ABSTRACT

Title of dissertation: ESSAYS IN EMPIRICAL MICROECONOMICS ON ECONOMIC DEVELOPMENT

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My dissertation consists of two independent empirical microeconomics studies in development economics. Though studying different interventions, they share the same methodology, which estimates the impact of a nationwide policy by contrasting trends over time between areas that are likely to have benefited differently from the policy. In the first essay, I estimate the economic benefits of the introduction of iodized salt in the United States in 1924. Using data from the decennial censuses of the United States and exploiting geographic variation in iodine deficiency before 1924, as proxied by the prevalence of goiter, I find that the introduction of iodized salt led to improvements in schooling among boys and greater income gains among men. No impact is found for females. Many of the estimated benefits are attributable to postnatal exposure to iodized salt, further demonstrating the importance of iodine supplements, which had previously been shown only in utero. The estimated

benefits are especially impressive when contrasted with the remarkably low cost of salt iodization.

The second essay studies the impact on child schooling and work of the elimination of primary school fees in Uganda in 1997. Using Ugandan Census data and comparing changes in work and schooling between primary-school ages and other ages, as well as between cohorts with different probabilities of exposure to the program, I find that, thanks to the program, areas with previously low enrollment rates saw greater improvements in schooling and larger decreases in child work. This result is not found in a neighboring country, Kenya, nor is it observed among age groups and cohorts too old to benefit from primary-school fee elimination. The results suggest that education and child work are substitutes, although in no way perfectly so.

ESSAYS IN EMPIRICAL MICROECONOMICS ON ECONOMIC DEVELOPMENT

by

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Chapter 1

Dissertation Introduction

My dissertation contains two chapters that share a common theme as well as a common methodology. Both chapters consider the impact on human capital outcomes of a public policy intervention in a development context. In the first chapter, I estimate the economic benefits of the introduction of iodized salt in the United Sates in 1924. The second chapter examines the impact on child schooling and work of the elimination of primary school fees in Uganda in 1997.

Given that both interventions were rolled out nationwide all at once, the common empirical methodology that I employ to identify the treatment impact is based on the interaction of two sources of variation: the expected variation in treatment intensity across areas, and the variation in treatment exposure across subsamples. The key idea is that regions previously more disadvantaged are expected to benefit more from a nationwide policy. In the case of iodized salt, the expected variation in treatment intensity across areas comes from the geographic variation in iodine deficiency before 1924, whereas in the Uganda intervention, this variation comes from county-level differences in prior primary school enrollment rates. Meanwhile, treatment exposure of a subsample is determined based either on the age in the survey year or on the birth year of the cohort relative to the intervention year, depending on whether the effect being examined is contemporaneous or cumulative. I investigate whether there is a break in the trend of the interaction between these two sources of variation that coincides with policy implementation, under the assumption that in the absense of the policy, the trend would maintain throughout.

The above methodology takes advantage of a sharp policy shift that creates differential changes in outcomes due to pre-intervention differences, and appears particularly applicable to studying large-scale interventions when randomization or a regression discontinuity design is not available, or not practically feasible. For example, Chin (2005) evaluates the impact of redistributing teachers across all public primary schools in India in 1987 on school attainment, and exploits two sources of variation: by birth cohort (only children attending primary school after 1987 are exposed) and by state of residence (children in states with more one-teacher schools in the pre-program period are more intensively exposed). Bleakley (2007) finds that areas with higher levels of hookworm infection prior to the hookworm-eradication campaign the American South (circa 1910-15) experienced greater increases in school enrollment, attendance, and literacy after the intervention. Finkelstein (2007) identifies the impact of the introduction of Medicare in 1965 on hospital spending by using the fact that Medicare should have a larger effect in U.S. regions with lower prior private insurance rates among the elderly. A technical difference between the methodology used in Finkelstein (2007) and the one in Chin (2005) and Bleakley (2007) is that

Finkelstein focuses more on the gradual change in the impact over the years, whereas Bleakley and Chin focus on comparing the post-intervention period versus the pre-intervention period (with all the years in each period grouped together). In light of this difference, my methodology is closer to Finkelstein's.

My two studies both find that the nationwide policy raised school enrollment. The estimated benefit of salt iodization after five years implies a cost of 7 USD to 45 USD in today's currency in postnatal supplementation to increase one year of schooling among boys in an average iodine-deficient (5 per thousand goiter rate) area (assuming full use of iodized salt over the five years). (No impact is found for girls). This is more cost-effective than such measures as providing uniforms to students in Kenya, which costs 48.5 USD to 83.14 USD for an additional year of schooling (Evans et al., 2008), although more expensive than deworming at a cost of 3.5 USD for an additional year of schooling (Miguel and Kremer, 2004). The low cost of iodization also stands out in comparison with costly programs like the construction of 60,000 schools in Indonesia during a five-year period that increased schooling by about 0.15 years (Duflo, 2001), or another program in South Africa that reduced class size by 25% and thereby increased schooling by half a year (Case and Deaton, 1999). The cost-effectiveness of deworming and iodine supplementation indicates that better health may be one of the most effective channels to get children to school in developing countries. In iodine-deficient areas, salt-iodization could be even more of a worthwhile route. Moreover, the estimated benefits are attributable to postnatal exposure to iodized salt, further demonstrating the importance of iodine supplements, which had previously been shown only in utero. On the other hand, in studying the elimination of primary school fees in Uganda, I cannot quantify the cost-effectiveness of the program due to lack of cost information.

I also document some other impacts of these nationwide policies. The introduction of iodized salt led to greater income gains among men in areas with higher pre-intervention goiter rates, suggesting a long-term impact on human capital outcomes. This long-term impact could be due to better education achievement associated with more school enrollment, or due directly to more school enrollment itself (such as due to social capital gains from socializing more with peers early on). In the case of Uganda, my results also indicate an impact of primary school fee elimination on child labor, which nobody had previously studied. The results suggest that child labor and school enrollment are substitutes, albeit not perfectly.

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Chapter 2

Iodized Salt and U.S. Development

2.1 Introduction

The importance of micronutrients in human development has received increasing attention both in medical science and in economic policymaking. Iodine is one of the most discussed micronutrients in the context of less developed countries. According to UNICEF, as of 1998, "iodine deficiency [was] the single most important cause of preventable brain damage and mental retardation. [...] An estimated 43 million people worldwide [were suffering] from varying degrees of brain damage and physical impairment due to iodine deficiency, including 11 million who [were] cretins, afflicted with profound mental retardation. [...] Less severe iodine deficiencies in children and adults can mean a loss of 10 intelligence quotient (IQ) points and can impair physical coordination." The most visible manifestation of iodine deficiency is goiter, an enlargement of the thyroid gland. Although goiter is rarely seen in the US today, in 1924 it was described as "one of the most important and widespread causes of human suffering and of physical and mental degeneracy with which society has had and still has to deal" (Marine, 1924).

In this research, I focus on the introduction of iodized salt in the US in 1924 as a public health intervention. To the best of my knowledge, my study is the first economics research to examine the direct economic impact of postnatal, in addition to prenatal, iodine supplementation. Considering the low cost of iodized salt, which is 0.4 to 0.5 US cents/kg, or 2 to 9 US cents per person/year in today's currency, my results contribute to evidence of considerable economic gains from cost-effective treatment programs that tend to the most basic elements of health, thereby speaking to the great human capital needs of developing countries, in the face of their tight budget constraints, and the large costs of alternative human capital building initiatives, such as school construction and teacher supply.

My identification strategy is based on the timing of the introduction of iodized salt in the US combined with cross-area differences in pretreatment goiter prevalence, used as a proxy for the prevalence of iodine deficiency. As demonstrated by numerous surveys and in particular, the WWI draft statistics, the goiter distribution in the US before 1924 followed a rather clear geographic pattern due to the availability of iodine in the local environment. I hypothesize that areas with higher pretreatment goiter rates had more to gain from iodized salt than areas with lower pretreatment goiter rates. My empirical strategy is to look for a break around 1924 in any pre-existing differences in trend between higher and lower goiter rate areas. The identifying assumption is that, in the absence of salt iodization, after 1924, the difference across areas would have continued on the same trends as before.

Important to my empirical strategy is that, in the U.S. context, neither the introduction of iodized salt in 1924 nor the various measures taken to prove and promote the role of iodine in goiter treatment and prevention should be considered endogenous to the growth prospects of the more affected areas. In fact, the adoption of iodized salt was the result of numerous surveys, experiments and public health initiatives undertaken over many decades by medical scientists in the US as well as in Europe, which historically bore a large share of the goiter burden.

The economic outcomes I consider in this research are school enrollment among children, and educational attainment, income, wages, occupational scores and employment status among adults. The relevance of these outcomes is based on the scientific fact that iodine deficiency suffered at any stage of life has adverse effects on mental and physical function and development, since iodine is crucial to regulating the metabolism of every cell in the living body. For children, it is likely that schooling would be affected by a lack of iodine due to both a current poor mental and physical state (lethargy, slow comprehension…) and low expected returns to human-capital investment. Adults may perform poorly as a result of lethargy, fatigue and slow speech and thought. There could also be a link from early life impacts to adult outcomes due to the long-term nature of human capital accumulation. I also consider this potential long-term impact.

The estimations for males are more reliable than for females because my measure of goiter prevalence was obtained only for males from WWI draft examinations, and there are important differences in the iodine deficiency prevalence of the genders, as discussed further in Section 2. Also, it is possible that variation in outcomes may be greater for males than females in the cohorts under study due to higher rates of human capital investments and labor force participation of males. Another possibility is that females were suffering more severely from iodine deficiency, thus taking longer to "recover". In order to maintain focus on the most reliable and significant results, I will only present and discuss results for males.

For reasons related to data availability, I perform estimations based on variation in prior goiter rates both across and within states. The within-state geographic division is called the section, as determined by the U.S. Surgeon General's Office for statistical reporting purposes. (There were 156 sections in total).[1] I find that, after five years of intervention, in a state like South Dakota, in the most iodine deficient section (with a prior goiter rate of 12.12 per 1,000 drafted men) relative to the least iodine deficient section (with a prior goiter rate of 1.24 per 1,000 drafted men), iodized salt brought an increase in school enrollment of 2 to 3 percentage points. These benefits are attributable to postnatal exposure to iodine supplementation since the children in the sample were all born before 1924. Although section-level estimations are feasible only up to 1930 due to data limitations, the section-level findings are supported at the state level over a more extended period, controlling

[1] The division of states into 156 sections was a compromise between reporting for 50 states and reporting for 4,557 local examination boards. The sections were determined mostly based on the racial composition of the population. Each section usually contains a number of counties, although some sections contain only one county or one big city. Alaska, Delaware, District of Columbia, Idaho, Nevada, Rhode Island, Vermont, and Wyoming were not divided into sections.

for census-region-level changes over time and including prenatal exposure to iodized salt. Results also suggest that iodized salt improved adult outcomes in the short run and over the lifetime. Moreover, because the years of childhood and youth are the most crucial in human capital investment but also the most vulnerable to iodine deficiency due to biological growth, early use of iodized salt could lead to economic gains in adulthood. My results lend support to this hypothesis.

