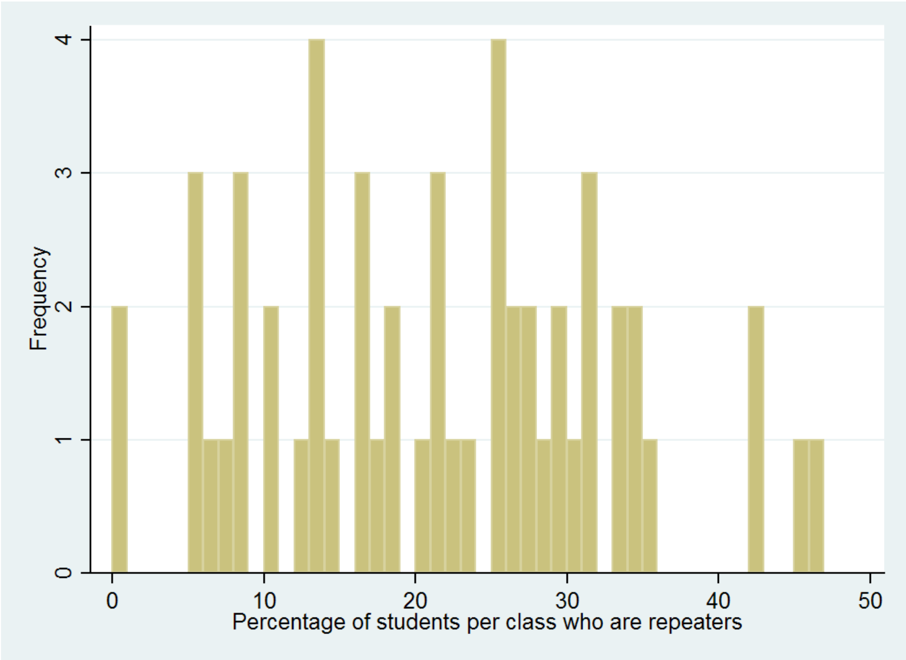
variation in the share of repeaters per class came from, essentially, comparing different cohorts of students within schools.

The estimates consistently show that test scores are significantly negatively affected by increases in the share of repeaters. A one standard deviation increase in the share of repeaters per class is estimated to reduce individual test scores by about 13 percent of a standard deviation. Estimates are robust to changes in model specification associated with different sources of variation in the share of repeaters. The effect is heterogenous across subjects taught, and it is particularly pronounced for mental arithmetic and math word problems. I also showed that the estimates can be interpreted as showing the impact of actual changes in class composition rather than changes in class size.

A drawback of the data used for the analysis is that they do not include additional individual control variables. In order to further verify the robustness of the estimated peer effect it would be preferable to be able to control for additional determinants of educational achievement. Since the household data described chapter in 3 were collected in the same community, the majority of children from the student data can be expected to be included in the household data as well. Since the household data also include information on the children's names and current enrollment status, including the grade and school they attend (for both waves, i.e. in fall 2008 and early 2011), it is, technically, possible to match observations from both datasets. However, given the frequent misspelling of names and the ambiguous use of first names described in section 5.3, this is a task that is either very programming- or time-intensive (if done by hand), and it is left for future research.

5.7 Appendix: additional tables and figures

Fig. 5.1: Share of students per class who are repeaters



Tab. 5.6: Means and standard deviations of variables used for the analysis

		All Grades	Grade 1	2	3	4	5	6
Student level characteristics								
Total scores, 3rd trimester	Mean	95.47	114.8	102.94	82.59	81	79.73	123.98
	S.D.	34.46	38.88	28.85	29.5	32.36	20.18	18.38
Total scores, 1st trimester	Mean	96.28	128.65	104.49	72.77	88.3	79.5	112.64
	S.D.	37.8	33.01	31.31	31.3	34.57	19.46	15.66
Retention status	Mean	.22	.2	.2	.24	.28	.23	.12
	N	1331	301	322	431	143	100	34
Dictation, 3rd trimester	Mean	3.43	5.8	2.39	3.12	3.17	1.46	2.96
	S.D.	3.08	2.31	2.41	3.27	2.86	2.73	1.97
	N	1331	301	322	431	143	100	34
Mental arithmetic, 3rd trimester	Mean	5.07	6.24	4.66	4.24	5.38	5.46	6.65
	S.D.	3.09	2.95	3.21	3.09	2.6	2.66	1.95
	N	1331	301	322	431	143	100	34
Math, 3rd trimester	Mean	5.57	6.05	4.01	5.11	4.32	9.53	15.4
	S.D.	3.72	3.07	2.62	2.68	3.36	4.61	4.31
	N	1330	301	321	431	143	100	34
Math word problems, 3rd trimester	Mean	6.32	9.04	7.91	4.04	4.42	5.17	7.6
	S.D.	4.07	2.36	3.41	4.2	3.19	3.68	2.5
	N	1328	301	319	431	143	100	34
Drawing, 3rd trimester	Mean	6.05	5.69	5.77	6.25	6.5	6.27	6.87
	S.D.	1.05	1.15	.9	.97	.94	1.06	.71
	N	1329	301	321	431	143	99	34
Class level characteristics								
Number of repeaters	Mean	6.4	5.82	5.82	8.77	7.4	4	2
	S.D.	3.65	3.71	3.54	3.7	2.7	1.41	0
Class size	Mean	31.94	36.27	31.09	36.62	32.6	19.33	18.5
	S.D.	14.51	15.63	16.38	12.98	14.4	2.73	7.78
Previous class size	Mean	31.65	35.64	29.82	36.31	34.2	20	18
	S.D.	14.77	16.95	14.32	14.19	16.63	2.83	7.07
Previous number of failures	Mean	15.33	8.82	9.55	24	20.6	13.83	18
	S.D.	9.47	5.25	4.82	10.86	7.57	2.64	7.07
	N	48	11	11	13	5	6	2

Tab. 5.7: Assignment of teachers to class es in the sample

		Year					
School	Grade	4	5	6	7	8	9
1	1			A	A	A	
1	2	B	B	B	B	B	
1	3				C	C	
1	4		D	E			
2	1			F			F
2	3			G	H	H	H
3	1	n.a.	n.a.	n.a.	I	I	J
3	2	n.a.	n.a.	n.a.	K	K	I
3	3			n.a.	L	L	L
3	4		M	M			M
3	5	n.a.	n.a.	n.a.	N	M	O
3	6				O	O	
4	2						P
4	3				Q	Q	Q

Sample: All classes for which exam results for the same grade in the same school in the previous school year are available and for which at least one more class in the same school and same grade also fulfills the same requirement (N=48). The different letters represent different teachers. "n.a." indicates that observations from this class are included in the data, but the teacher identity could not be recovered.

Tab. 5.8: Effect of previous class' number of failures and previous class size on current share of repeaters at the class level (N=48)

	Number of failures				Previous class size			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
				Baseline Model				
Number of failures	$.47^{***}$	$.48^{***}$	$.63^{***}$	$.63^{***}$				
	.13	*.13*	*.17*	*.17*				
Previous class size					$.28^{**}$	$.27^{*}$	$.23^{*}$	.21
					.13	*.14*	*.13*	*.14*
Restricted R^2	.29	.35	.43	.5				
Unrestricted R^2	.44	.51	.57	.64	.35	.41	.47	.53
				Within schools				
Number of failures	$.48^{***}$	$.63^{***}$	$.72^{***}$	$.74^{***}$				
	.16	*.17*	*.21*	*.24*				
Previous class size					-.11	.21	-.1	.01
					.23	*.14*	*.24*	*.26*
Restricted R^2	.41	.35	.5	.56				
Unrestricted R^2	.51	.64	.62	.66	.41	.53	.5	.56
				Within year by school				
Number of failures	$.43^{**}$		$.66^{**}$					
	.19		*.28*					
Previous class size					-.07		-.04	
					.31		*.33*	
Restricted R^2	.56		.65					
Unrestricted R^2	.63		.72		.56		.65	
				Within grade by school				
Number of failures	$.84^{***}$		$.88^{***}$					
	.25		*.28*					
Previous class size					-.18		-.07	
					.28		*.3*	
Restricted R^2	.54		.6					
Unrestricted R^2	.65		.7		.55		.6	
Additional controls:								
Year dummies		✓		✓		✓		✓
Grade dummies			✓	✓			✓	✓

Sample: All classes for which exam results for the same grade in the same school in the previous school year are available and for which at least one more class in the same school and same grade also fulfills the same requirement (N=48).

Tab. 5.9: Coefficient estimates for the share of repeating classmates: controlling for class size (rather than the number of non-repeaters)

	Specifaction							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Model 5.2							
Share of repeaters	-.88***	-.98***	-.31	-.3	-.47**	-.46**	-.35*	-.34**
	.25	*.26*	*.2*	*.19*	*.2*	*.21*	*.2*	*.17*
First trimester score					.58***	.6***	.61***	.62***
					.04	*.04*	*.03*	*.03*
	Model 5.4							
Share of repeaters	-.38*	-.41*			-.49***	-.52***		
	.21	*.21*			*.18*	*.18*		
First trimester score					.63***	.63***		
					.03	*.03*		
Additional controls:								
First trimester score					✓	✓	✓	✓
Year dummies		✓		✓		✓		✓
Grade dummies			✓	✓			✓	✓

Sample: All regressions control for class size as well as individual retention status. Sample: Observations for all students from classes for which results for the same grade in the same school in the previous school year are available and for which at least one more class in the same school and same grade also fulfills the same requirement; individual observations are dropped for all students form whom first or third trimester total exam results are missing (N=1331). Standard errors of coefficient estimates are given in italic; asterisks indicate statistical significance at the 1-, 5-, and 10-percent significance level, respectively. Standard errors are clustered at the class level.

Chapter 6

General appendix

6.1 Questionnaire for a household survey conducted in a Togolese community - First wave (2008)

Enquête auprès des ménages du canton de Tomégbé, Togo

Octobre 2008

Effectué par l'Université de Hannover, Allemagne
(Institut des recherches sur le travail)
en collaboration avec l'ONG ASMERADE

Concession	
Ménage	

Interviewer	

Introduction

«Bonjour/bonsoir Monsieur/Madame. Permettez-moi d'user un peu de votre temps. En effet, je m'appelle …………..et je travaille dans le cadre d'un projet de recherche concernant Tomegbé, initié par l'Université Allemande de Hannover en collaboration avec l'ONG ASMERADE. Ces recherches permettront d'évaluer des projets de développement (surtout dans le domaine de l'éducation) dans le futur. Pour une bonne réalisation de cette recherche, nous avons de ce fait, besoin de certaines informations qui certes font parties de votre vie privée mais qui sont nécessaires pour ce projet. Des résultats vous seront publiés après l'analyse des données par l'Université de Hannover en Allemagne qui sera l'administrateur de ces données. Rassurez-vous que ces informations que vous nous donnerez resterons confidentielles et n'auront aucun impact négatif sur vous.»

Codes commun à toutes questions:

Ne sait pas……………………888
Refuse………………………..999

Rapport sur des visites chez le ménage:

Visite No.	Interview effectué? Quelles parties?	Langage principale de l'interview

Section 1: Liste de ménage **Page 1**

L'ENQUETÉ: RÉSPONSABLE DU MÉNAGE

CODE D'IDENTIFICATION	1	2		3	4			5	5a	6	7					8 >>
	SEXE Mascu-lin.….1 Fémi-nin.…2	Quel est le nom de ……..?		Lien de parenté avec le responsable Responsable..................1 Épouse ou mari….........2 Enfant du responsable...3 Petit-enfant ....4 Père ou mère ....5 Soeur ou frère…………6 Nièce ou neveu……..…7 Beau-fils/belle-fille..…..8 Beau-frère/belle-soeur...9 Beau-père/belle-mère..10 Autres parents du respon-sable son épouse/mari..11 Doméstique du ménage ou ses parents………...12 Autre personnes non-apparentée…………..13	Quelle est la date de naissance de …(NOM)..?			AGE DE (NOM)	PRÉ-SENT PEN-DANT L'IN-TER-VIEW Oui…1 Non..2	Quelle est la situation de famille actuelle de (NOM)? Est-il (ou elle) actuellement… Marrié/e……...1 Célibataire…....2 (>> Q.8) Divorcé/e……..3 (>> Q.8) Veuf/veuve…...4 (>> Q.8) Jamais marié/e.6 (>> Q.8)	Je voudrais savoir où l'épouse/ les épouses/le mari de …(NOM)… habitent. RECOPIER LE CODE D'ID. DE CHAQUE ÉPOUSE/MARI QUI HABITE DANS LE MÉNAGE METTRE "HDM" POUR CHAQUE ÉPOUSE/MARI QUI HABITE DANS UN AUTRE MÉNAGE					Quelle est la religion* de ...(NOM)…? Catholique…1 Protestante..2 Méthodiste..3 Pentécôtiste.4 Christianisme célèlste……5 Islame………6 Animisme….7 Autre (préciser)…8
		PRÉNOM	NOM DE LA FAMILLE		JR.	MO.	AN.				1ÈR/E	2ÈME	3ÈME	4ÈME	5ÈME	
1																>>
2																
3																
4																
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6																
7																
8																
9																
10																
11																
12																
13																
14																

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concession ☐ ménage ☐

Séction 2: Éducation
Partie A: Scolarité

Page 2

CODE D'IDENTIFICATION	9	10	11	12	13	14	15	16		16(a)
	(NOM). à quelle ethnie appartient-il (elle)? Akposso de Litimé…1 Autre Akposso……..2 Ewe…………………..3 Akebou………………4 Kabye…..…………….5 Kotokoli..…………….6 Kambola…..………….7 Buem…………………8 Autre (préciser)…………9	CODE D'ID. DU PÈRE DE ..(NOM).. METTRE "HDM" SI LE PÈRE DE …(NOM)… HABITE DANS UN AUTRE MÉNAGE. METTRE "MORT" S'IL N'EST PLUS VIVANT	CODE D'ID. DE LA MÈRE DE ..(NOM).. METTRE "HDM" SI LA MÈRE DE …(NOM)… HABITE DANS UN AUTRE MÉNAGE. METTRE "MORT" SI ELLE N'EST PLUS VIVANTE	…(NOM)..dé-tient-il une des positions tradi-tionelles sui-vantes? Chef du village…….1 Notable……..2 Chef du quartier…...3 Chef coutumier…4 Gongonneur…5 Autre (préc.).6 Aucune posi-tion trad. …..7	…(NOM).. est-il (elle) né(e) dans le canton de To-mégbé? Oui........1 Non........2 (>> Q.15)	…(NOM).. a-t-il (elle) toujours habité à Tomegbé? Oui........1 (>> Pro-chain membre) Non........2	…(NOM).. a-t-il (elle) résidé ailleurs pendant plus d'un ans? Oui........1 Non........2 (>> Pro-chain membre)	Depuis combien de temps (NOM) habite-t-il (elle) à Tomegbé? ANS	MOIS	SI .(NOM). A MOINS DE 6 ANS Qui garde (NOM) pendant la journée, d'habitude? NOTER CODE D'ID SI PAS DANS LE MÉNAGE: La mère……..31 Autre adulte feminin….…..32 Le père……...33 Autre adulte masculin……..34 Soeur/frère.....35 Autre enfant..36 Autre (préc.).37
1										
2										
3										
4										
5										
6										
7										
8										
9										
10										
11										
12										
13										
14										

CODE D'IDENTIFICATION	17	18	19 >>
	…(NOM)… est-il instruit? Oui ...1 Non ....2 (>> pro-chaine membre)	Quel est le niveau d'étude de …(NOM)..? Jardin d'enf.JD CP1, CP2 CE1, CE2 CM1, CM2 6E, 5E 4E, 3E Seconde Première Terminale GHANA: primary school…..G1 Junior secon-dary school.G2 Senior secon-dary school.G3	Quelle a été le diplôme scolaire le plus élevé obtenu? Aucun ....1 CEPD…….2 BEPC…….3 BAC I……4 BAC II…..5 Autre (précis.)..6 GHANA: primary certificate.G1 A-level….G2 O-level….G3
1			>>
2			
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14			

Leibniz Universität Hannover

concession ______ ménage ______

Section 2: Éducation
B: Formation/université/apprentissage

Q.25-30: À REMPLIR POUR CHAQUE MEMBRE QUI A RÉPONDU "OUI" À LA QUESTION 17. Q.31-33: TOUS MEMBRES

Page 3

CODE D'IDENTIFICATION	20 …(NOM), combien d'années a-t-il (elle) fréquenté l'école? ANNÉS ÉCOLE PRIMAIRE	20 ANNÉS ÉCOLE SECONDAIRE	21 Quelle était la dernière école que …(NOM)… a fréquanté ou quelle école fréquante-il/elle? EPP Tomégbé…………1 École évangelique de T.2 École primaire catolique de Tomégbé (groupe A).3 École primaire catolique de Tomégbé (groupe B).4 CEG de Tomégbé……...5 Collège St. Bosco de T..6 Lycée de Tomégbé…….7 Autre (préciser)……….8	22 Est-ce que (NOM) a finit ses études à l'école? Oui …....1 (>> prochain membre) Non ....2 Retournera quelque jour……..3 (>> prochain membre)	23 Pouveu-vous me dire combien …(NOM). recoit pour le petit déjeuner lorsqu'il/elle part pour l'école? MONTAT	24 En plus de l'écollage et de fourniture de base de …(NOM)…, combien avez-vous dépensé pour ce début de rentré scolaire (c'est à dire jusqu'au 6 Octobre)? MONTANT
1						
2						
3						
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5						
6						
7						
8						
9						
10						
11						
12						
13						
14						

CODE D'IDENTIFICATION	25 …(NOM).. a-t-il (elle) fait une étude universitaire? Oui, terminé……1 Oui, est étudiant/e actuellement…..2 Non.......3 (>> Q.28)	26 Pendant combien de temps? ANN.	26 MOIS	27 Quelle a été le diplôme universitaire le plus élevé? BTS……….1 License…..2 Maîtrise….3 DEA……..4 DESS……..5 Doctorat..6 Aucun……7	28 …(NOM)… a-t-il (elle) suivi une formation professionnelle suplémentaire? Oui, terminé……1 Oui, est formé actuellement…..2 Non.......3 (>> Q.31)	29 Quel genre de formation? Bureautique...1 Informatique.2 Marketing.....3 Pedagogie…..4 Transit……..5 Électronique.6 Maintenace..7 Autre……....8 (Préciser)	30 Quelle a été la durée de la formation de …(NOM)..? ANN.	30 MOIS	31 …(NOM)… a-t-il/elle été apprenti? Oui, il/elle l'est actuellement……...1 Oui, il/elle n'est plus...2 Non, il/elle n'a jamais été…..……3 (>> prochain membre)	32 Pour quel genre de metier? Menuiserie…1 Maçonnerie..2 Couture…….3 Coiffure……4 Forgeron…..5 Mécanique…6 Peinture…...7 Autre….…..8 (Préciser)	33 Quelle a été la durée de l'apprentissage? ANN.	33 MOIS
1												
2												
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Section 3: Fécondité ENQUETÉES: TOUS FEMMES AGÉES 14 ANS ET PLUS

Page 4

	34	35	36	37
CODE D'IDEN-TIFICA-TION (RE-COPIER TOUT LES ID DES FEM-MES AGÉS 14 ANS ET PLUS)	Avez-vous donné naissance à un enfant? Oui…….1 Non.......2 (>> Q.36)	À combien d'enfants avez-vous donné naissance, même si quelques-uns parmi ces enfants ne sont plus vivants?	SI LA FEMME A MOINS DE 50 ANS	
			Êtes-vous enceinte présentement? Oui, depuis …….. mois (>> prochaine femme) Non.........20	Avez-vous été enceinte pendant les 12 derniers mois? Oui…….1 Non.......2 (>> prochaine femme)
		NOMBRE TOTAL DES ENFANTS	MOIS	

OBSERVATION DE L'INTERVIEWER; SECTIONS 1 À 3

concession ____ ménage ____

Section 4: Santé ENQUETÉES: LES PARENTS DES ENFANTS; SI ABSENT, DEMANDER LE RESPONSABLE DU MÉNAGE

Partie A: état de santé des enfants de moins de 17 ans ***RECOPIER LES CODES D'ID DES ENFANTS***

CODE D'IDENTIFICATION	44	45	46	47	48	49	50	51
	..(NOM)… a-t-il/elle souffert de blessures ou d'une maladie au cours des 3 derniers mois? Oui, diarrhée…..1 Oui, autre maladie……2 Oui, blessé..3 Non……….4 (>> Q.48) ACCEPTER PLUSIEURS RÉPONSES	Avez-vous été à l'hopi-tal ou chez un guérisseur traditionnel? Oui, hopital…....1 Oui, médécin traditionnel.2 Non……….3 (>> Q.47) ACCEPTER PLUSIEURS RÉPONSES	Combien de nuits a-t-il/elle passé à l'hopital ou chez le guérisseur traditionnel? NUITS	..(NOM). a-t-il/elle dormi sous un mousti-quaire la nuit dernière? Oui…….1 Non…..2	Avez-vous été à l'hopital ou chez un médécin traditionnel pour d'autres raisons que une maladie ou blessure de ….(NOM)…? Oui………1 Non……2	Combien avez-vous dépensé en argent au total en médica-ments ou des séjours à l'hopital/ chez le guérisseur pour …(NOM).. ou cours des 3 der-niers mois? MONT.	Est-ce que ...(NOM)…a jamais eu des vaccinations? Oui………1 Non……2 (>> prochain enfant)	Au cours des 12 dernèrs mois, a (NOM) eu des vaccinations? Oui, Polio………..…………..1 Oui, Fièvre jaune……………2 Oui, contre la méningite…..3 Oui, autres vaccins…………4 Non…………………………..5 ACCEPTER PLUSIEUR RÉPONSES

concession

Parti B: état de santé de tous les membres

Page 5

CODE D'IDENTIFICATION	57	58
	En général, comment évaluez-vous votre santé/la santé de …(NOM)…? INTERROGER LA MÈRE SI L'EN-FANT A MOINS DE 11 ANS. Très bonne…………1 Bonne………………2 Satisfaisante……….3 Faible………………4 Mauvaise…………..5	En comparaison de votre santé il y a un un, diriez-vous que votre santé aujourd' hui est… OU En comparaison de la santé de …(NOM). il y a un an, diriez-vous que la santé de …(NOM)… aujourd' hui est… Beaucoup mieux…1 Un peu mieux……2 Plus au moins la même……………..3 Un peu empirée…4 Beaucoup empirée…………..5
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ménage