The rest of the paper is organized as follows. Section 2 provides background information on iodine, iodine deficiency, and the geographic variation in goiter prevalence. Section 3 briefly covers the related literature. In Section 4 I describe the data, identification strategy, and estimation framework. Results for children are presented in Section 5. Section 6 presents results for adult outcomes, including both short-term and long-term effects of iodized salt. Section 7 concludes.

2.2 Iodine Deficiency, Goiter Prevalence and Iodized Salt

2.2.1 Iodine Deficiency and its Consequences

Iodine is an essential nutrient for normal growth and development in animals and human beings. The thyroid gland, located in the lower part of the neck, uses iodine from foods to produce thyroid hormones, which are released into the blood stream and transported throughout the body to control metabolism (the conversion of oxygen and calories to energy). Therefore, iodine is crucial to the functioning of every cell in the body.[2]

The main sources of iodine for human consumption are foods, milk and water, with the highest iodine content found in some milks, leafy vegetables, and sea foods, especially seaweeds. The optimal iodine intake is very small: the WHO recommends a daily dose of 90 μg for infants of 0-59 months, 120 μg for schoolchildren of 6-12 years, 150 μg for adolescents and adults, and 200 μg for pregnant and lactating women (Clar et al., 2002). (Half a teaspoon of iodized salt contains about 150 μg of iodine). Nevertheless, iodine deficiency is a risk to many people due to the minute iodine content in most foods. When iodine intake is insufficient, the thyroid gland has to work extra hard to produce the thyroid hormones needed by the body, causing an enlargement.

At any stage of life, from the fetal age to adulthood, insufficient iodine intake causes a number of functional and developmental abnormalities, often referred to as iodine deficiency disorders (IDD). The main IDD's are goiter, hypothyroidism (causing fatigue, lethargy, slow speech and thought), impaired mental function, retarded physical development, and increased susceptibility of the thyroid gland to nuclear radiation (WHO, 2004). (Note that goiter may not be visible if iodine deficiency is minimal. On the other hand, iodine deficiency is the single major, but not exclusively the only, cause of goiter. Goiter itself obviously has a cosmetic cost and some possible complications such as aspiratory difficulty if the goiter size is too

[2] http://www.endocrineweb.com/thyfunction.html

large.) Although the most detrimental IDD is cretinism resulting from severe iodine deficiency in utero, postnatal iodine deficiency can be highly damaging and result in a 5%-50% loss of individual productivity depending on severity (Hetzel and Pandav, 1996).

Most iodine is found in the ocean. Large amounts of iodine are leached from surface soil by glaciations, snow and rain, and then carried by wind, rivers and floods into the sea. Therefore, the areas most likely to be iodine deficient are lands far from the sea, especially mountainous areas (Hetzel, 1989a; Delange and Ermans, 1996). Medical research has found that iodine deficiency and goiter incidence vary not only by geography, but also by gender. The ratio of male to female goiter rates tends to increase as the overall prevalence increases (Schiel and Wepfer, 1976). Susceptibility to iodine deficiency appears to be biased toward females from in utero (Friedhoff et al., 2000), possibly through hormonal pathways (Chan et al., 2005).

2.2.2 The Geography of Iodine Deficiency in the US

At the beginning of the iodized salt campaign, there existed a “goiter belt” in the northern parts of the US where the iodine content of the water and soil was generally low. The WWI draft examinations constitute the first and only nationwide goiter survey and provide an overall view of the geographic pattern of goiter prevalence in the US in 1917 (Figure 1, from McClendon, 1939), which coincides very well with the concurrent map of iodine content in drinking water (Figure 2, from McClendon and Hathaway, 1924).[3]

It is important to note that there could be substantial variation in goiter prevalence across sections within a state, or across states within a census region, as can be seen from the summary of the WWI draft statistics in Table 1. This variation is important for my empirical strategy because of the need to control for state-level or region-level time effects to remove any systematic coincidence between the goiter distribution and geographic differences in economic development over time, such as the North-South divide. I will discuss this issue further below. It should be noted that the goiter rates from the draft statistics are much lower than the goiter rates collected from the common population, such as from school children (see Olesen, 1929, whose data were collected at unspecified dates both before and after the introduction of iodized salt, therefore I cannot use them in my study). Olesen attributes this to the fact that goiters are much more visible in children, and that only goiters so large that military collars would not button around them were noted in the draft examinations. Due to the gender difference in goiter incidence mentioned in Section 2.1., the geographic pattern of goiter prevalence among drafted men may not represent well the prevalence pattern among females. The non-linear relationship

[3] In my empirical work below, I do not use the data on water iodine content in McClendon and Hathaway (1924) because the data are available only for 69 towns across the US, often one town per state (as opposed to WWI draft examination data, which are available for 153 sub-state regions of the US), thus not providing enough variation when I control for state fixed effects. I also do not use information on today's soil content because of changes that could have happened over the long period of time since the introduction of iodized salt, as a result of climate changes, geological processes, and human activity.

between female and male goiter rates discourages attempts to impute the female goiter distribution from the male goiter distribution.

In using the drafted men's goiter distribution to study outcomes among males, I make two assumptions. One assumption is that the geographic distribution of the kind of goiter noted in the draft examinations (that is, large goiters) is representative of the geographic distribution of simple goiter among all men of the same age range. The second assumption is that the variation in goiter incidence from one age to another in the male lifetime is comparable across geographic areas, so that the adult goiter distribution proxies adequately for the distribution among boys, young men, and all adult men aged 25-60.

2.2.3 Introduction of Iodized Salt in the US in 1924

According to Annegers and Mickelsen (1973), a number of factors brought attention to goiter as a health problem in America during the early 1920s, among them: (a) the decline of other childhood diseases allowed more attention to be paid to endemic goiter; (b) David Marine's 1917-19 experiment showed that iodine could prevent and treat goiter[4]; (c) McClendon discovered that the iodine content of drinking water

[4] From 1917-19, two grams of iodated syrup were given twice a year to 2,190 of 4,495 school girls in Akron, Ohio. At the end of the test, only five treated girls developed thyroid enlargement whereas 495 untreated girls did. 773 of 1,182 girls who had initial thyroid enlargement and were treated showed a decrease in gland size, whereas only 145 of 1,049 girls who had initial thyroid enlargement but were not treated showed a decrease in gland size (Markel, 1987).

and the incidence of goiter were closely related; and (d) the WWI draft examinations revealed the nationwide extent of goiter prevalence.

David Marine's experiment inspired Switzerland to set up prophylactic programs against goiter, one of which was the use of salt as a vehicle for adding iodine to the population's diet. This idea was picked up enthusiastically by David Cowie of the University of Michigan, who was interested in eliminating widespread simple goiter in his home state, and who pioneered the movement to bring iodine into the American daily diet. Endorsed by the Michigan State Medical Society, iodized salt produced by a number of salt companies first appeared on Michigan grocers' shelves on May 1, 1924. In the fall of 1924, the Morton Salt Company began selling iodized salt nationally. With the continued educational efforts of the Michigan State Medical Society and the zealous advertisements by salt manufacturers, iodized salt grew rapidly in popularity. By 1930, sales of iodized salt were eight times those of plain salt (Markel, 1987).

Many follow-up surveys were conducted to assess the impact of the use of iodized salt and all found marked decreases in thyroid enlargement, especially among continuous users. Interestingly, Schiel and Wepfer (1976) found that, among Michigan school children in 1924-51, there was a decline in goiter rates even among non-users. Cowie postulated that this was due to children ingesting iodized salt at least part of the time without realizing it, such as in school canteens and restaurants, which seems plausible given that iodized salt made up 90% of salt sales in Michigan at the time

(Markel, 1987). This observation alleviates concerns related to possible self-selection in the use of iodized salt and supports the approximate universality of exposure to the intervention. Figure 3 shows U.S. goiter survey results compiled by the Chilean Iodine Educational Bureau in 1950 and the American Geographical Society in 1953. The size of the endemic goiter areas decreased considerably between the WWI draft era (Figure 1) and 1950. A further decrease can be seen from 1950 to 1953.

While little prior research has mentioned the effects of iodized salt on human capital outcomes (as opposed to goiter) following the 1924 U.S. intervention, the adverse effects of iodine deficiency on human capital outcomes have been surveyed and documented in more recent small-scale experiments. Correlational evidence among school-age children is particularly abundant. In a study on Bangladeshi children, Huda et al. (1999) find that hypothyroid children performed worse than those with normal thyroid gland function on reading, spelling and cognition, after controlling for a wide range of health, nutritional, and socioeconomic factors. Even in a developed-country area with very mild iodine deficiency (Jaen, Spain), Santiago-Fernandez et al. (2004) report the risk of having an IQ below 70 to be greater in children with urinary iodine levels less than 100μg/liter. Recently a few randomized experiments have been conducted, such as Zimmermann et al. (2006), who find that iodine supplementation improves cognition in iodine-deficient schoolchildren in Albania.

2.3 Related Literature

Obviously there has been no lack of medical research on goiter and IDD given their long history. However, systematic economic research on iodine deficiency in particular, and on the role of micronutrients in general is only starting.

The three studies on iodine deficiency that I am aware of in the economics literature are Field et al. (2007), Feyrer et al. (2008), and Politi (2008). Field et al. (2007) examine an intervention in Tanzania and find that treatment of mothers with iodated oil improves children's schooling by 0.33 years, with a larger increase among girls than boys. Feyrer et al. (2008), concurrently to my research, study the same U.S. intervention in 1924, but use WW II enlistment data for outcome variables and find that those enlistees born after 1924 were more likely to be assigned to the Army Air Forces (which requires higher army test scores) than those born before. Politi (2008), also concurrently to my research, looks at the introduction of iodized salt in Switzerland in the 1920s and 1930s, and finds that the cohorts born after the intervention achieved higher levels of schooling.

All of the above studies consider their intervention as a treatment in utero. While such identification is obvious in the Tanzania case where only expectant mothers received the treatment, it may not seem so obvious for the U.S. and Swiss interventions. The all-encompassing nature of the introduction of iodized salt in the US means that it was a treatment to the population of all ages. Given that iodine is needed for the normal function and development of the body at any stage of life, it is sensible to expect *postnatal* iodine supplementation to have an impact as well.

Besides considering economic benefits from postnatal iodine supplementation, I also contribute to the existing discussion by providing evidence for a richer set of outcome variables from census data. My estimation strategy is partly similar to Finkelstein (2007), and partly similar to Bleakley (2007).

2.4 Data, Identification Strategy, and Estimation Framework

2.4.1 Data

To proxy for the geographic distribution of iodine deficiency in the US before 1924, I use the goiter distribution among 2,510,701 drafted men who participated in the WWI draft examinations in 1917-18, and whose aggregate draft statistics were published in 1920 by the U.S. Surgeon General's Office. The 2,510,701 drafted men include approximately two million men sent to mobilization camps prior to the signing of the armistice on November 11, 1918, and also those who were rejected by the local examination boards. According to Love and Davenport (1920), "[t]his number constituted about four-fifths of all the men who were physically examined and [was] representative of all. The men examined were of ages 18-30 inclusive, but relatively a larger proportion of the male population between the ages of 21 and 30 years [was] included in these statistics than those of [younger ages]." Importantly, there does not appear to be any sample selection issue in the baseline goiter data.

As mentioned in Section 2.3., Marine's 1917-19 experiment was the first to inform the U.S. public that iodine supplementation could prevent and treat goiter. I did not find any evidence of people consuming iodine supplements or engaging in other efforts to prevent and treat goiter prior to 1924. Therefore, there is strong reason to believe that the WWI goiter rates systematically pick up geographical (and not socioeconomic) variation in iodine deficiency before the 1924 intervention. After matching the sections defined in the 1920 report with U.S. counties, I end up with 153 sections due to changes in area boundaries over time that result in some mismatches. In the estimations that study within-state variation, the samples include 146 sections because, as mentioned in Section 1, seven states and the District of Columbia were not divided into sections. At the state level, the goiter rate is the weighted average rate across the sections of the state, using the provided sample weights.

I link the aggregate data on goiter prevalence to individual socioeconomic data from the Census Integrated Public Use Micro Sample (IPUMS) database. Since county of residence is available in IPUMS only up to 1930, I consider the 1870-1930 period in my postnatal-impact analysis. I then extend the analysis to the period from 1870 to 1960, exploiting variation in goiter prevalence at the state level, and considering both prenatal and postnatal exposure to iodized salt. To gain a better understanding of the cumulative impact of iodine supplementation over a lifetime, I also analyze cohort-level economic outcomes observed in ten census years from 1900 to 1990 for cohorts born from 1840 to 1965. Finally, I focus on exposure to iodized salt during childhood and youth and use the 1960 sample to examine whether benefits from iodized salt when young materialized into better adult outcomes later in life. The cohort and long-term analyses are similar to those in Bleakley (2007) to a large extent.

For children, I use the age range 8-16 and the school enrollment variable. School enrollment indicates whether the child had gone to school at least one day in the months before the census (the number of months changes somewhat from census to census). I also look at literacy (ability to both read and write). Literacy is not available after 1930. Educational attainment information is only available from 1940.

For adults, I look at the age range 25-60. The adult outcomes that I use for the period 1870-1930 are the Duncan socioeconomic index (SEI), the occupational prestige score (PRESGL), and the occupational income score (OCCSCORE)[5]. There are three other occupational standing variables available for this period (although no income and wages variables are available), but given that they bear strong correlation with the above listed variables, I will only briefly mention the results for them. Literacy is used for a falsification check since it is a "stock" variable that is unlikely to change in response to iodization by 1930 for adults aged 25-60. In the long-term analysis, using outcome data in 1960, I look at the SEI, PRESGL, OCCSCORE, employment status, highest grade of schooling, income, and wages (the four latter

[5]SEI is a measure of occupational status based upon the income level and educational attainment associated with each occupation in 1950. The maximum value of SEI is 96. PRESGL assigns a Siegel prestige score to each occupation using a 1950 occupational classification scheme. OCCSCORE assigns each occupation in all years a value representing the median total income (in hundreds of 1950 dollars) of all persons with that particular occupation in 1950. The maximum value of OCCSCORE up to 1960 is 80, and from 1970-1990 is 79.

variables are not available prior to 1940). Census data give me the advantageous availability of a variety of economic outcomes, in contrast to existing long-term studies that suffer from scarcity of outcome variables.

Table 2 reports mean outcomes in 1920, before salt iodization. Note that, consistent with the well known North-South gap in development, except for PRESGL, all the other initial outcomes were better for high goiter rate areas, which are primarily in the North. This systematic pattern calls for inclusion of state-year or region-year fixed effects in estimations in order to control for unobserved state-level or regional characteristics and policy changes that may have affected outcomes in the absence of the iodized salt intervention.

2.4.2 Identification Strategy and Estimation Framework

The basic empirical idea is to compare changes in outcomes in regions where iodized salt potentially had a larger effect (because of greater prior iodine deficiency, proxied by higher goiter prevalence) to where it potentially had less of an effect (because of lower prior iodine deficiency, proxied by lower goiter prevalence). In other words, I look for a break around 1924 in any pre-existing differences in trend between higher and lower goiter rate areas. The underlying identifying assumption is that without iodized salt, any pre-existing differences across areas would have continued on the same trends as before. To support the validity of these assumptions, when exploiting section-level variation in goiter prevalence, I account for section fixed effects to purge out fixed differences across sections, as well as state-level year effects. Controlling for state-level time effects helps take into account state-level policy changes and movements such as changes in child labor and compulsory schooling laws, and the women's suffrage movement[6], that happened at the beginning of the twentieth century. In state-level estimations, I include state fixed effects and census-region year effects. The inclusion of census-region year effects helps purge out average changes over time among the states in each region.

One could worry that at the same time as the introduction of iodized salt, there may have been other concurrent changes in the U.S. diet, such as other food fortification initiatives, in particular. Backstrand (2002) notes that food fortification in the US began with salt iodization in 1924, and that salt iodization was roughly coincident with new discoveries of the role of vitamin and mineral deficiencies in causing many diseases and sicknesses. However, the knowledge remained in the laboratory until the end of the 1930s when "[v]itamin and mineral deficiencies were prevalent in the U.S. population." It was not until May of 1941 that President Roosevelt called a National Nutrition Conference for Defense, in response to high rates of malnutrition and fears of potential U.S. involvement in war. Figure 4 shows the change in the per capita riboflavin, iron, niacin, and thiamin content of the U.S. food supply between 1909 and 1994, which does not coincide with the timing of salt iodization.

[6] Grant Miller (2008) finds that suffrage rights for women helped improve children's health.

With the data and identification strategy discussed above, I employ the following estimation frameworks. Over the period 1870-1930, for outcome Y of child or adult i residing in section j of state k and census year t, the main regression equation is:

$$(1) \qquad Y_{ijkt} = \beta_t(G_{jk} * \delta_t) + \delta_t + \mu_{jk} + \gamma_{kt} * \delta_t + X_{ijkt}\Gamma + \varepsilon_{ijkt}$$

where G_{jk} is the pre-treatment goiter rate of the section, δ_t are year dummies, μ_{jk} are section fixed effects to control for fixed differences across sections, γ_{kt} are state-year fixed effects to control for state-level changes over time, and $X_{ijkt}\Gamma$ is a vector of individual-level controls.

For the 1870-1960 period, I exploit variation in goiter rates at the state level and use the same framework as (1), with j being the state, and k the census region. I also use (1) as the estimation equation for the cohort analysis using data from 1900 to 1990, with j being the state, k the census region, and t the birth year of the cohort.

The key variable of interest in equation (1) is $(G_{jk} * \delta_t)$. Importantly, since I do not impose any ex-ante restrictions on when any structural breaks may occur, I allow the estimated β_t's to reveal in a flexible way the movements over time in the dependent variable in sections with higher prior goiter rates relative to sections with lower prior goiter rates. A change in the trend of the β_t's before and after 1924 indicates the impact of the introduction of iodized salt.

Using 1960 data, the regression equation for long-term outcome Y of adult i of age a born in state j is as follows:

$$(2) \qquad Y_{ija} = \beta_a(G_j * Exp_a) + \delta_a + \mu_j + \mu_j * a + X_{ija}\Gamma + \varepsilon_{ija}$$

where Exp_a=max(min(21,56-age),0)[7], which implies that exposure during childhood and youth is 0 for cohorts born 21 or more years before 1925, increases linearly for cohorts born in the 21 years before the introduction of iodized salt, and is 21 for all cohorts born in and after 1925; $\delta_a$are age fixed effects to control for nationwide age effects; and $\mu_j * a$ are average state-level age trends, which are assumed to be linear. This specification assumes that the main impact of iodization on long-run outcomes occurs through human capital accumulation and that this process ends at age 21. I choose age 21 because in 1960, about 17% of the sample male population of ages 25-60 had two years of college or higher education. In fact most of the results are not sensitive to moving the threshold age by a few years. I find the use of the exposure variable as defined above to be suitable for this study for two reasons: it encompasses postnatal exposure, and it allows for possible delays in in-utero impacts of uncertain length (in contrast to a specification that replaces Exp_a with a dummy for being born after the intervention).

The next two sections present the empirical results. The goiter rate used for estimation is the rate per 1,000 men, as in the original draft statistics. To accommodate serial correlation over time within areas, I allow for an arbitrary variance-covariance matrix in the error structure within each section or state, in accordance with the level of geography of goiter rate variation for the sample under study.

[7]This exposure measure is adopted from Bleakley (2007).

2.5 Impact of Salt Iodization on Children

Table 3 presents the β_t's from estimating equation (1) for boys aged 8-16, taking 1920 as the reference year. One plausible concern is that while the state-year effects purge out average state changes over time, different sections of a state may have responded differently to policy changes, and those differences may have coincided with the prior goiter distribution and the timing of salt iodization. To address this concern, in columns (2), (4), and (6), I include interactions of the 1930 dummy with several lagged section-level variables (literacy, school enrollment, fraction of black children, black * school enrollment, sex * school enrollment) that could characterize the prior socioeconomic status of the section (especially in relation to the dependent variable), thus accounting for changes that could have been correlated with prior section characteristics. The inclusion of lagged section average outcomes also takes into account the potential problem of mean reversion. For example, some sections with high goiter rates could have had low schooling in the prior period because of a temporary shock, so that an improvement could be observed in those areas in the following period even in the absence of iodized salt.

It is important to note the downward trend in the β_t's from 1870 to 1920 for school enrollment, in contrast to the statistically significant β_{1930}, which is greater in magnitude than for all the three census years before 1920. The downward trend in the β_t's up to 1920 indicates that, prior to 1924, school enrollment was not growing as fast in higher goiter rate areas as in lower goiter rate areas, which could be partly due to accumulated adverse effects of iodine deficiency over generations, and partly because more iodine deficient boys were not able to take advantage of improvements in schooling availability that may have happened over time as well as less iodine deficient boys. β_{1930} becomes even more economically and statistically significant when I include interactions between the 1930 year dummy and lagged section-level covariates, thus alleviating concerns about mean reversion in 1930. (The large β_{1870}'s here may be puzzling. However, the β_{1870}'s for adults are much smaller, suggesting that there was not a systematic abnormality in measurement or sampling in 1870).