Section 5: Emploi

Page 6

Partie A: Occupations au cours des 12 derniers mois

Je voudrais vous interroger sur vos activités pendant les 12 derniers mois, c'est-à-dire depuis Octobre 2007

CODE D'IDENTIFICATION	59	60	61	62	63		64			65												66	
	Au cours des 12 derniers mois, avez-vous exercé un travail pour lequel vous avez reçu du salaire ou d'autre rémunération? Oui….1 (>> Q.63) Non....2	Au cours des 12 derniers mois, avez-vous travaillé à votre compte ou travaillé comme propriétaire d'une petite entreprise non-agraire ou un commerce? Oui….1 (>> Q.63) Non....2	Au cours des 12 derniers mois, avez-vous travaillé à un champ ou jardin à vous ou avez-vous élevé des animaux à vous? Oui….1 (>> Q.63) Non....2	Au cours des 12 derniers mois, avez-vous travaillé dans un commerce, une entreprise ou à un champ à un autre membre de votre menage sans être payé? Oui….1 (>> Q.63) Non....2 (>> prochain membre)	Quelle est votre occupation principale? NOTER LE NOM ET CODE DE L'OCCUPATION RECOURIR À LA LISTE DES OCCUPATION SUR LA PROCHAINE PAGE		Avez-vous fait un autre travail que le travail principal au cours des 12 derniers mois? Quel genre de travail? POUR LES CODES DES OCCUPATIONS, RECOURIR À LA LISTE DES OCCUPATIONS. NOTER JUSQU'À 4 OCCUPATIONS EN ORDRE DE DEMANDE DE TEMPS.			CALENDRIER Je voudrais reconstituer ce que vous avez fait pendant la plupart des jours au 12 derniers mois. S'il vous plaît, indiquez ce que vous avez fait pendant la plupart des jours pour jaque mois - avez vous… Travaillé pendant la plupart des jours……………………..….1 Suivi une formation/allé à l'école/institution d'éducation pendant la plupart des jours……………………………….……2 Pas travaillé pendant la plupart des jours, étant disponible pour le travail supplémentaire………………….3 Pas travaillé pendant la plupart des jours, sans être disponible pour le travail supplémentaire…………..…4 Pas travaillé du tout, étant disponible pour le travail supplémentaire………………….5 Pas travaillé du tout, sans être disponible pour le travail supplémentaire…………..…6												Quelle est le moindre salaire que vous accepteriez pour travailler pour quelqu'un, c'est à dire cesser tout autre activités économiques?	
					OCC. PRINCIPALE	CODE	OCC.2 CODE	OCC.3 CODE	OCC.4 CODE	OCT.	NOV.	DEC.	JAN.	FEV.	MAR	AVR.	MAI	JUN.	JUL.	AOU.	SEP.	MONT.	UNITÉ D.TMP.
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Section 5, Emploi: Partie B

Je voudrais vous poser des questions sur votre occupation principale et sécondaire.

UNITÉ DE TEMPS	Mois……………….2	Jour…………………….4
Ans…………………….1	Semaine…………..3	Heure…………………..5

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ENQUETÉES: TOUS MEMBRES QUI ON RÉPONDU "OUI" À AU MOINS UNE DES Q. 59-62 (***RECOPIER ID***)

CODE D'IDENTIFICATION	67	68	69		70	71	72	73	74		75
	Faites-vous encore ce travail? Oui…...1 (>> Q.69) Non…..2	Pourquoi vous ne faites plus le même travail? Licenciement…...1 Tâche accompli..2 Travail saisonier.3 Clôture d'entreprise……………..4 Trouvé/préféré d'autre travail…..5 Autre...……......6 (Préciser) (>> prochain membre)	Combien avez-vous reçu comme salaire plus tous primes pour ce travail? NOTER "0" POUR "NE RECOIS PAS D'ARGENT"		Dans le cadre de ce travail, êtes-vous: Un employée payé/un salarié……………………..…1 Proprièteur d'un (petit) commerce ou entreprise non-agraire..(>> Q.73)….2 Collaborateur dans une entreprise familiale non-agraire…(>> Q.72)...3 Proprièteur d'une exploitation agraire…(>> Q.73)...4 Collaborateur dans une exploitation familiale agraire...(>> Q.72)..…….5	Pour qui avez-vous travaillé? C'est à dire pour… Une societé ou entreprise privée (y compris des apprentissages)..1 Une societé d'état ………………2 Le gouvernement ou l'armée….3 Une societé ou entreprise semi-publique……………………..4 Une ONG……..…………………..5 Une coopérative……………….6 Une institution international…7 Autre (préciser)…………………8	Pour ce travail, avez-vous reçu ou recevrez-vous une rémunération sous forme de… Aliments récolte ou animaux……..1 Logement gratuit ou subventionné..2 Vêtements………3 Transport gratuit ou subventionné..4 Autre forme…….5 Aucune…………..6 ACCEPTER PLUSIEURS RÉPONSES	Est-ce que l'endroit où vous travaillez se trouve à Tomegbé? Oui……....1 Non……..2 Ambulant.3	Avec quelle fréquence allez-vous à cet endroit? Par exemple, deux fois par jour, une fois par semaine?		Lorsque vous avez commencé ce travail, avez-vous signé un contrat où votre salaire était précisé? Oui…..1 Non....2 (>> Occup. secondaire ou prochain membre)
			MONTANT	UNITÉ DE TEMPS					FOIS	UNIT. TEMP.	

OCCUPATION PRINCIPALE

OCCUPATION SÉCONDAIRE

concession ______ ménage ______

LISTE DES OCCUPATIONS

Agriculture:
Culture……………………….1
Élevage……………………...2

Production des biens dérivés des cultures (p.e. huile/vin de palme, Sodabi, gari, tapioca, fruits/légumes secs, charbon…)…………………17

Production des biens d'origine animale (p.e. lait, oeufs, miel, charcutier...)..27

Petit commerces:
Alimentaires………………..3
Boutique……………………14
Autre commerce…………..5

Artisanat:
Menuisérie…………………..4
Couture……………………..15
Coiffure……………………..6

Prod. des balais, paniers…..7

Autre artisanat..…………..22

Services:

Mécanique…………………31

Répérature des chaussures.49

Construction………………34

Loisir (bar, video club…)..47

Moulin……………………..35

Transport (taxi moto,…).46

Communication…….……38

Enseignant…………………50
Médécin……………………60
Autre fonctionnaire………70

Section 5, Emploi
Partie C, Emploi d. temps

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ENQUETÉES: TOUS MEMBRES DE PLUS DE 5 ANS

*par exemple: se laver, se vètir, preparer le repas, manger, faire la vaisselle, faire la lessive, faire les courses, etc.

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concession: ____ ménage: ____

ID: ………	aube		matin							après-midi						nuit								
activité	04:00	05:00	06:00	07:00	08:00	09:00	10:00	11:00	12:00	13:00	14:00	15:00	16:00	17:00	18:00	19:00	20:00	21:00	22:00	23:00	minuit	01:00	02:00	03:00
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activités domestique personnel*																								
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éducation (partir pour l'école; devoirs..)																								
autre																								
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Section 5C (continué) Leibniz Universität Hannover ENQUETÉES: TOUS MEMBRES DE PLUS DE 5 ANS

*par exemple: se laver, se vètir, preparer le repas, manger, faire la vaisselle, faire la lessive, faire les courses, etc.

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concession: ménage:

ID: ………	aube		matin							après-midi						nuit								
activité	04:00	05:00	06:00	07:00	08:00	09:00	10:00	11:00	12:00	13:00	14:00	15:00	16:00	17:00	18:00	19:00	20:00	21:00	22:00	23:00	minuit	01:00	02:00	03:00
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gardant des enfants																								
ID: ………	04:00	05:00	06:00	07:00	08:00	09:00	10:00	11:00	12:00	13:00	14:00	15:00	16:00	17:00	18:00	19:00	20:00	21:00	22:00	23:00	minuit	01:00	02:00	03:00
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gardant des enfants																								
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ID: ………	04:00	05:00	06:00	07:00	08:00	09:00	10:00	11:00	12:00	13:00	14:00	15:00	16:00	17:00	18:00	19:00	20:00	21:00	22:00	23:00	minuit	01:00	02:00	03:00
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gardant des enfants																								

Section 6: Agriculture

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76. Au cours des 12 derniers mois, y a-t-il eu des membres de ce ménage qui ont fait l'élevage? Si oui, remplir le tableau.

Oui…….1 Non………..2 (>> Q.77)

Combien de …..ont les membres du ménage actuellement?

Moutons	Chèvres/cabris	Cochons/porcs	Volaille

Abeille	Escargots	Autre (préciser)

77. Au cours des 12 derniers mois, y a-t-il eu un membre de votre ménage qui a cultivé un champ?

Oui…….1 Non…....2 (>> prochaine séction)

UNITÉS DE SUPERFICIE:

Corde………….1
Hectare……….2

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Je voudrais lister touts les champs dont des membres de votre menage disposent (y compris des champs, plantations ou jardins qui ont été loué ou loué à quelqu'un, pris ou mis à bail, ou mis en jachère au cours des 12 derniers mois).

78	79		80		81	82	83	84	85
PERSONNE RÉSPONSABLE POUR LE CHAMP	PERSONNE INTERROGÉ	NUMÉRO DU CHAMP	Superficie du champ BIEN EXPLIQUER LA NOTION DU CHAMP DANS LE CADRE DE CETTE ENQUÊTE		Code d'ID de la personne qui possède le champ "30" POUR "PERSONNE DANS LE MÉNAGE N'EST PROPRIETAIRE"	Le champ a-t-il été loué à quelqu'un ou mis à bail au cours des 12 derniers mois? Non……..2 Oui, loué à quelqu'un..3 (>> proch. champ) Oui, mis à bail………4 (>> proch.)	Le champ a-t-il été loué ou pris à bail au cours des 12 derniers mois? Non…..2 Oui, loué…..3 Oui, pris à bail….4	Le champ a-t-il été mis en jachère au cours des 12 derniers mois? Oui……..1 (>> proch. champ) Non…….2	Combien de personnes qui ne sont pas des membres du ménage avez-vous employé à ce champ au cours des 12 derniers mois?
ID	ID		SUPERF.	UNITÉ	ID				
		1							
		2							
		3							
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		7							
		8							
		9							
		10							

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concession ménage

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NUMÉRO DU CHAMP	86														
	Quelle quantité de …….de ce champ avez vous recoltée ces 12 derniers mois? Café……………….1 Cacao…………….54 Maïs……………….8 Manioc……………26 Riz………………..4 Igname…………...13 Gingembre………..5 Banane……………62 Cola……………….3 Palmier……………47 Orange…………….6 Avoca……………..39 Banane plantain….9 Autre fruits………28 Legumes………….76 Teck……………..55 (pour le Teck une indication de recolte n'est pas necéssaire) Autre (préc.)...100 UNITÉS: Bols…………………11 Charges……………..22 Bassines…………….33 Kilos………………..44 Tubercules…………55														
	1			2			3			4			5		
	CULT. CODE	NOM.	UNI-TÉ	CULT. CODE	NOM.	UNI-TÉ	CULT. CODE	NOM.	UNI-TÉ	CULT. CODE	NOM.	UNI-TÉ	CULT. CODE	NOM.	UNI-TÉ
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OBSERVATION DE L'INTERVIEWER: SECTIONS 4 A 9

concession ______ ménage ______

Section 7: Emploi indépendant non-agricole

Page 12

87. Au cours des 12 derniers mois, y a-t-il eu un membre de votre ménage qui a fait un travail indépendant autre que l'agriculture ou l'élevage? Par exemple, quelqu'un qui a possédé une entreprise, un commerce, un industrie, quelqu'un qui a fait une profession libérale indépendant, ou quelqu'un qui a travaillé comme atrisan indépendant?

Oui……………1

Non…………..2

(>> Séction 8)

N	88	89	90	91	92			93	94	95	96				97
NUMÉRO D'ENTREPRISE	Quels sont les différentes entreprises possédés ou gérés par les membres de votre ménage au cours des 12 derniers mois?	Qui dans votre ménage est le mieux informé sur les activités de….. ?	CODE D'ID. DE LA PERSONNE INTERROGÉ	Quelle est l'activité prinicpale de cette entreprise?	Quelles biens ou services l'entreprise vend-t-elle? LES PLUS IMPORTANTS SELON LEUR RENDEMENT			Pendant quels mois cette entreprise fonctionnait-elle depuis Octobre 2007?	Votre entreprise, a-t-elle réalisé plus de ventes (efféctué plus de services) au cours des 12 derniers mois, que l'année precédente? Plus…………………1 Moins………………2 Pas de différence…3	Au cours des 12 derniers mois combien de personnes qui ne sont pas de membres du ménage ont travaillé ou aidé cette entreprise, y compris des apprentis?	Votre entreprise, de combien de ……possède-elle actuellement?				Les activités de votre entreprise, sont-ils effectué dans votre domicile? Oui………1 Non….….2
					1	2	3				groupe électrogèn.	motos	immeubles	terrains	
	NOM D'ENTREPRISE	CODE D'ID.		CODE	CODE	CODE	CODE	MOIS							
1															
2															
3															
4															
5															

S'IL Y EN A PLUS DE CINQ, DEMANDER D'INDIQUER LES CINQ ENTREPRISES LES PLUS IMPORTANTES POUR LE MÉNAGE.

ACTIVITÉS	
Maçonnerie………...12	Scierie…………..53
Menuiserie………….32	Bar/Vidéo club…28
Couture……………..25	Moto-taxi……….6
Petit commerce…….4	Vente…………...14
Artisannerie………..61	Autre..(préciser).70
Boulangerie…………21	
Coiffure………………7	
Mecanique auto et engins à deux roues..45	
Dépanage……………17	

BIENS:	SERVICES:
Meubles……………………….1	Construction…………..11
Habilles……………………….2	Transport……………...12
Oeuvres d'art…………………3	Dépannage…….……….13
Bois…...……………………….4	Reparation (engins..)....14
Planches……………………..5	Reparation (chaussure).15
Produits de consommation..6	Loisir……………………16
Briques………………………..7	Moulin………………….17
Produits alimentaires………8	Communication……….18
Essence………………..……..9	Traitement d. cheveux.19
Autres biens (préciser)……..10	Autres services………..20

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Séction 8: Type de logement

Maintenant je voudrais vous poser des questions sur votre logement. Par logement je voudrais dire toutes les pièces et les bâtiments séparés utilisés par les membres de votre ménage.

Quelles sont les differents structures occupées par votre ménage?

98. Type principal de domicile
Maison individuelle…………...1
Maison familiale……………..2
Plusieurs cases ou maisons…..3

99. Est-ce que cette maison/ces maisons/cet appartement/ces appartements fait/font partie d'une cour?
Oui……………………………...1
Non……………………………..2

100. Combien de pièces le ménage occupe-t-il, y compris les chambres à coucher et les salles de séjour?
NE PAS COMPTER LES SALLES DE BAIN, W.C., OU CUISINES

101. Est-ce que ce logement appartient à un membre de votre ménage?
Oui…………………………....1
Non…………………………...2

102. Est-ce que ce logement est loué en échange de biens, de services, ou d'argent?
Oui…………………………....1
Non…………………………...2

103. Quelle source d'approvisionnement votre ménage utilise-t-il pour la boisson?
Puits avec pompe………………….1
Puits sans pompe………………….2
Rivière, lac, source, marigot……..3
Eau de pluie……………………….4
Autre (préciser)……………………5

104. A combien de métres de votre logement se trouve ce …(MODE D'APPROVISIONNEMENT À NO. 103)…?

105. Ce ..(MODE D'APPROVISIONNEMENT À NO. 103).., est-il utilisé uniquement par votre ménage ou est-il utilisé par les autres ménages?
Ce ménage uniquement………....1
Partage…………………………..2

106. Quel est le principal mode d'approvisionnement en eau qu'utilise votre ménage pour faire la sessive et se baigner?
Puits avec pompe………………….1
Puits sans pompe………………….2
Rivière, lac, source, marigot……..3
Eau de pluie……………………….4
Autre (préciser)……………………5

107. Ce ...(MODE D'APPROVISIONNEMENT POUR LA LESSIVE ET LE BAIN)… est-il utilisé uniquement par votre ménage, ou est-il utilisé par d'autres ménages?
Ce ménage uniquement………..1
Partage…………………………..2

108. Quel type de W.C. est disponible pour votre ménage?
Chasse d'eau……………………….1
Latrine à fossé intérieur………2
Pas de W.C. ………………………3
Autre (Préciser)…………………..4

109. Ce W.C. sert-il uniquement à votre ménage, ou est-il accessible à d'autres ménages?
Ce ménage uniquement………..1
Partage…………………………..2

110. Quelle est la principale course d'éclairage de ce logement?
Electricité…………………………………….1
Bougies, torches, lampes à gaz ou petrole….2
Néant………………………………………….3

111. Quel est le combustible le plus souvent utilisé par le ménage pour la préparation des repas?
Bois…………………….1
Charbon………………..2
Gaz……………………..3
Autre (Préciser)………4

Q.112a-112d: À POSER AU RESPONSABLE DU MÉNAGE!

	112a
	Est-ce qu'il y a des membres de votre ménage qui possédent ....? Oui……...1 Non……..2 (>> Prochain lot)
Machine à coudre	
Ventilateur	
Radio simple	
Radio/cassette/ HiFi	
Video/TV	
Ordinateur	
Camera	
Téléphone portable	
Vélo	
Mobylette	
Voiture/autre véhicule	
Maison	
Terrain	
Compte d'épargne	

112b
Au cours des 12 derniers mois, y a-t-il eu des décès dans votre famille où vous avez engagé des dépenses de plus de 25 000CFA?
Oui……...1
Non……..2

112c
Vue vos ressources économiques, pourriez-vous me chiffrer ce que vous avez pu gagner cette année comme recette totale de vos activités économiques (c'est à dire depuis Octobre 2007 jusqu'à aujourd'hui)?
25 000 à 50 000………1
50 000 à 75 000………2
75 000 à 100 000……..3
100 000 à 125 000……4
125 000 à 150 000……5
150 000 et plus…………6

112d
Au cours des 12 derniers mois, avez-vous reçu d'argent d'un membre de votre famille qui habite…
À Lomé…………………………1
Dans une autre partie du Togo.2
Dans une autre pays Africaine..3
En Europe/en Amérique……….4
Non……………………………….5

concession

ménage

Leibniz Universität Hannover

Section 9: Test cognitive L'ENQUETÉES: TOUS ENFANTS AGÉS DE 3 À 14 ANS **Page 14**

C	113	114	115	116	117	118	119	120	121		122
O D E D ' I D E N T I F I C A T I O N	Quel jour de la semaine est-il aujourd'-hui? Juste….1 Faut….2	Quel est le nom du Prési-dant de la Répu-blique? Juste….1 Faut….2	SI L'EN-FANT A MOINS DE 6 ANS: Je vuex te mon-trer trois objets, et je vou-drais que tu me dis le nom de chaque objet. NOM-BRE SU	SI L'ENFANT A PLUS DE 5 ANS Je veux maint-tenant énon-cer trois ob-jets. Je vou-drais que tu les repètes. S'il te plaît, mémorise les objets, car je vais te demander de les répé-ter encore une fois dans quel-ques minuntes. ORANGE, MAISON, CHAT	Si j'ai 200 Francs et je te donne 50 Francs, combien me reste-t-il? Juste….1 Faut….2	Si un grand sac de riz coute 2000 Francs, com-bien coûtent deux grands sacs? Juste….1 Faut….2	Combien est-ce que quattre grand sacs coutent? Juste….1 Faut….2	Est-ce que tu te sou-viens des trois objets que je vient d'énoncer? Repètes-les, s'il te plaît. NOMBRE RÉPÉTÉ	Je veux te raconter une petite histoire et je te demande de mémoriser le contenu, car je te demanderai de répéter l'histoire après. NOTER LE CODE DE L'HISTOIRE RACONTÉE PLUS LE NOMBRE DES ÉLÉMENTS RÉPÉTÉS HIST.	ELEMENTS RÉPÉTÉS	LANGAGE PARLÉ PAR L'ENFANT PENDANT L'INTER-VIEW Ewe………1 Akposso…2 Autre (préciser)..3

Partie B: État de santé de tous enfants de moins de 11 ans et qui arrivent à se tenir debout ***RECOPIER LES CODES D'ID***

C	123	124	125		126	127
O D E D ' I D E N T I F I C A T I O N	SI L'ENFANTS A AU MOINS 3 ANS Comment tu t'es senti régulière-ment ces der-niers temps? MONTRER LES DÉSSINS À L'ENFANT Très bonne……1 Bonne…………2 Satisfaisante….3 Faible…………4 Mauvaise……..5	EXER-CISE 1 QUELLE EST LA DISTANCE QUI SÉ-PARE LE SOL DES DOIGTS DE L'ENFANT LORSQU'IL SE COURBE LES BRAS TENDUS VERS LE SOL? CENTI-MÈTRE	EXER-CISE 2 COMBIEN DE FOIS EN 2 FOIS 15 SÉC. L'ENFANT PEUT-IL SAUTILLER D'UN PIED À L'AUTRE SANS PER-DRE LA CADENCE? 1ÈRE FOIS	2ÈME FOIS	EXER-CISE 3 COMBIEN DE FOIS EN 1 MIN. L'ENFANT TOUCHE LE SOL AVEC L'AUTRE PIED LOR-SQU'IL NE SE TIEND QUE SUR UN PIED? FOIS	TAILLE DE L'EN-FANT CENTI-MÈTRE