To gain insight on the statistical significance of the reversal in trend that occurred in 1930, I test the statistical significance of $\beta_{1930} - (- \beta_{1910})$, which is the difference between the change in β_t from 1920 to 1930, and the change in β_t from 1910 to 1920. The robust results of this test are reported in Table 4. I then test whether in any year prior to 1924, there is evidence of a dramatic reversal in trend as in 1930. To this end, I limit the data to 1920 and re-estimate (1), falsely assigning some census year prior to 1920 as the reference year, and performing the exact above test that I did for 1910-1920 and 1920-1930. I find no such positive reversal in trend before 1920 (Table 4). Results for literacy show no impact of iodized salt, possibly because literacy is more of a stock variable, taking longer to change than a flow variable like school enrollment, and therefore an improvement thanks to iodine supplementation is not yet observed only five years after the intervention.

The regression results indicate that an increase of one goiter case per 1,000 increased a section's school enrollment rates by about 0.22 to 0.3 percentage points five years following the introduction of iodized salt. In a state like South Dakota with considerable variation in prior goiter prevalence (see Table 1), in the most iodine deficient section (with a prior goiter rate of 12.12 per 1,000 drafted men), iodized salt brought an increase in school enrollment of about 2.2 to 3 percentage points more among boys than in the least iodine deficient section (with a prior goiter rate of 1.24 per 1,000 drafted men). It is perhaps worth recalling that detection of goiter in the draft examinations tends to be an underestimation of the prevalence of iodine deficiency in children, since only goiters so large that military collars would not button around them were noted. This implies that the actual marginal impact of iodized salt among boys is likely to be lower. For example, if the goiter rate among boys is three times that among drafted men, then an area with a one percentage point higher goiter rate in boys would gain up to about 1 percentage point in schooling thanks to salt iodization.

The importance of these results lies in the fact that the iodized salt exposure of the children in the 1930 sample is entirely postnatal. Children 8 to 16 years old in 1930 were born from 1914 to 1922, before the introduction of iodized salt in late 1924. The results therefore are novel findings of the effects of postnatal iodine supplementation, in addition to the in utero effects that economics research has focused on up to now. Given that salt iodization costs 2 to 9 cents/person/year, the estimated benefit after five years implies a cost of 7 USD to 45 USD in today's currency in postnatal supplementation to increase one year of schooling among boys in an average iodine-deficient (5 per thousand goiter rate) area (assuming full expenditure on iodized salt over the five years).This is more cost-effective than such measures as providing schooling uniforms to students in Kenya, which costs 48.5 USD to 83.14 USD for an additional year of schooling (Evans et al., 2008), although more expensive than deworming at a cost of 3.5 USD for an additional year of schooling (Miguel and Kremer, 2004). In iodine-deficient areas, salt-iodization could be one of the most cost-effective measures.

To examine the benefits from salt iodization among boys over a longer period, I re-estimate (1) on data from 1870 to 1960, using goiter prevalence at the state of birth or state of residence for identification, controlling for census region-year effects. Figure 5 graphs the estimated β_t's with 1920 as the reference year, and β_{1920} normalized to 0. In all cases, the reversal in trend found for 1870-1930 at the section level is confirmed for 1870-1960 at the state level. After 1920, school enrollment started to grow at about the same rate or faster in the states where prior goiter prevalence was greater. The β_t's from 1940 onward, when estimated at the state of birth, capture both prenatal and postnatal exposure to iodized salt, and perhaps some intergenerational transfers of benefits from iodized salt from the children's parents as well.

In general, the trends given by the state-level estimations track the trends given by the section-level estimations quite well where the two samples overlap, that is, up to 1930 (comparing results in Table 3 with Figure 5). This suggests that it is reasonable to extend the section-level analysis to the state level by using state-level goiter distribution to identify the impact of iodized salt, with census-region year effects appropriately accounted for. I perform similar tests of the statistical significance of the reversal in trend as I did for the 1870-1930 sample. I first compare the n-census-year change in β_t after 1924 relative to the n-census-year change in β_t before 1924. Table 5 shows a particularly strong reversal in trend when goiter rates are measured at the state of birth, perhaps indicating the importance of early exposure to iodine supplementation, including in utero exposure. By contrast, when I falsely assign some census year prior to 1920 as the reference year, I find no such positive reversal in trend in any case (Table 5). It is also worth noting that the patterns from estimations at the state of birth are generally very close to those at the state of residence, which is consistent with the low rate of migration among children.

2.6 Impact of Salt Iodization on Adults

The β_t's from estimating equation (1) for men aged 25-60, taking 1920 as the reference year, are reported in Table 6. For the same reasons as explained in the children section, in columns (2), (4), (6) and (8), I include interactions of the 1930 dummy with the lagged average of the dependent variable and the lagged fraction of black. As expected, there is no impact of iodized salt on literacy for the adults in the sample, who were at least 20 years old by the time of salt iodization. Based on the reported coefficients and the results of a falsification test similar to that performed for children (not reported but available upon request), a reversal in trend at year 1920 is not as apparent for the occupational standing variables in the adults' sample as for the schooling variables in the children's sample. However, I perform another test comparing the changes in outcomes from one census year to the next, and find that the changes in PRESGL and SEI from 1920 to 1930 are more statistically significant than between any other two census years before, especially after accounting for mean reversion (Table 7; columns (2) and (4) account for mean reversion by interacting the later year dummy with the lagged average of the dependent variable and the lagged fraction of black.) I find the same results for two other occupational standing variables available for this period (results not reported but available upon request). In summary, four out of six occupational outcomes saw a more statistically significant change from 1920 to 1930 than between any other two census years before. The less apparent impact found for adults by 1930 perhaps reflects the fact that adult outcomes are the result of human capital investment over a long time whereas school enrollment is a choice variable that can adjust quickly.

To examine the economic gains from salt iodization among men over a more extended period, I re-estimate (1) on data from 1870 to 1960, using goiter prevalence at the state of birth or state of residence for identification. With the data extended to 1960, when I look at goiter variation at the state of residence, there is hardly any continued break in trend after 1930 that is discernable (results not shown but available upon request), perhaps because of a considerable rate of migration among adults (in 1930, about half of the men were not living in their state of birth). For example, a person born in a high goiter rate state may have benefited greatly from consumption of iodized salt when young, but later moved to a low goiter rate state and is observed as an adult in the sample. This would cause imprecision in identifying the impact of iodized salt based on the prior goiter rate of the current state of residence. I do not estimate (1) for migrants and non-migrants seperately due to the problem of selection into migration. One thing to note before moving to the state-of-birth results is that, as in the case for children, I find that the patterns given by the estimations at the state of residence track the patterns given by the section-level estimations quite well where the two samples overlap, that is, up to 1930. This further supports the relevance of extending the section analysis to 1960 at the state level.

Unlike for the state of residence, when I look at goiter variation at the state of birth, I find the much more clear trends depicted in Figure 6, which graphs the estimated β_t's with 1920 as the reference year, and β_{1920} normalized to 0. The graphs show a reversal in trend at 1930, indicated by a slope upward for PRESGL, and a less negative slope for SEI. A trend is not apparent for OCCSCORE. The fact that the reversal in trend for PRESFL and SEI is observed at 1930 instead of 1920 suggests a lag in impact of five years when identified at the state of birth. This is again consistent with the high rate of migration among adults. For many of the adults in 1930, iodized salt was introduced when they had moved out of their state of birth, thus not benefiting from it. In later census years, since salt iodization had happened for longer, there would be an increase in the probability of the adults in the sample being exposed to iodized salt at their state of birth in the earlier years of their life. It is important to note that exposure to iodized salt of the adults observed in 1930, 1940 and 1950 (except some of the 25 year-olds in 1950) is postnatal because these adults were born before 1924. This provides further evidence of benefits from postnatal iodine supplementation.

To gain more insight on the lifetime impact of iodized salt, I perform a cohort analysis to study how salt iodization at the state of birth affected lifetime outcomes. I pull together ten censuses, from 1900 to 1990, and estimate (1) with δ_t being cohort birth years, controlling for region-cohort effects. The estimated β_t's are plotted in Figure 7, with birth year 1840 as the reference year. An upward shift starts around birth year 1910, coinciding with cohorts exposed to iodized salt at around fifteen -sixteen years old, the last year of age of our children sample, demonstrating once more benefits from postnatal supplementation. One may be concerned that 1910 is in a period when schooling and child labor laws in many states became effective. However, if it were these laws that caused a shift in outcomes, the shift would be downward because higher goiter rate states tend to be in the North with better initial outcomes, and Lleras-Muney (2002) shows that these laws helped reduce inequality. Therefore, if anything, considering such concerns makes the impact of iodized salt appear more positive. Since the impact is identified at the state of birth, the plots also indicate the important role of early iodine supplementation for later outcomes.

Motivated by Figure 7, I focus further in on the effects of exposure when young on adult outcomes with a long-term follow-up based on equation (2), using the 1960 IPUMS and the goiter distribution in the state-of-birth. The older cohorts serve as a comparison group in comparing the impact of early exposure to iodized salt across cohorts. The results are reported in Table 8.

I find that iodized salt had a positive impact on the absolute level of income and wages but not on log income and log wages, suggesting that the impact was statistically significant in level terms but not in percentage terms. (This could be due to the initial pattern of high income in high-goiter-rate areas and low income in low-goiter-rate areas. A certain absolute change in income for relatively high-income men translates into a relatively small percentage.) Iodized salt also appears to have contributed to higher educational attainment and higher socioeconomic status. On the other hand, there seems to be no impact of iodized salt on employment status and hours worked (results for hours worked not shown), suggesting that the gains were more in terms of quality than in terms of quantity of labor. Table 8 contains a falsification test, in which I run equation (2) for the sample of men aged 25-60 in 1920, before the introduction of iodized salt. I find no impact of the exposure measure on occupational measures observed in 1920. Unfortunately income, wages, highest grade of schooling, employment status and hours worked are not available for 1920.

The results suggest that men with full school-age exposure to iodized salt (21 years of exposure) who were born in states with a 5 per 1,000 prior goiter rate enjoyed about 368 USD (= 3.5 * 21 * 5) more in income in 1960 than men with no school-age exposure who were born in an area with no iodine deficiency. Such a difference is about more than 7% of the average income of the positive-income sample in 1960.

The long-term gains due to iodized salt could be attributed to accumulated gains in human capital investment, given that schooling is found to receive a direct impact from iodization. However, there are at least two other possible, related channels that could play an important part in microeconomic models of social and economic development – personal attitudes and the marriage market, thanks to better (goiterless) looks and higher income-earning powers, as is the case for a Chinese village reported in Hetzel (1989b).