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concession ☐ ménage ☐

6.2 Questionnaire for a household survey conducted in a Togolese community - Second wave (2011)

Enquête auprès des ménages du canton de Tomégbé, Togo
Janvier 2011

Effectué par l'Université de Hannover, Allemagne
(Institut des recherches sur le travail)
en collaboration avec l'ONG ASMERADE

Introduction: *«Bonjour/bonsoir Monsieur/Madame. Permettez-moi d'user un peu de votre temps. En effet, je m'appelle …………..et je travaille dans le cadre d'un projet de recherche concernant Tomegbé, initié par l'Université Allemande de Hannover en collaboration avec l'ONG ASMERADE. Ces recherches permettront d'évaluer des projets de développement (surtout dans le domaine de l'éducation) dans le futur. Pour une bonne réalisation de cette recherche, nous avons de ce fait, besoin de certaines informations qui certes font parties de votre vie privée mais qui sont nécessaires pour ce projet. Des résultats vous seront publiés après l'analyse des données par l'Université de Hannover en Allemagne qui sera l'administrateur de ces données. Rassurez-vous que ces informations que vous nous donnerez resterons confidentielles et n'auront aucun impact négatif sur vous.»*

Langage principale de l'interview: ______________________

Rapport sur des visites chez le ménage:

Visite No.	Date / heures

Concession (si inconnu: "n.a.") _________ Ménage (si inconnu: "n.a.") _________ Interviewer ☐

Numéros des concessions les plus proches: 1 ☐ 2 ☐ Quartier ______________

C1) Quelle est la source d'origine du numéro de concession et du numéro de ménage?
- Les numéros correspondent à un bâtiment dessiné est marqué sur le plan de village ☐ > C4
- Le ménage ne peut pas être lié avec un bâtiment dessiné est marqué sur le plan de village ☐

C2) Le responsable du ménage, affirme-t-il d'avoir été interviewé lors de l'enquête en Octobre 2008?
- Oui, et il a residé dans ce même bâtiment ☐ > Q.1
- Oui, mais il a demenagé depuis Octobre 2008 ☐
- Non, et il a residé dans ce même bâtiment en Octobre 2008 ☐ > Q.1
- Non, et il n'a pas residé dans ce même bâtiment en Octobre 2008 ☐

C3) Le responsable du ménage, où a-t-il réside en Octobre 2008?
- Hors de Tomégbé ☐ > Q.1
- À Tomégbé …………… { quartier ☐ concession ☐ ménage ☐ > Q.1
  - Responsable de l'ancien ménage __________________________

C4) Qui reside dans ce ménage?
- Même responsable du ménage qu'en 2008 ☐ > Q.1
- Même famille qu'en 2008, responsable a changé ☐
- Autre famille qu'en 2008 ☐
- Le bâtiment est inhabité ☐

C5) Le responsable du ménage en Octobre 2008, où habite-il présentement? Selon…

	ne con-vient pas	Hors de Tomégbé	À Tomégbé: quartier	À Tomégbé: responsable du nouveau ménage
Présent responsable du ménage				
Voisin 1				
Voisin 2				

> reprendre à C2 (!) si la réponse à C4 a été "Autre famille qu'en 2008", sinon > Q.1

Codes commun à toutes questions: Ne sait pas = "-1" Refuse = "-2"

Section 1: Liste de ménage

Page 1

L'ENQUETÉ: RÉSPONSABLE DU MÉNAGE

C O D E D ' I D E N T I F I C A T I O N	1 S E X E M ..1 F ..2	2 Quel est le nom de ……..? APRÉS AVOIR NOTÉ LES NOMS DES PRESENTS MEMBRES DU MÉNAGE, COPIER LES NOMS DE CEUX QUI ONT ÉTÉ PRESENT EN 2008 MAIS QUI NE FONT PLUS PARTIE DU MÉNAGE ENFIN, NOTER LES NOMS DE TOUS AUTRES ANCIENS MEMBRES DU MÉNAGE QUI ONT FRÉQUANTÉ UNE ÉCOLE À TOMÉGBÉ PENDANT LES DERNIERS DIX ANNÉES NOTER TOUT LES PRÉNOMS ET SOULIGNER LE PRÉNOM USUEL NOM DE LA FAMILLE	PRÉNOMS	3 Lien de parenté avec le responsable Responsable.........1 Épouse ou mari….2 Enfant du resp. ...3 Petit-enfant ....4 Père ou mère ....5 Soeur ou frère……6 Nièce ou neveu…7 Beau-fils/b.-fille...8 Beau-frère/b.-s. ..9 Beau-père/b.-m. .10 Autres parentés du responsable ou son épouse/mari..11 Doméstique du mén. ou ses parents…..12 Autre personnes non-apparentée..13	4 Quelle est la date de naissance de …(NOM)..? JR.	MO.	AN.	5 AGE	6 Jour de la semaine que .(NOM). est né/e Lundi..1 Mardi..2 Mercredi…….3 Jeudi…4 Vendredi…….5 Samedi …..6 Dimanche……7	7 Code d'identification en 2008 … ID Pas trouvé en 2008 … 30 (>12)	8 ..(NOM).. fait-il (elle) toujours partie du ménage? Oui…1 (>13) Non…2	9 ..(NOM).. pourquoi ne fait-il (elle) plus partie du ménage? Ancien/ancienne mari/ épouse du/de la responsable: divorcé/separé..1 Parti(e) pour constituer un propre ménage…...2 Parti(e) pour fréquenter l'école/formation ailleurs..3 Parti(e) pour travailler ailleurs (transitoirement)..4 Parti(e) pour travailler ailleurs (permanent/ retour imprévesible)…..5 Parit(e) pour vivre av. autres membres de la famille..6 Décédé(e) …………………..7 Autre raison (préciser)…....8	10 ..(NOM).., où habite-il (elle) présentement? Ménage à Tomégbé, nouvellement constitué…1 Autre ménage à Tomégbé …2 + NOM DU RESPONSABLE (SI CONNU) À Lomé……..3 (>1) Dans une autre partie du Togo………..4 (>1) Ghana………..5 (>1) Autre pays Afric. ..6 (>1) Hors de l'Afrique..7 (>1)	11 .(NOM)., dans quel quartier habite-il (elle)? (>1)
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12														
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concession ____ ménage ____

Séction 2: Éducation

Page 2

CODE D'IDENTIFICATION	12		13	14			15	16	17	18	19	20
	..(NOM)…, pourquoi est-ce qu'il (elle) n'a pas été présent en 2008? Née après……………………………1 Nouvelle épouse/nouveau mari venant d'un autre ménage à Tomégbé ..2 + NOM D. RESPONS. (SI CON.) + QUARTIER Nouvelle épouse/nouveau mari venant d'ailleurs……………………….3 Autre personne venant d'un autre ménage à Tomégbé….4 + NOM DU RESPONSABLE (SI CONNU) + QUARTIER Autre personne venant d'ailleurs…5 Était absent pour travailler ailleurs..6 Était absent pour fréquenter l'école ailleurs………………………………..7 Autre (préciser)….8	QUARTIER	Quelle est la situation de famille actuelle de (NOM)? Est-il (ou elle) actuellement… Marrié/e……..1 Célibataire…..2 (>15) Divorcé/e……3 (>15) Veuf/veuve….4 (>15) Jamais marié/e..6 (>15)	Je voudrais savoir où l'épouse/les épouses/le mari de ..(NOM).. habitent. RECOPIER LE CODE D'ID. DE CHAQUE ÉPOUSE/MARI QUI HABITE DANS LE MÉNAGE hors du ménage… 30 1	2	3	Quelle est la religion de ...(NOM)…? Catholique…1 Protestante..2 Méthodiste..3 Pentécôtiste.4 Christianisme célèlste……5 Islame………6 Animisme….7 Boudisme…..8 Croix Blan…9 Ass.d. Dieu..10 Autre………11 Aucune……12	(NOM).. À quelle ethnie appartient-il (elle)? Akposso de Litimé…1 Autr. Akp…2 Ewe………..3 Akebou……4 Kabye…….5 Kotokoli..6 Kambola…7 Buem……8 Zamanana..9 Bassila……10 Losso…….11 Kante……12 Bassu…….13 Autre…….14	CODE D'ID DU PÈRE DE …. hors du ménage …30 mort …80	CODE D'ID DE LA MÈRE DE …. hors du ménage …30 morte …80	ENQUETÉES: TOUS FEMMES AGÉES 14 ANS ET PLUS À combien d'enfants avez-vous donné naissance, même si quelques-uns parmi ces enfants ne sont plus vivants? NOMBRE TOT. DES ENFANTS	SI LA FEMME A MOINS DE 50 ANS Êtes-vous enceinte présentement? Oui, depuis …….. mois Oui (mois inconnu).15 Non.......20 MOIS
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14												

concession ménage

CODE D'IDENTIFICATION	21	22	23	24	25		26
	(NOM). est-il instruit? Oui ...1 Non ..2 (>36)	Dans quelle année scolaire, ... (NOM) …. a-t-il /elle commencé ses études?	Quel est le niveau d'étude de …(NOM)..? JD (>30) CP1, CP2 CE1, CE2 CM1, CM2 6E, 5E 4E, 3E Seconde Première Terminale GHANA: primary school…..G1 Junior secondary school.G2 Senior secondary school.G3	Quelle a été le diplôme scolaire le plus élevé obtenu? Aucun....1 CEPD…2 BEPC….3 BAC I….4 BAC II...5 Autre…6 GHANA: primary certificate…..31 A-level.32 O-level.33	(NOM), combien d'années a-t-il (elle) fréquenté l'école? ANNÉES ABSOLUS PRIM.	SEC.	Quelle était la dernière école que .(NOM). a fréquanté/quelle école fréquante-il/elle? EPP Tom..1 (>28) E. évang…2 (>28) EPC (A)…3 (>28) EPC (B)…4 (>28) EPP Akloa.5 (>28) EPP Maflo.6 (>28) EPC Maflo.7 (>28) EP Badou..7 (>28) CEG Tom. …..8 St. Bosco ……..9 Lycée Tom…..10 CEG priv. Aklo.11 CEG Maflo……12 E.sec. Badou…13 Autre…………..14
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Section 3: Emploi **Page 3**

CODE D'IDENTIFICATION	27	28	29	30	31	32	33	34	35	36	37	38	
		ENQUETÉS: LES PARENTS DE TOUT ENFANTS DE 5 A 17 ANS											
	Quelle était l'école primaire que .(NOM). a fréquanté? EPP Tomégbé..1 École évangelique de Tom. ...2 EPC T. (gr. A)…3 EPC T. (gr. B)…4 EPP Akloa……..5 EP Badou….……6 Autre hors de T..7	Das quelle école primaire avez-vous initialement souhaité inscire votre enfant? L'EP qu'il/elle fréquent/e; a fréquanté…1..(>30) Autre EP… ..EPP Tom….2 ..EP evang….3 ..EP cath…….4 ..EPP Akloa..5 ..EP hors de Tom. …..6	Pour quel raison l'enfant n'a-t-il pas pu fréquenter cette école preférée? Classe plein…1 Pas pu payer les frais……….2 Prof./Directeur énoncait que l'enfant n'est pas prêt/trop jeune..3 Autre (précis.)..4	Est-ce que (NOM) a finit ses études scolaires? Oui …....1 Non ....2 (>32) Retournera quelque jour……..3	Pendant/ à la fin de quelle année scolaire, …(NOM).. a-t-il/elle finit/sus-pendu ses études à l'école? (>34)	Pouvez-vous me dire combien .(NOM). recoit pour le petit déjeuner lorsqu'il/elle part pour l'école? (en moyen, d'habitude)	En plus de l'écollage et de fourniture de base de …(NOM)…, combien avez-vous dépensé pour ce début de rentré scolaire (c'est à dire jusqu'au 13 Septembre 2010)? *>prochain membre*	…(NOM).. a-t-il (elle) fait une étude universitaire? Oui, terminé……1 Oui, est étudiant/e actuellement…..2 Oui, abandonné….3 Non.......4	(NOM)… a-t-il (elle) suivi une formation professionnelle suplémentaire*? Oui, terminé……1 Oui, est formé/e actuellement…..2 Non.......3	…(NOM)… a-t-il/elle été apprenti? Oui, il/elle l'est actuellement……..1 Oui, il/elle n'est plus...2 Non, il/elle n'a jamais été….……3 (>21)	Pour quel genre de metier? Menuiserie…1 Maçonnerie..2 Couture…….3 Coiffure……4 Forgeron…..5 Mécanique…6 Peinture…...7 Chauffeur….8 Boulanger….9 Cordonnier.10 Autre………11	Quelle a été la durée de l'apprentissage?	
						MONTANT	MONTANT					ANN.	MOIS
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7													
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(*) Par exemple: bureautique, informatique, marketing, pedagogie, transit, électronique, maintenance…

concession

ménage

ENQUETÉS: TOUS DE 7 ANS ET PLUS

CODE D'IDENTIFICATION	39			
	Au cours des 12 derniers mois, avez-vous… …exercé un travail pour lequel vous avez recu du salaire ou d'autre rémunération? ……………..1 ...travaillé à votre compte ou travaillé comme propriétaire d'une petite entreprise non-agraire ou un commerce?..2 ...travaillé à un champ ou jardin à vous ou avez-vous élevé des animaux à vous?..3 ...travaillé dans un commerce, une entreprise ou à un champ à un autre membre de votre menage sans être payé?...4 …aucune des réponses convient. …5 (>64)			
	ACCEPTER JUSQU'À 4 RÉPONSES			
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Section 3 (cont.) **Page 4**

Je voudrais vous interroger sur vos activités pendant les 12 derniers mois, c'est-à-dire depuis Janvier 2010

> OCCUPATION 2

CODE D'IDENTIFICATION	40	41		42	43			44				45	46	47	48		49			50	51	
	Quelle est votre occupation principale? POUR LES CODES DES OCCUPATIONS, RECOURIR À LA LISTE DES OCCUPATIONS. CETTE SECTION PERMET DE NOTER JUSQU'À 4 OCCUPATIONS. COMMENCER AVEC CELLE QUI DEMANDE LE PLUS DU TEMPS	Combien avez-vous reçu comme salaire plus tous primes pour ce travail? Alternative: Quel est votre revenu pour ce travail? "ne fais plus" … -3 (>50) NOTER "0" POUR "NE GAGNE RIEN"		Dans le cadre de ce travail, êtes-vous: Un employée payé/un salarié……………………..….1 Propriètaire d'un (petit) commerce ou entreprise non-agraire..(>44)……..…2 Collaborateur dans une entreprise familiale non-agraire… ...ID-EP (>47) …propriètaire n'est pas un membre du ménage..3 (>47) Propriètaire d'une exploitation agraire…(>47)……...4 Collaborateur dans une exploitation familiale agraire...(>47)……..……..5	Pour ce travail, avez-vous reçu ou recevrez-vous une rémunération au-delà de l'argent sous forme de… Aliments récolte ou animaux……..1 Logement gratuit ou subventionné..2 Vêtements………3 Transport gratuit ou subventionné..4 Nourriture…..….5 Autre forme…….6 Aucune………......7 (>47) ACCEPTER JUSQU'À 3 RÉPONSES			De quel genre d'entreprise s'agit-il?	ACCEPTER PLUSIEURS REPONSES			Pendant combien de mois cette entreprise fonctionnait-elle depuis Janvier 2010?	Au cours des 12 derniers mois combien de personnes qui ne sont pas des membres du ménage ont travaillé ou aidé cette entreprise, y compris des apprentis?	Est-ce que l'endroit où vous travaillez se trouve à Tomegbé? Oui, dans mon domicile……….1 À Tomégbé mais pas dans mon domicile…2 Ambulant.3 Ailleurs….4	Avec quelle fréquence allez-vous à cet endroit? Par exemple, deux fois par jour, une fois par semaine?		Pendant combien des heures avez-vous fait ce travail cette dernière/ ce dernier…			Avez-vous fait un autre travail que le travail principal au cours des 12 derniers mois? Quel genre de travail? Oui…CODE Non……..-3 (>39)	Combien avez-vous reçu comme salaire plus tous primes pour ce travail? Alternative: Quel est votre revenu pour ce travail? "ne fais plus" … -3 (>50) NOTER "0" POUR "NE GAGNE RIEN"	
	OCC.1 CODE	MONTANT	UNI. D. TEMPS					IDENTREPRISE	CODE 1	CODE 2	CODE 3	MOIS			FOIS	UNIT. TEMP.	jeudi	mardi	semaine	OCC.2 CODE	MONTANT	UNI. D. TEMPS
1								E01														
2								E02														
3								E03														
4								E04														
5								E05														
6								E06														
7								E07														
8								E08														
9								E09														
10								E10														
11								E11														
12								E12														
13								E13														
14								E14														

UNITÉ DE TEMPS	Ans…..1	Mois….2	Semaine…3	Jour...4	Heure…..5

concession ____ ménage ____

Section 3 (cont.)

Page 5

> OCCUPAT. 3 | > OCCUPAT. 4

CODE IDENTIFICATION	52	53	54	55	56	57	58	59	60	61	62	63	64
	Dans le cadre de ce travail, êtes-vous: Employée payé/un salarié…………1 Propriètaire (petit) commerce/entreprise non-agraire….2 (>54) Collaborateur entreprise fam. non-agraire… ...ID-EP (>57) …propriètaire n'est pas un membre du ménage…...3 (>57) Propriètaire exploit. agraire….(>57)…..4 Collaborateur exploit. familiale agraire ...(>57)……...……..5	Pour ce travail, avez-vous reçu ou recevrez-vous une rémunération au-de-là de l'argent sous forme de… Aliments récolte ou animaux……..1 Logement gratuit ou subventionné..2 Vêtements………3 Transport gratuit ou subventionné..4 Nourriture…..…5 Autre forme……6 Aucune………….7 (>57) ACCEPTER JUSQU'À 3 RÉPONSES	De quel genre d'entreprise s'agit-il? ACCEPTER PLUSIEURS REPONSES ID ENTREPRISE | CODE 1 | CODE 2 | CODE 3	Pendant combien de mois cette entre-prise fonc-tionnait-elle depuis Janvier 2010? MOIS	Au cours des 12 der-niers mois combien de person-nes qui ne sont pas de membres du ménage ont travaillé ou aidé cette entre-prise, y compris des apprentis?	Est-ce que l'endroit où vous tra-vaillez se trouve à Tomegbé? Oui, dans mon domi-cile……1 À Tomég-bé mais pas dans mon domicile..2 Ambulant.3 Ailleurs….4	Avec quelle fréquence allez-vous à cet endroit? Par exem-ple, deux fois par jour, une fois par semaine? FOIS | UNITÉ D. TEMP.	Pendant com-bien des heures avez-vous fait ce travail cette dernière/ ce dernier… jeudi | mardi | semaine	OCCUPATION 3 CODE	Pendant com-bien des heures avez-vous fait ce travail cette dernière/ ce dernier… "ne fais plus" … -3 jeudi | mardi | semaine	OCCUPATION 4 CODE	Pendant com-bien des heures avez-vous fait ce travail cette dernière/ ce dernier… "ne fais plus" … -3 jeudi | mardi | semaine	CALENDRIER Je voudrais reconstituer ce que vous avez fait pendant la plupart des jours au 12 derniers mois. S'il vous plaît, indiquez ce que vous avez fait pendant la plupart des jours pour chaque mois - avez vous… Travaillé pendant la plupart des jours……………..1 Suivi une formation/allé à l'école/institution d'éducation pendant la plupart des jours………..2 Pas travaillé pendant la plupart des jours, étant disponible pour le travail supplémentaire……..3 Pas travaillé pendant la plupart des jours, sans être disponible pour le travail supplémentaire…4 Pas travaillé du tout, étant disponible pour le travail supplémentaire………5 Pas travaillé du tout, sans être disponible pour le travail supplémentaire…6 JAN | FEV | MAR | AVR | MAI | JUN | JUL | AOU | SEP | OCT | NOV | DEC
1			E15										
2			E16										
3			E17										
4			E18										
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11			E25										
12			E26										
13			E27										
14			E28										

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UNITÉ DE TEMPS	Ans…..1	Mois……..2	Semaine…3	Jour...4	Heure……..5

concession ☐ ménage ☐

Qu'est-ce que vous avez fait la plupart du temps du… (65)

allé à l'école……1
étudié/fait des devoirs à la maison……2
travaillé comm apprenti hors du ménage……3
fait le menage……4
travaillé au champ……5
trav. dans un magasin/atelier/.. hors du men./amulant...6
travaillé dans le menage……7
fait des courses……8
rien……9
autre……10

66: Quelle est le moindre salaire que vous accepteriez pour travailler pour quelqu'un, c'est à dire cesser tout autre activités économiques?