2.7 Conclusion

Using U.S. WWI draft statistics and Census data, I find that iodized salt had both short and long-term effects on human capital outcomes. The results demonstrate significant effects of postnatal iodine supplementation, while the existing economics literature on iodine deficiency has focused solely on in utero treatment effects. The results also suggest that iodine depletion may be one factor contributing to the inferior economic outcomes of many parts of the developing world, such as West African countries. On the other hand, there appears to be a lot of room for low cost public health interventions to bring considerable economic gains. As in the case of iodine supplementation, the gains can be quick and shared by the mass population, even by those who receive the treatment late in their life. In particular, given the extremely low cost of iodized salt, the results in this paper speak directly to the search for effective ways to improve the human capital stock of developing countries, in the face of their tight budget constraint. The cost-effectiveness of iodization stands out in comparison with costly programs like the construction of 60,000 schools in Indonesia during a five-year period that increased schooling by about 0.15 years (Duflo, 2001), or another program in South Africa that reduced class size by 25% and thereby increased schooling by half a year (Case and Deaton, 1999).

The case of salt iodization also addresses the problem of "illusion of sustainability" brought up by Kremer and Miguel (2007) about certain development programs. Kremer and Miguel report failure of cost-recovery from beneficiaries (i.e. program users), health education, and individual mobilization to induce voluntary deworming drug take-up in Kenya. In light of this issue, the U.S. iodized salt intervention exemplifies a sustainable approach through coordination between the private and public sectors. The two sectors joined forces to create and promote a market for iodized salt by means of extensive educational campaigns, while at the same time salt companies were willing to absorb the added cost of only a fraction of a cent per salt box (in today's currency) in order to compete and maintain their market share, and also to do their part for social welfare (Bishai and Nalubola, 2002). The consumer hence faced a zero-cost choice that promised health improvements and that would be integrated naturally into daily consumption, entailing no explicit effort or change in daily routine. This mechanism could also work for similar low cost interventions such as food fortification with other micronutrients that are still lacking in the diet of people living in many developing countries.

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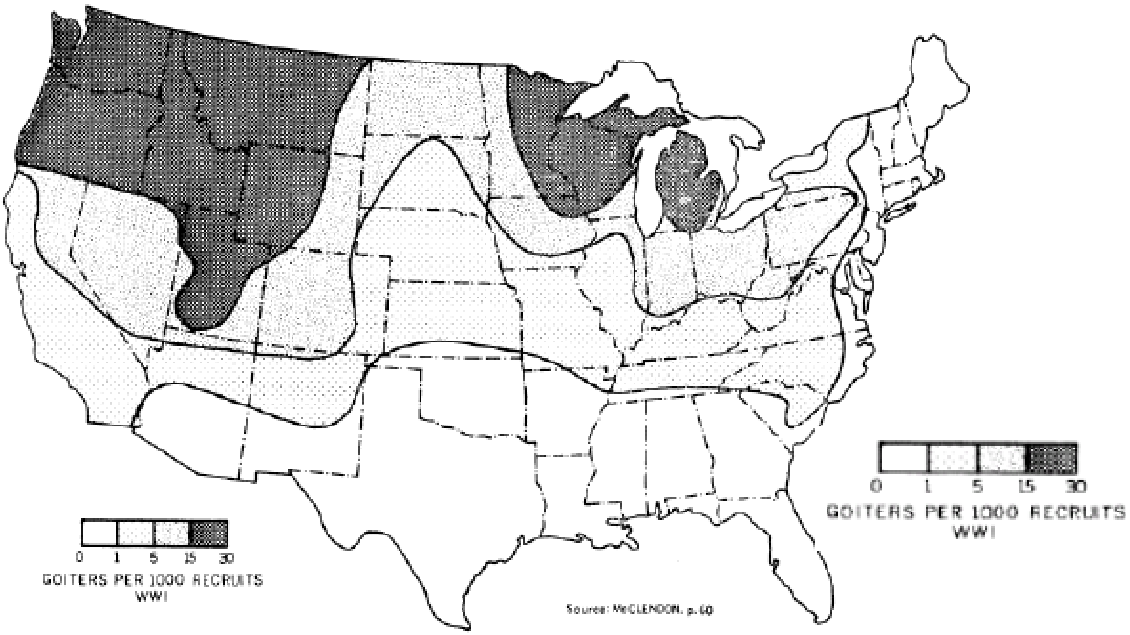

Figure 1. Number of simple goiters per 1,000 men drafted in the US in WWI
Source: McClendon, 1939.

Figure 2. Iodine content in drinking water in the US.
Black area: 1 to 22 parts of iodine per hundred billion parts of water.
White area: 23 to 18,470 parts of iodine per hundred billion parts of water.
Source: McClendon and Hathaway, 1924.
In my empirical work, I do not use the data on water iodine content in McClendon and Hathaway (1924) because the data are available only for 69 towns across the US, often one town per state (as opposed to WWI draft examination data, which are available for 153 substate regions of the US), thus not providing enough variation when I control for state fixed effects.

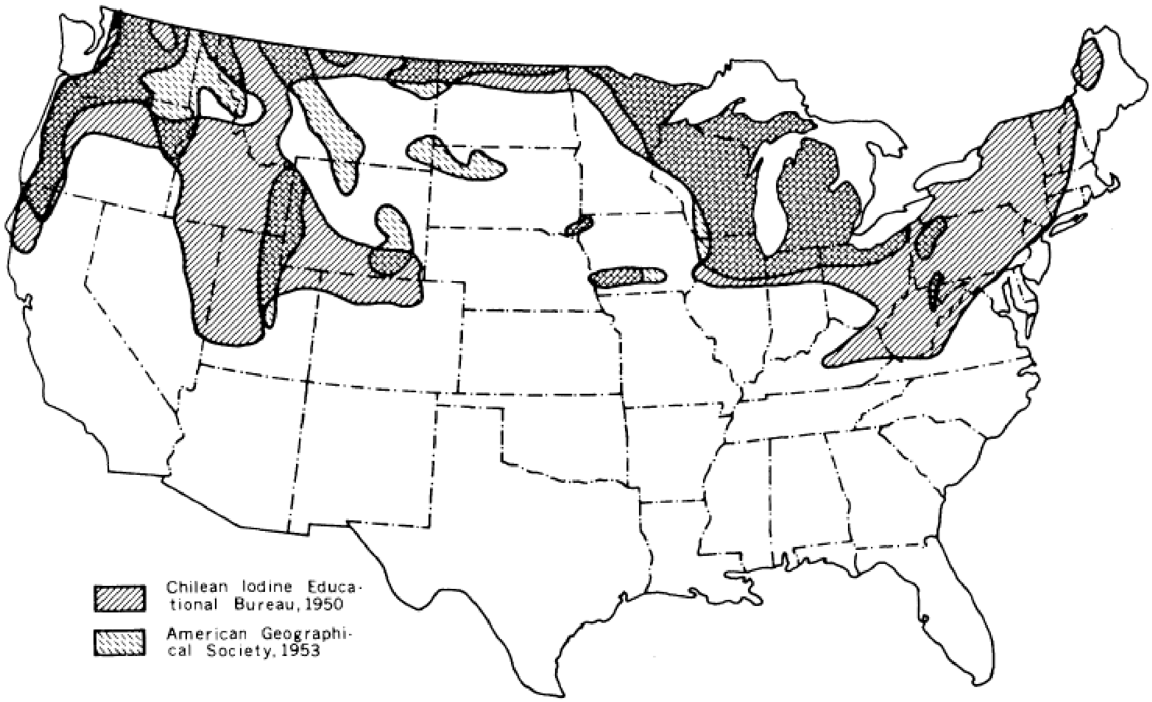

Figure 3. U.S. goiter distribution in the early 1950s
Source: Schiel and Wepfer, 1976

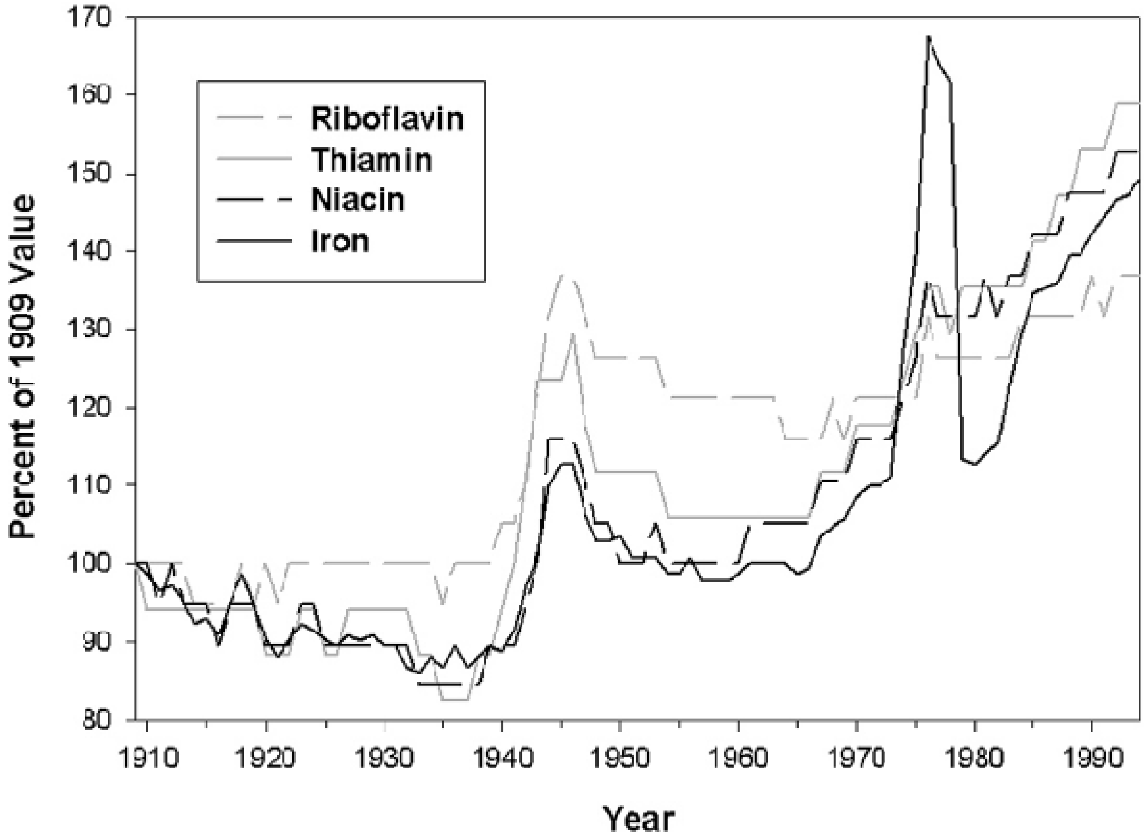

Figure 4. Change in the per capita riboflavin, iron, niacin, and thiamin content of the U.S. food supply between 1909 and 1994.
Source: Backstrand, 2002.