CODE D'IDENTIFICATION	matin de ce mardi dernier	pendant combien d'heures?	activité secondaire? non.-3	pendant combien des heures gardé d. enfants d. moins d. 6 ans? tout le matin.12	après-midi/soir de ce mardi dernier	pendant combien d'heures?	activité secondaire? non.-3	pendant combien des heures gardé d. enfants d. moins d. 6 ans? tout le soir..12	matin de ce jeudi dernier	pendant combien d'heures?	activité secondaire? non.-3	pendant combien des heures gardé d. enfants d. moins d. 6 ans? tout le matin.12	après-midi/soir de ce jeudi dernier	pendant combien d'heures?	activité secondaire? non.-3	pendant combien des heures gardé d. enfants d. moins d. 6 ans? tout le soir..12	MONT.	UNITÉ D.TMP.
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OBSERVATIONS

UNITÉ DE TEMPS	Mois……..2	Jour...4
Ans……………..1	Semaine…..3	Heure.5

concession ☐ ménage ☐

Section 4: Connaissances

Page 7

CODE D'IDENTIFICATION	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87
						Enquetés: membres du ménage de plus de 14 ans															Avez-vous suivi un cours d'al-phabét-sation? Oui……1 Non…..2
1																					
2																					
3																					
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OBSERVATIONS

concession

ménage

Section 6: Agriculture | Section 7: Recoltes | **Page 8**

(92) Liste de touts les champs que des membres de votre ménage ont cultivé au cours des 12 derniers mois

PERSONNE RÉSPONSABLE POUR LES CHAMPS	PERSONNE INTERROGÉ	SUPERFICIE: *aucun champ* [ ] (>95) — UNITÉS DE SUPERFICIE: Corde…1, Hectare…2								
ID	ID		CH 1	CH 2	CH 3	CH 4	CH 5	CH 6	CH 7	CH 8	TOTAL
		SUPERF.									
		UNITÉ									
		SUPERF.									
		UNITÉ									
		SUPERF.									
		UNITÉ									
		SUPERF.									
		UNITÉ									
		SUPERF.									
		UNITÉ									

(92a) Combien de ces champs appartiennent à vous? [ ]

(92b) La superficie total de ces champs: superf. [ ] unité [ ]

(93) Combien avez vous dépensé pour le métayage de tous vos champs au cours des 12 derniers mois? CFA [ ]

(94) Quel quantité de ….. avez vous recolté ces 12 derniers mois?

UNITÉS DES RECOLTES: Bols…1, Charges…2, Bassines…3, Kilos…4, Tubercules…5, Sacs…6, Regimes…7

	NOMBRE	UNITÉ	PERSONNE(S) RÉSPONSABLE(S) POUR LA CULTURE ID		
Café					
Cacao					
Maïs					
Manioc					
Riz					
Igname					
Gingembre					
Banane					
Cola					
Palmier					
Orange					
Avoca					
Banane plantain					
Autre fruits					
Legumes					

(95) Combien de ….. ont les membres du ménage actuellement?

Moutons
Chèvres/cabris
Cochons/porcs
Volaille

Séction 8: Type de logement: Maintenant je voudrais vous poser des questions sur votre logement. (Logement = toutes les pièces et les bâtiments séparés utilisés par les membres du ménage) ENQUÊTÉ: RÉSPONSABLE DU MÉNAGE

96. Est-ce que cette maison/ces mais:/cet appartement/ces appart. fait/font partie d'une cour? (Oui..1/Non..2) [ ]

97. Combien de pièces le mén. occupe-t-il, y compris les chambres à coucher et les salles de séjour? [ ]
NE PAS COMPTER LES SALLES DE BAIN, W.C., OU CUISINES

98. Est-ce que ce logement appartient à un membre de votre ménage? (Oui..1/Non..2) [ ]

99. Est-ce que ce logement est loué en échange de biens, de services, ou d'argent? (Oui..1/Non..2) [ ]

100. Quelle source d'approvisionnement votre ménage utilise-t-il pour la boisson?
- Puits avec pompe……….1 Eau de pluie…………………..………4
- Puits sans pompe……….2 Autre ………….……………………5 [ ]
- Rivière, lac, source, marigot..3

101. A combien de métres de votre logement se trouve ce (MODE D'APPROVISIONNEMENT À NO. 100)? [ ]

102. Quel est le principal mode d'approvisionnement en eau qu'utilise votre ménage pour faire la sessive et se baigner?
- Puits avec pompe……….1 Eau de pluie…………………..………4
- Puits sans pompe……….2 Autre ………….……………………5 [ ]
- Rivière, lac, source, marigot..3

103. Quel type de W.C. est disponible pour votre ménage?
- Chasse d'eau…………………1
- Latrine à fossé intérieur…...2
- Pas de W.C. …………………3
- Autre …………..…………….4 [ ]

104. Ce W.C. sert-il uniquement à votre ménage, ou est-il accessible à d'autres ménages?
- Ce ménage uniquement……..…..1
- Partage………………………..…..2 [ ]

105. Quelle est la principale source d'éclairage de ce logement?
- Electricité………………………………….……1
- Bougies, torches, lampes à gaz ou petrole….2 [ ]
- Néant………………………………………….3

106. Quel est le combustible le plus souvent utilisé par le ménage pour la préparation des repas?
- Bois…………………….…1
- Charbon………………..2 [ ]
- Gaz…………………….3
- Autre ………….……….4

concession [ ]
ménage [ ]

Section 9: Divers

ENQUETÉ: RESPONSABLE DU MÉNAGE

107. Les membres de votre ménage, combien de . possédent-ils?	
Machine à coudre	
Ventilateur	
Radio simple	
Radio/cassette/HiFi	
Video/TV	
Ordinateur	
Camera	
Téléphone portable	
Vélo	
Mobylette	
Voiture/autre véhicule	
Maison	
Terrain	
Livres	

109. Y-a-t-il des membres de votre famille qui travaillent dans une/plusieurs des écoles suivantes?	oui	non
EPP Tomégbé		
EP evangélique de Tomégbé		
EP catholique de Tomégbé		
EPP Akloa		

110. Quelle EP les enfants dans votre quartier fréquentent-ils normalement?	oui	non
EPP Tomégbé		
EP evangélique de Tomégbé		
EP catholique de Tomégbé		
EPP Akloa		

108. Vue vos ressources économiques, pourriez-vous me chif-frer ce que vous avez pu gagner cette année dernière comme recette totale de vos activités économiques (c'est à dire depuis Janv. 2010 jusqu'à aujourd'hui)?

25 000 à 50 000…..1
50 000 à 75 000…..2
75 000 à 100 000…..3
100 000 à 125 000…4
125 000 à 150 000.5
150 000 à 175000….6
175 000 à 200 000.7
200 000 et plus……8

111. Combien de temps faut-ils pour aller de votre maison jusqu'à…? MINUTES	
EPP Tomégbé	
EP evangélique de Tomégbé	
EP catholique de Tomégbé	
EPP Akloa	
Jardin d'enfants (ASMERADE)	

112. Je voudrais que vous mettez les EP de Tom.-Akloa en ordre de qualité. (plusieurs peuvent avoir le même rang)	
EPP Tomégbé	
EP evangélique de Tomégbé	
EP catholique de Tomégbé	
EPP Akloa	

113. Au cours des 12 derniers mois, avez-vous reçu d'argent d'un membre de votre famille qui habite…

À Lomé………..1 Dans une autre partie du Togo………..2
Europe/Amérique…4 Dans une autre pays Africaine………..3
Non…………………5

114. Jaimerais que vous m'indiquez combien de décès il y a eu dans votre famille (la famille nucléaire des membres de votre ménage) dans l'année..

2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010

Section 10: Opinions

Page 9

Enquetées: toutes femmes dans le ménage de plus de 16 ans

S'il vous plaît, quelle est votre évaluation des énonciations suivantes (questions 118–128):

Échelle: (1)="tout à fait d'accord", (5)="complètement déclinatoire"

CODE D'IDENTIFICATION	115	116	117	118	119	120	121	122	123	124	125	126	127	128
	Imaginez qu'un enfant de 2 à 6 ans est gardé par sa maman pendant la journée. Quel est le moindre age que cet enfant doit atteindre avant que sa maman ne peut faire un travail (au champ, dans un atélier/magasin, etc.)? <2 …………..1 >6 …………..7	Imaginez qu'un enfant de 2 à 6 ans est gardé pendant la journée par une autre personne responsable (dans un jardin d'enfants, à l'école, par un autre membre de la famille, etc.), à partir de quel âge de l'enfant sa maman peut-elle faire un travail? <2 …………..1 >6 …………..7	Jusqu'à quel âge est-il désavantageux pour le développement d'un enfant de 2 à 6 ans s'il n'est pas gardé par son maman régulièrement pendant la journée? <2 ……..1 >6 ……..7	Si j'ai un petit enfant (moins de 6 ans) j'essaye de passer du temps avec lui autant que possible.	Si j'ai un petit enfant je peux facilement trouver quelqu'un qui le garde pendant la journée.	Normalement, une femme ne doit pas travailler hors du ménage.	Normalement, une maman d'un enfant de moins de 6 ans ne doit pas travailler hors du ménage.	Nos enfants doivent impérativement fréquenter l'école.	Parfois les besoins de la famille sont plus importants que l'éducation de nos enfants.	Il est important que nos enfants ont une éducation préscholaire.	Il est désavantageux pour le développement d'un enfant de 3 à 5 ans s'il est gardé par un autre enfant plus agé.	Il est important dans ma vie que je surviens à mes besoins à travers de mon travail.	J'aime travailler.	Si j'avais l'occasion, j'aimerais travailler plus.
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concession ____ ménage ____

Section 10 (cont.) **Page 10**

Enquetés: tous membres du ménage de plus de 16 ans

CODE IDENTIFICATION	129	130	131	132	133	134	134a	135	136	137	138	139	141	142
	Avez-vous entendu parler de/ connaissez-vous l'association ASMERADE? Oui…..1 Non….2 (>132)	Pourriez-vous me nommer les projets entretenus par ASMERADE que vous connaissez? NOMBRE DE PROJ. CONNUS	Pensez-vous que la communauté profite de ce que ASMERADE fait? oui, la com. en profite.1 non, aucun impacte…2 non, desserve la com….3	ENQUETÉS: PARENTS DES ENFANT DE 2-6 ANS Avez vous fait des démarches pour inscrire votre enfant dans le jardin d'enfants créé par ASMERADE? Oui…1 (>134) Non……2	Pour quel raison? ne connait pas ce jardin d'enf..1 prefère d'envoyer l'enfant à l'école primaire………………2 prèfere impérativement d'avoir l'enfant à la maison……………3 le jardin d'enfants est trop loin d'ici…………………………4 qualité incertain de l'encadrement dans ce jardin d'enfant..5 Les frais sont trop élevés……6 L'enfant n'est pas encore prêt/est trop jeune……………7 L'enfant des voisins n'a pas été admis………………………8 voulu l'inscrire, mais l'enfant n'a pas été admis………………9 Autre………………………….10 ACCEPTER JUSQU'À 3 REP.	Combien paye-t-on pour aller au jardin d'enfants d'ASMERADE par ans? MONT. CFA	Qui a repondu aux questions 132 à 134? CODE D'ID	Comment jugez-vous la qualité de l'encadrement dans le jardin d'enfants d'ASMERADE, entre… (1) excellent ET (5) très mauvais? Ne connais pas.-3	Dans votre opinion, quel serait le projet social le plus important pour promouvoir le développement de votre communauté?	Dans votre opinion, est-ce qu'on peut, généralement.. avoir de confiance en la plupart des gens…….1 ou doit-on être précautionneux en matière de contact avec des gens…..2	Désirez-vous que vos enfants ne se marient pas avec des membres d'un ou plusieurs des groupes suivants ? Aucune….-3 Autre ethnie..1 Non-Togolais.2 Chrétiens……3 Chrétiens d'une autre confession….4 Musulmans…5 Des gens parlant une autre langue………6 ACCEPTER JUSQU'À 3 REP.	Désirez-vous que vos enfants ne jouent pas avec des enfants d'un ou plusieurs des groupes suivants ? Aucune….-3 Autre ethnie..1 Non-Togolais.2 Chrétiens……3 Chrétiens d'une autre confession….4 Musulmans…5 Des gens parlant une autre langue………6 ACCEPTER JUSQU'À 3 REP.	Pensez-vous que la plupart des gens vous exploiteraient s'ils avaient la possibilité, ou essayeraient-ils d'être loyal/ »fair-play » ? Ils m'exploitaient 1 . . . 5 Ils essayaient d'être loyal	En général, comment évaluez-vous votre santé? Très bonne….1 Bonne…2 Satisfaisante…..3 Faible….4 Mauvaise….5
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concession   ménage