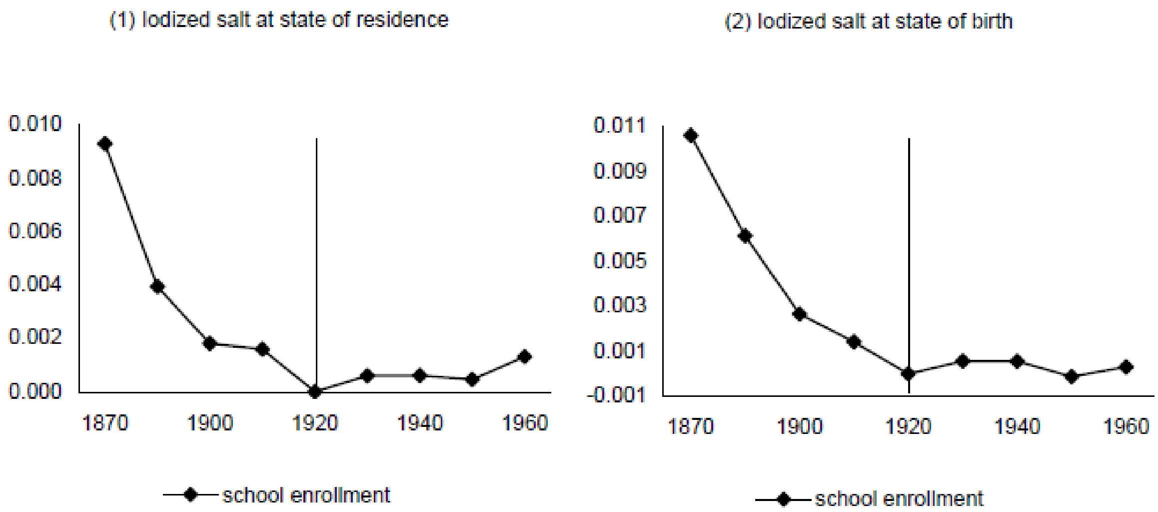

Figure 5. Impact of salt iodization on boys

Figure 5 graphs the pattern of the βt's from estimating equation (1), using goiter prevalence at the state of birth or residence for identification. 1920 is the reference year with β_{1920} normalized to 0. All regressions include age, black, rural/urban, their pair interactions, the interactions between those demographic controls and the year dummies, as well as state fixed effects and census region-year effects.

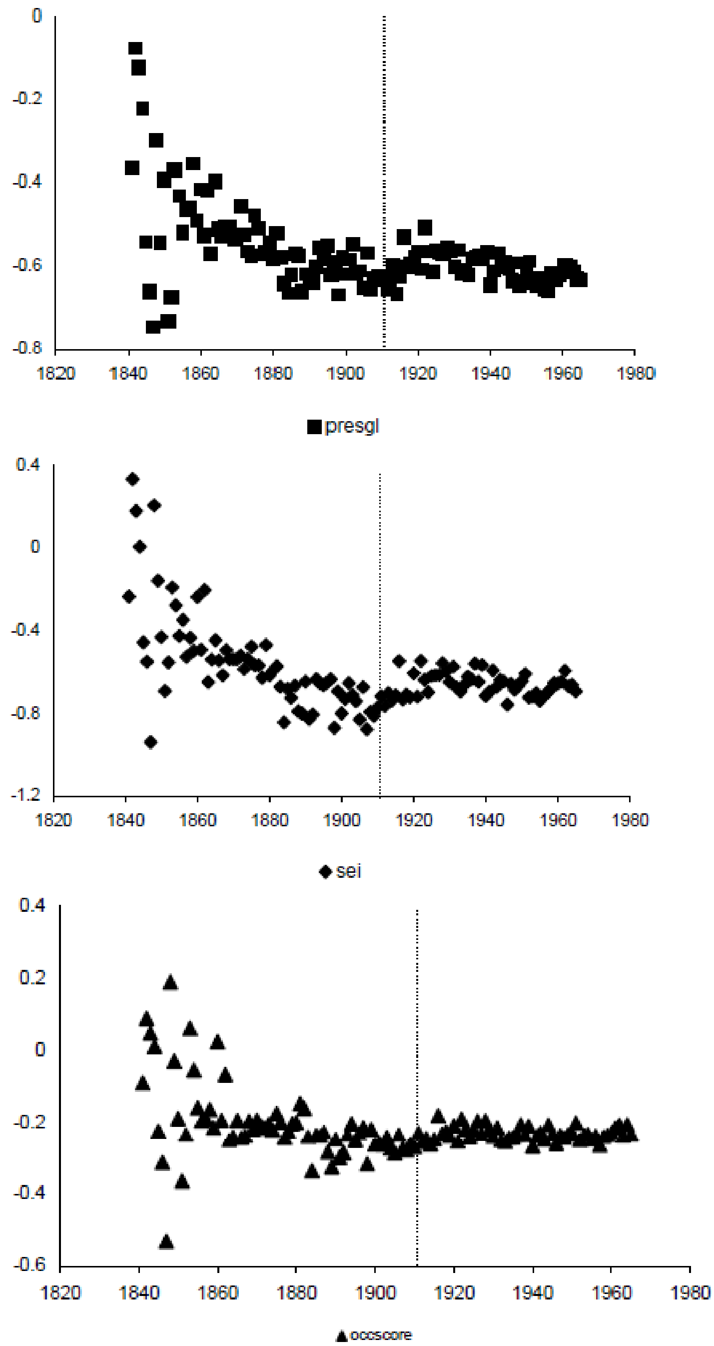

Figure 6. Impact of salt iodization on men

Figure 6 graphs the pattern of the βt's from estimating equation (1), using goiter prevalence at the state of birth for identification. 1920 is the reference year with β1920 normalized to 0. All regressions include age, black, rural/urban, their pair interactions, the interactions between those demographic controls and the year dummies, as well as state fixed effects and census region-year effects. See text for a detailed description of the outcome variables.

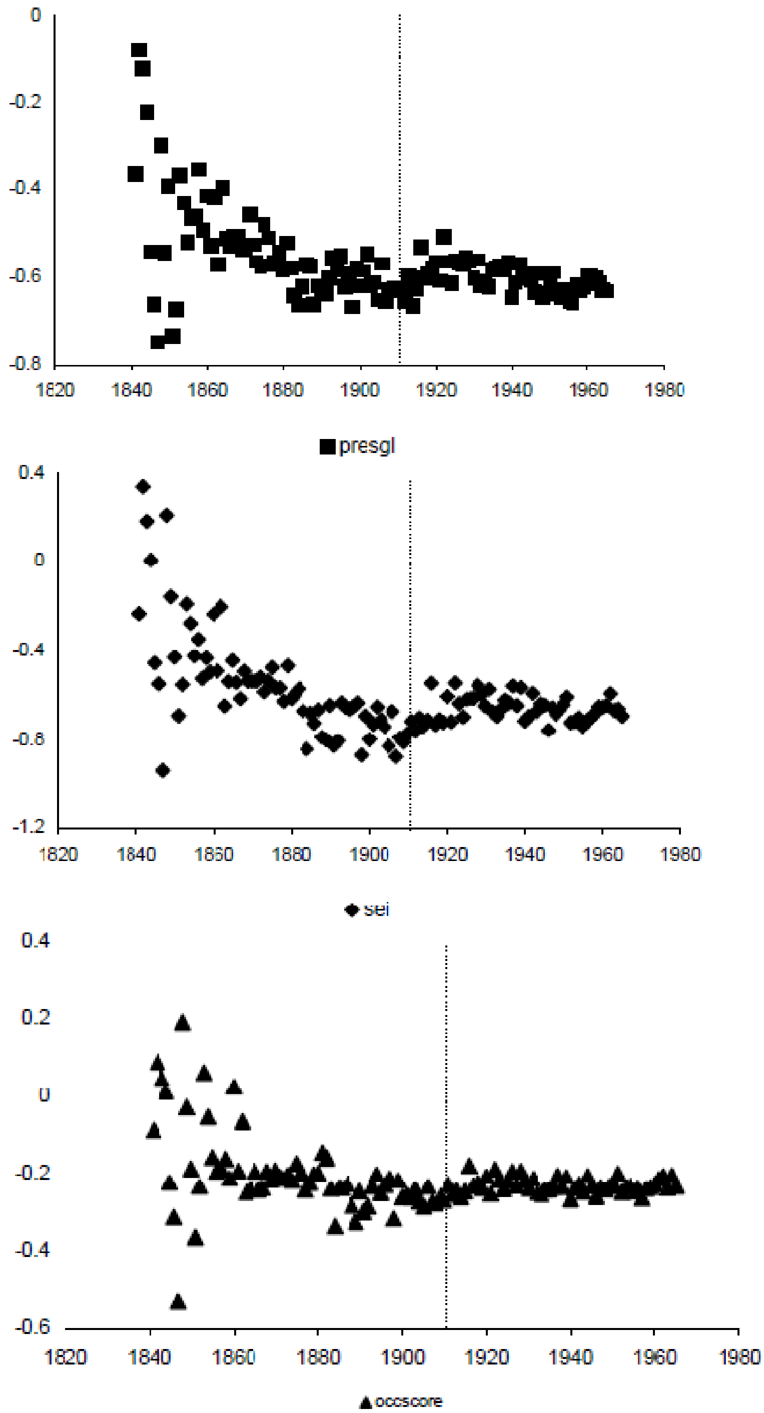

Figure 7. Impact of salt iodization on men, by cohort.

Figure 7 graphs the pattern of the βt's from estimating equation (1), with t being cohort birth years, using goiter prevalence at the state of birth for identification. Birth year 1840 is the reference year. Data are from 1900-1990 censuses, including cohorts born from 1840 to 1965. All regressions include state fixed effects and census region-year effects. See text for a detailed description of the outcome variables.

Table 1. Goiter Prevalence per 1,000 Drafted Men in 1917-18, Detailed Summary Statistics
Source: Love and Davenport (1920)

State	No. of Sections	25th percentile	75th percentile	State	No. of Sections	25th percentile	75th percentile
	New England Region Division				South Atlantic Region Division (cont'd)		
Connecticut	2	0.81	0.95	Maryland	4	0.13	1.32
Maine	3	0.00	0.66	North Carolina	6	1.22	1.80
Massachusetts	4	0.25	0.66	South Carolina	3	0.61	1.23
New Hampshire	2	0.50	0.78	Virginia	4	1.33	5.01
	Middle Atlantic Region Division			West Virginia	2	7.82	8.32
New Jersey	3	0.34	0.62		East South Central Region Division		
New York	8	0.94	2.08	Alabama	5	0.49	0.58
Pennsylvania	7	3.71	5.32	Kentucky	2	1.08	2.40
	East North Central Region Division			Mississippi	2	0.45	0.65
Illinois	8	3.66	7.04	Tennessee	3	0.87	2.67
Indiana	3	6.16	7.78		West South Central Region Division		
Michigan	5	7.13	15.15	Arkansas	3	0.35	0.52
Ohio	4	5.37	6.46	Louisiana	3	0.36	1.19
Wisconsin	4	10.83	16.63	Oklahoma	2	0.61	0.74
	West North Central Region Division			Texas	5	0.00	0.27
Iowa	2	5.50	7.11		Mountain Region Division		
Kansas	2	1.16	1.92	Arizona	2	0.85	1.21
Minnesota	4	5.93	15.71	Colorado	6	4.58	5.40
Missouri	4	3.12	4.21	Montana	2	18.41	24.36
Nebraska	2	1.45	2.60	New Mexico	3	0.46	1.24
North Dakota	3	7.28	11.84	Utah	3	13.81	26.05
South Dakota	3	1.42	12.12		Pacific Division		
	South Atlantic Region Division			California	5	3.67	5.39
Florida	4	0.05	0.35	Oregon	2	22.51	26.44
Georgia	2	0.35	0.65	Washington	3	15.71	29.85

Table 1 does not include eight states that had only one section are not included. The rates reported here are of goiters too large for military collars.

Table 2. Census Data Summary Statistics

	Whole sample	By Goiter Prevalence (goiters too large for >=5‰	<5‰
Goiter rate	0.005	0.011	0.002
	(0.006)	(0.006)	(0.001)
Outcomes of boys aged 8-16 in 1920-1930 sample			
School enrollment	0.890	0.914	0.878
	(0.313)	(0.281)	(0.327)
Read and write	0.955	0.991	0.938
	(0.207)	(0.093)	(0.241)
Outcomes of men aged 25-60 in 1920-1930 sample			
SEI	27.409	27.643	27.278
	(21.944)	(21.946)	(21.941)
PRESGL	35.533	35.237	35.699
	(12.531)	(12.546)	(12.519)
OCCSCORE	24.155	24.723	23.837
	(10.904)	(10.665)	(11.023)
Read and write	0.929	0.956	0.914
	(0.257)	(0.205)	(0.281)

Number of sections: 153. Number of states: 50
Variable means displayed without parentheses. In parentheses are standard deviations.

Table 3. Postnatal Effects of Iodized Salt on Boys in 1870-1930 Sample

	(1) Enroll	(2) Enroll	(3) Read write	(4) Read write
β_{1870}	0.0063**	0.0059**	-0.0001	0.0002
	(0.0030)	(0.0030)	(0.0023)	(0.0022)
β_{1880}	0.0014	0.0011	0.0019	0.0020
	(0.0033)	(0.0031)	(0.0015)	(0.0015)
β_{1900}	0.0018	0.0017	-0.0022**	-0.0022**
	(0.0019)	(0.0018)	(0.0008)	(0.0007)
β_{1910}	0.0011	0.0010	-0.0010	-0.0010
	(0.0009)	(0.0009)	(0.0006)	(0.0006)
β_{1930}	**0.0022****	**0.0030****	**-0.0001**	**0.0002**
	(0.0011)	**(0.0012)**	**(0.0003)**	**(0.0007)**
1920 controls * 1930	no	yes	yes	yes
State * Year FE	yes	yes	yes	yes
Section FE	yes	yes	yes	yes
Cluster: Section	yes	yes	yes	yes
Observations	442,361	442,361	338,760	338,760

Robust standard errors in parentheses. ** significant at 5%; ***

Table 3 reports the βt's from estimating (1) for the 1870-1930 sample. Column headings show dependent variables, which are binary. Different sample sizes reflect differences in availability of data. 1920 is the reference year. All regressions include age, black, rural/urban, their pair interactions, as well as the interactions between those demographic controls and the year dummies. The 1920 section-level aggregate controls include the 1920 section-level average school enrollment and literacy, fraction of black children, fraction of school enrollment that was black (black * school enrollment) and fraction of school enrollment that was female (sex * school enrollment).

Table 4. Postnatal Effects of Iodized Salt on Boys in 1870-1930 Sample – Robustness

	Enroll
β_{1930} - (- β_{1910})	0.0032**
	(0.0015)
Falsification	
(β_{1900} - β_{1880}) - (β_{1880} - β_{1870})	0.0054
	(0.0047)
(β_{1910} - β_{1900}) - (β_{1900} - β_{1880})	-0.0013
	(0.0026)
(- β_{1910}) - (β_{1910} - β_{1900})	-0.0004
	(0.0016)
State * Year FE	yes
Section FE	yes
Cluster: Section	yes

Robust standard errors in parentheses. ** significant at 5%; *** significant at 1%

Table 4 reports the test statistics on the βt's from estimating (1), assigning different census years in the period 1870- 1930 as the reference year, and comparing the n-census-year change in the βt's after the reference year relative to the n census-year change in the βt's before the reference year (see text for more details). Column headings show dependent variables, which are binary. All regressions include age, black, rural/urban, their pair interactions, as well as the interactions between those demographic controls and the year dummies.

Table 5. Postnatal Effects of Iodized Salt on Boys in 1870-1960 Sample – Robustness

	Goiter rate in state of residence	Goiter rate in state of birth
	(1)	(2)
	School enrollment	School enrollment
1930-1920 vs. 1920-1910	0.0022***	0.0020**
	(0.0008)	(0.0009)
1940-1920 vs. 1920-1900	0.0024	0.0032**
	(0.0014)	(0.0015)
1960-1920 vs. 1920-1880	0.0053	0.0065**
	(0.0028)	(0.0031)
1940-1930 vs. 1920-1910	0.0016**	0.0014
	(0.0007)	(0.0009)
Falsification		
1900-1880 vs. 1880-1870	0.0033	0.0011
	(0.0038)	(0.0042)
1910-1900 vs. 1900-1880	0.0020	0.0023
	(0.0028)	(0.0029)
1920-1900 vs. 1900-1870	0.0059	0.0055
	(0.0039)	(0.0033)
1920-1900 vs. 1900-1880	0.0006	0.0021
	(0.0025)	(0.0026)
1920-1910 vs. 1910-1900	-0.0014	-0.0002
	(0.0011)	(0.0014)
Census region * Year FE	yes	yes
State FE	yes	yes
Cluster: State	yes	yes

Robust standard errors in parentheses. ** significant at 5%; *** significant at 1%

The first three rows of results in Table 5 report the test statistics comparing the n-census-year change in the post-1924 βt's relative to the n-censusyear change in the pre-1924 βt's, as explained in the text. The fourth row of results compares the second census-year change in βt after 1924 (1940-1930) to the 1920-1910 change. The last five rows of results report the test statistics on the βt's from estimating (1), falsely assigning different census years in the period 1870-1920 as the reference year, and comparing the n-census-year change in the βt's after the reference year relative to the n-census-year change in the βt's before the reference year (see text for more details). Column headings show dependent variables, which are binary. All regressions include age, black, rural/urban, their pair interactions, as well as the interactions between those demographic controls and the year dummies.

Table 6. Postnatal Effects of Iodized Salt on Men in 1870-1930 Sample

	(1) sei	(2) sei	(3) presgl	(4) presgl	(5) occscore	(6) occscore	(7) read_write	(8) read_write
1870 * Goiter rate	0.100	0.097	-0.027	-0.029	0.070	0.069	0.0010	0.0011
	(0.094)	(0.091)	(0.076)	(0.074)	(0.038)	(0.038)	(0.0025)	(0.0025)
1880 * Goiter rate	0.097	0.094	-0.075	-0.076	0.071**	0.070**	0.0008	0.0008
	(0.058)	(0.057)	(0.045)	(0.044)	(0.035)	(0.035)	(0.0016)	(0.0015)
1900 * Goiter rate	0.021	0.020	-0.043	-0.042	0.040**	0.039	0.0001	0.0001
	(0.040)	(0.040)	(0.024)	(0.024)	(0.020)	(0.020)	(0.0008)	(0.0008)
1910 * Goiter rate	-0.046	-0.047	-0.051**	-0.051**	0.020	0.020	-0.0012***	-0.0012***
	(0.034)	(0.034)	(0.020)	(0.020)	(0.020)	(0.020)	(0.0004)	(0.0004)
1930 * Goiter rate	**0.100*** **	**0.117*** **	**0.057*** **	**0.057*** **	**0.028**	**0.036**	**0.0006**	**0.0000**
	(0.030)	**(0.032)**	**(0.016)**	**(0.015)**	**(0.022)**	**(0.021)**	**(0.0004)**	**(0.0004)**
1920 controls * 1930	no	yes	no	yes	no	yes	no	yes
State * Year FE	yes	yes	yes	yes	yes	yes	yes	yes
Cluster: Section	yes	yes	yes	yes	yes	yes	yes	yes
Observations	73,830	73,830	73,748	73,748	73,829	73,829	74,364	74,364

Robust standard errors in parentheses. ** significant at 5%; *** significant at 1%

Table 6 reports the βt's from estimating (1) for the 1870-1930 sample. Column headings show dependent variables, which are binary. Observations are weighted cell means. Different sample sizes reflect differences in availability of data. 1920 is the reference year. All regressions include age, black, rural/urban, their pair interactions, as well as the interactions between those demographic controls and the year dummies. The 1920 section-level aggregate controls include the 1920 section-level average SEI/PRESGL/OCCSCORE/read_write/labor force status (respectively in accordance with the dependent variable), and the fraction of black. See text for a detailed description of the outcome variables.

Table 7. Postnatal Effects of Iodized Salt on Men in 1870-1930 Sample – Robustness

	(1) sei	(2) sei	(3) presgl	(4) presgl
1870-1880	0.006	-0.005	-0.032	-0.125
	(0.093)	(0.104)	(0.057)	(0.035)
1880-1900	-0.055	-0.061	0.042	-0.021
	(0.045)	(0.046)	(0.038)	(0.037)
1900-1910	-0.049	-0.044	0.001	-0.041
	(0.034)	(0.034)	(0.018)	(0.025)
1910-1920	0.046	0.047	0.051**	0.013
	(0.034)	(0.034)	(0.020)	(0.020)
prior year controls * post year	no	yes	no	yes
State * Year FE	yes	yes	yes	yes
Cluster: Section	yes	yes	yes	yes

Robust standard errors in parentheses. ** significant at 5%; *** significant at 1%

Table 7 reports the test statistics on the βt's from estimating (1) for pairs of census years from 1870 to 1920, with the earlier census year in each pair as the reference year (see text for more details). Column headings show dependent variables, which are binary. All regressions include age, black, rural/urban, their pair interactions, as well as the interactions between those demographic controls and the year dummies. The prior year controls include the lagged section-level average SEI or PRESGL (respectively in accordance with the dependent variable), and the fraction of black. See text for a detailed description of the outcome variables.

Table 8. Long-term Effects on Men Aged 25-60
Level of geographic variation: state of birth.