Section 11: caractéristiques des enfants

ENQUETÉES: LES PARENTS DES ENFANTS DE MOINS DE 17 ANS

ENQUET: ENF. DE MOINS DE 11 ANS QUI ARRIVENT À SE TENIR DEBOUT

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CODE D'IDENTIFICATION	143	143a	144	144a	145	147	148	149	150	151	151a	152	153	154	155	156	157	158
	(NOM) quel age a-t-il/elle eu lorsqu'il/ elle a commencé à aider avec des travaux ménagères (nettoyer, chercher de l'eau, faire la cuisine, etc.)? n'aide pas=-3 (>144)	(NOM) aide-t-il/ elle avec les travaux ménagères… rarement…1 souvent.2 très souvent….3	(NOM) quel age a-t-il/elle eu lorsqu'il/ elle a commencé à aider avec des travaux au champ? n'aide pas=-3 (>145)	(NOM) aide-il/elle avec les travaux au champ.. rarement…1 souvent.2 très souvent….3	POSER LES QUESTIONS À LA MÈRE DE L'ENFANT RESPECTIF (UN AUTRE PARENTÉ PLUS AGÉ SI LA MÈRE N'EST PAS DANS LE MÊME MÉNAGE) J'aimerais savoir si les traits suivants bien charactèrisent …(NOM)… ou non. Apréciez, et donnez une réponse entre (1) oui, exactement, et (5) non, complètement faux. Il/elle aime partager.	Il/elle est soli-taire, souvent joue tout seule.	Il/elle est aimé/ adoré par ces camarades.	Les autres se moquent de lui/ d'elle.	Il/elle souvent aide bénévolement les autres (condisciples, parents, les vieux).	Il/elle aime prendre le devant.	(NOM).., pendant combien d'heures par jour fait-il/elle des devoirs/ des études à la maison? "ne va pas à l'école"..-3 HEURES	.(NOM). a-t-il/ elle souffert de blessures ou d'une maladie au cours des 3 derniers mois? Oui, diarrhée…..1 Oui, paludisme……..2 Oui, autre maladie……3 Oui, blessé..4 Non.5 (>154) ACCEPTER PLUSIEURS RÉPONSES	Avez-vous été à l'hopital ou chez un guérisseur traditionnel? Oui, hopital....1 Oui, médécin traditionnel.2 Non……….3 ACCEPTER PLUSIEURS RÉPONSES	En général, comment évaluez-vous la santé de …(NOM)…? Très bonne…1 Bonne………2 Satisfaisante...3 Faible……….4 Mauvaise…….5	EXERCISE 1 QUELLE EST LA DISTANCE QUI SÉPARE LE SOL DES DOIGTS DE L'ENFANT LORSQU'IL SE COURBE LES BRAS TENDUS VERS LE SOL? Échec….-3 CENTIMÈTRE	EXERCISE 2 COMBIEN DE FOIS EN 1 MIN. L'ENFANT TOUCHE LE SOL AVEC L'AUTRE PIED LORSQU'IL NE SE TIEND QUE SUR UN PIED? Échec….-3 FOIS	TAILLE DE L'ENFANT CENTIMÈTRE	SI .(NOM). A MOINS DE 6 ANS Qui garde (NOM) pendant la journée, d'habitude? NOTER CODE D'ID SI PAS DANS LE MÉNAGE: Jardin d'enfants.44 École primaire.55 La mère……..31 Autre adulte feminin…..…32 Le père……...33 Aut. adul. masc.34 Soeur/frère.....35 Autre enfant..36 Autre ………...37
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concession ménage

Section 12: Connaissances des enfants

Page 12

L'ENQUETÉES: TOUS ENFANTS AGÉS DE 6 À 14 ANS (159–164a)

L'ENQUETÉES: TOUS ENFANTS AGÉS DE 3 À 14 ANS (165–168)

CODE D'IDENTIFICATION	159	160	161	162	163	164	164a	165	165a	166	167	167a	167b	168
	Je veux maintenant énoncer trois objets. Je voudrais que tu les repètes. S'il te plaît, mémorise les objets, car je vais te demander de les répéter encore une fois dans quelques minuntes. ORANGE, MAISON, CHAT	Si j'ai 200 Francs et je te donne 50 Francs, combien me reste-t-il? Juste….1 Faut….2	Si un grand sac de riz coute 2000 Francs, combien coûtent deux grands sacs? Juste….1 Faut….2	Combien est-ce que quattre grand sacs coutent? Juste….1 Faut….2	Est-ce que tu te souviens des trois objets que je vient d'énoncer? Repètes-les, s'il te plaît. NOMBRE RÉPÉTÉ	Quel est le nom de ton meilleur ami?	Si tu as un but, est-ce que tu l'atteinds normalement? Oui…….1 Non…..2	Est-ce que tu aimes aller à l'école? Oui…….1 Un peu..2 Non…..3	Lorsque tu es à l'école, est-ce que tu es à l'aise? Oui…..1 Non….2 Ne va pas à l'école.3 (>168)	En classe, est-ce que tes camerades de classe te distraient souvent? Oui…..1 Non….2	En classe, est-ce que tu destrais souvent tes camerades de classe? Oui…..1 Non….2	En classe, est-ce que tu t'ennuie souvent? Oui…..1 Non….2	En classe, est-ce que tu comprends ce que le/la professeur/e dit? Oui…..1 Non….2	Comment tu t'es senti régulièrement ces derniers temps? MONTRER LES DÉSSINS À L'ENFANT Très bonne……1 Bonne…………2 Satisfaisante….3 Faible…………4 Mauvaise……..5
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Section 12 (cont.)

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CODE D'IDENTIFICATION	169	170	171	172	173	174	175	176	177	178	179	180	181	182	183	184	185	186	187	188	189	190	191	192	193	194	199	200	200a	201
	Groupe de questions utilisé pour la suite A ou B																										Pendant l'interview, l'enfant a-t-il eu l'air… inattentif…1 particulièrement attentif …2 attentif..3	Pendant l'interview, l'enfant a-t-il eu l'air… timide …1 ouvert …2 très communicatif …3	Pendant l'interview, l'enfant a-t-il été sous forte influence des autres membres du ménage? Oui….1 Non…2	LANGAGE PARLÉ PAR L'ENFANT PENDANT L'INTERVIEW Ewe………1 Akposso…2 Francais…3 Autre (préciser)..4
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6.3 Map of the studied Togolese community - Overview


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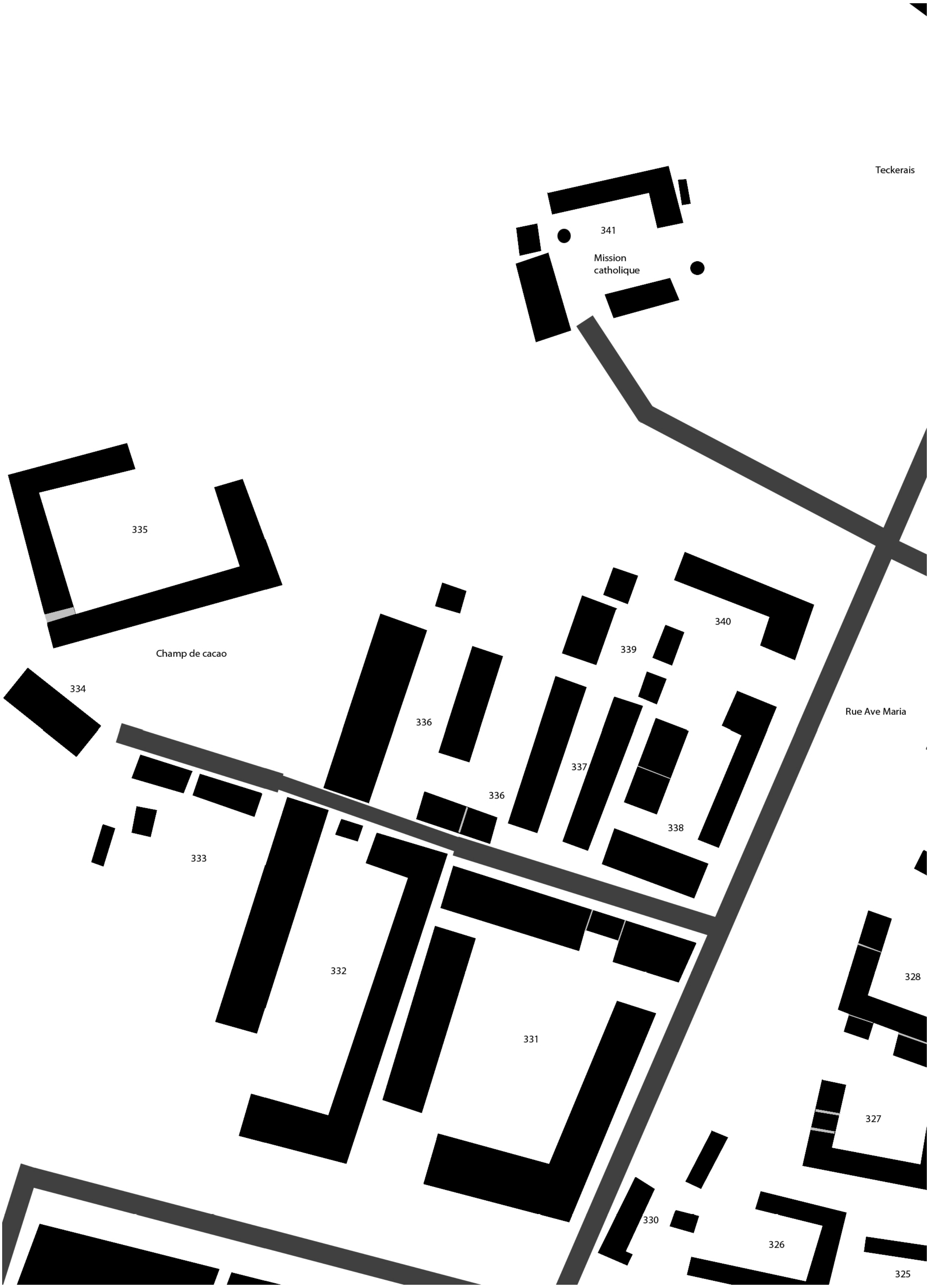
T17


A01
A02
A03
A04
A05

6.4 Map of the studied Togolese community - Map detail (sample)


Teckerais
341
Mission
catholique
335
Champ de cacao
334
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340
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Rue Ave Maria
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