1960 Sample	Log total income	Total income	Positive total income	Log wages	Wages
	0.00025	3.48984***	3.78652***	0.00026	2.56469**
	(0.00017)	(1.29767)	(1.27501)	(0.00013)	(1.04048)
	N=324,072	N=335,187	N=324,072	N=273,027	N=273,027
	Highest grade	SEI	PRESGL	OCCSCORE	Employed
	0.01929**	0.01260***	0.00750***	0.00245	0.00003
	(0.00937)	(0.00367)	(0.00261)	(0.00189)	(0.00003)
	N=335,187	N=321,186	N=314,050	N=321,186	N=314,883
1920 Sample		SEI	PRESGL	OCCSCORE	
Falsification Test		-0.00307	0.00294	0.00460	
		(0.00586)	(0.00378)	(0.00304)	
		N=174,197	N=173,326	N=174,179	
State FE	yes	yes	yes	yes	yes
Age*State FE	yes	yes	yes	yes	yes
Cluster: State	yes	yes	yes	yes	yes

Robust standard errors in parentheses. ** significant at 5% *** significant at 1%

Table 8 reports estimates of the interaction between exposure to iodized salt (Expa=max(min(21,56-age),0)) and the pre-intervention goiter rate of the state. All regressions include state of birth fixed effects, age dummies, black, rural/urban, their pair interactions, as well as the interactions between the state of birth dummies and age, black and rural/urban. Different sample sizes and outcome variables reflect differences in availability of data. See text for a detailed description of the outcome variables.

Chapter 3

The Impact of Uganda's Introduction of Universal Primary Education on Child Schooling and Work

3.1 Introduction

Primary school fee elimination programs are popular education programs in developing countries, particularly in sub-Saharan Africa. These programs are often dubbed as universal primary education (UPE) programs because they specifically aim at achieving universal primary education, the second United Nations Millenium Development Goal, which is to "ensure that all boys and girls complete a full course of primary schooling" by 2015.[1] Sub-Saharan Africa attracts particular attention in light of this goal. As of 2006, the region accounted for 38 of the 73 million children of primary school age who were out of school, a drop from 103 million in 1999[2]. There can be different explanations for this substantial drop in out-of-school rates, such as improved economic conditions in some countries or international development programs in some others. My paper assesses whether one type of policy, school fee

[1] www.unicef.org

[2] http://www.un.org/millenniumgoals/2008highlevel/pdf/newsroom/Goal%202%20FINAL.pdf

elimination, has contributed to this. By 2008, Burundi, Democratic Republic of the Congo, Ethiopia, Ghana, Kenya, Malawi, Mozambique, Tanzania and Uganda had abolished primary school fees. Despite their popularity, little evidence is available on their actual efficacy. In this paper, I study the impact on child schooling of a UPE program implemented in Uganda in 1997, and investigate whether this impact is associated with an opposite impact on child labor. As a comparison case study, I use data from another country, Kenya, that did not have such fee elimination, to disentangle this policy from other common factors affecting the region.

The UPE being a nationwide program causes an empirical challenge in identifying its consequences. I suggest comparing the trends in outcomes in regions with low enrollment prior to 1997 to those with high enrollment as a way to identify UPE's effects. The key underlying idea is that school expenditures are expected to be a significant hurdle to schooling, and a reduction in expenditures is expected to motivate households to send their children to school more (and maybe to work less). Comparing changes in work and schooling between primary-school ages and other ages, as well as between cohorts with different probabilities of being affected by fee elimination, I find that UPE appears to have increased school enrollment and reduced work activity of children. Furthermore, if we expect children at each age to be more likely to attend school, this would translate into cumulatively more time in school for each child at each age. Therefore I also consider years of schooling as an outcome variable, and find some indication of an increase in response to UPE. Another related outcome is grade attainment, which I will look at among older cohorts as a way to verify changes in their primary school enrollment behavior while in primary school ages. My paper will abstract from consideration of UPE's possible impact on the quality of schooling, such as by raising the student/teacher ratio, although I acknowledge that study of this impact would be important in understanding the full effects of UPE.

The availability of detailed geographic information allows controlling for district-level trends, thereby improving upon existing studies in showing that UPE has helped narrow the enrollment gap between counties with previously poor school enrollment and those with previously high enrollment. Existing studies have merely pointed to the change in national average school enrollment in urban and rural areas to suggest that UPE has improved regional equality in education (see, for example, Deininger, 2003, and Bategeka and Okurut, 2006). Also, to my knowledge, no other study has looked at the impact of UPE on child labor.

School fee elimination implies a reduction in the price of schooling. While there have been few rigorous studies of UPE programs, due mostly to lack of data and because these are fairly recent reforms, research on other programs also aimed at reducing schooling prices abounds. Families are found to respond to direct reductions in the cost of education either through subsidies to attend private schools (Angrist et al. 2002, 2006), reduced user-fees (Barrera-Osorio et al., 2007; this incentive is the closest to UPE), or scholarships (Kremer, Miguel, and Thornton, 2007). There is also extensive evidence that families respond to inducements to attend such as school meals and direct cash incentives (see Schultz, 2004; Vermeersch and Kremer, 2005; Schady and Araujo, 2006, among others).

The case of conditional cash transfer programs is particularly prominent, both in implementation and in research. Cash transfers in countries like Mexico (with the well-known and well-researched Oportunidades (PROGRESA) program) where primary and secondary education is free, imply direct increases in income, as well as reductions in the shadow price of schooling, as long as the household is credit constrained. Skoufias and Parker (2001) find a clear positive impact of PROGRESA on school enrollment, with much larger increases among girls than boys. These increases in schooling are associated with reductions in work. Barrera-Osorio et al. (2008) analyze a conditional cash transfer program in Bogotá, Colombia, and conclude that the program positively affected school enrollment of the recipients, but that this came, in part, at the expense of their siblings, who were more likely to drop out of school and enter the labor market.

It is important to note some critical differences between UPE and cash transfer programs. One key difference is that cash transfers tend to be quite larger than just school fees, such as PROGRESA cash transfers to improve primary school enrollment already provided virtually for free by the government. Another important difference is that most cash transfers are now given to the mother, based on empirical evidence that children benefit more when the mother, rather than the father, is in charge of the cash. Meanwhile school fee reduction or elimination applies to the household collectively, under the assumption that parents make schooling decisions for their children. Moreover, cash transfer packages may come with non-schooling conditions, such as regular health check-ups, which may cause any impact on schooling to differ from that of a transfer package with cash only, given that both healthcare and education are human capital investments, and subject to time and budget constraints.

Additionally, another way in which the shadow price of schooling may change is through changes in wages paid to children. This channel has both an income effect and a substitution effect that move in opposite directions, causing the prediction of the net impact on schooling to be ambiguous. A number of recent studies (such as Duryea and Arends-Kuenning, 2003; Beegle et al., 2006; and Kruger, 2007) have analyzed the effects of temporary fluctuations in labor income on children's outcomes, and have suggested that the substitution effects of such fluctuations tend to be greater than the income effects. In other words, unexpected, temporary improvements in economic conditions (accompanied by higher wages rather than pure income rises) may actually increase children's employment and deter schooling.

Despite the abundance of research on the effects of different kinds of schooling price reductions or in changes in economic circumstances, there has been only little well-identified evidence of the impact on enrollment in the specific case of reduced school fees, partly due to lack of data. (Such voucher and scholarship programs as those studied by Angrist et al., 2002, and Kremer et al., 2009, are essentially school fee reductions, but are also different in that they are tied to performance, thereby potentially resulting in different incentives.) Barrera-Osorio et al. (2007) employ a regression discontinuity design framework to evaluate the impact of a fee reduction program launched in 2004 by the city of Bogota, Colombia, and find that the program raised enrollment of primary-school-age students by about 3 percent, and of high-school-age students by about 6 percent. No gender differences are detected. However, the Bogota context may differ greatly from a typical sub-Saharan African context, given the differences in many important aspects like the baseline rate of school enrollment, as well as in culture and economic development in general.

Al-Samarri and Zaman (2007) use incidence analysis of Malawi data in 1990/91 and 1997/98 to show that enrollment at primary and secondary levels improved after school fees were abolished in 1994, and that such improvements were greatest for the poorer socio-economic groups[3]. Unfortunately the two datasets come from different sources and are not compatible, and there is practically no comparison age group (as both the primary and secondary levels were "treated", and very few youths followed tertiary education). The authors could only provide average and distributional summary statistics at the national level, not controlling for potential concurrent factors. Grogan (2008) uses only post-UPE data from the 2000 Uganda Demographic and Health Survey and the 2001 Education Data Survey to suggest that Uganda's UPE program has helped increase the probability that a child begins school before age nine, but does not account for inherent cohort-specific differences

[3]The authors show that gross and net enrollment rates more than doubled for the poorest per-capita-consumption quintile, whereas the increases were less than 10% for the richest quintile. On the other hand, I look at participation rates in my paper.

due to the availability of only one year of data. More econometric rigor can be found in Deininger (2003), who uses Ugandan household survey data in 1992 and 1999 to provide evidence of improved enrollment and reduced inequalities in attendance related to gender, income, and region. However, limited geographic information allows the author to only discuss inequality between urban and rural areas.

While there has been some existing research on schooling impacts, no previous study of UPE programs has considered children's labor supply as an outcome variable. Better understanding of how child labor may respond to lower schooling costs would potentially help better understand the importance of child work relative to the household's credit constraint, as well as the potential role of work in displacing schooling. While the prediction of increased school enrollment in response to reduced schooling prices is straightforward (as long as schooling is not a giffen good), it is more ambiguous in the case of the response in labor supply. For example, with a simple theoretical model, Ravallion and Wodon (2000) show that if there is utility-compensated substitution between child labor and leisure, then a decrease in the price of schooling is not guaranteed to bring down child labor since parents may arrange for schooling to take up non-labor time. They study the school-price change induced by a targeted enrollment subsidy in rural Bangladesh and empirically demonstrate that the subsidy increased schooling by far more than it reduced child labor, thereby questioning the seemingly common view that child labor comes largely at the expense of schooling and future prospects of the child. Skoufias and Parker

(2001) replicate this finding for the case of PROGRESA. However, the sub-Saharan African context could differ greatly from the South Asian or Mexican context.

In this paper, I use the recently available 2002 Ugandan census data, in combination with the 1991 census, to evaluate the program using a different approach than previous studies. My strategy requires that areas with low previous enrollment benefited more importantly from the UPE than areas with high enrollment, taking advantage of county-level geographic information to purge out region-level changes over time and explore within-region changes. Previous studies, such as Grogan (2008) and Deininger (2003), have only looked at the national average as opposed to cross-area comparison, hence not accounting for the possibility that some other concurrent national change could be driving the findings. For example, an overall increase in work opportunities for older ages could distract those ages from school, making it seem as if primary-school ages were experiencing a relatively more positive change in school enrollment.

In my empirical strategy, I compare change in outcomes (i.e. change between before and after UPE) for primary school-age children with change in outcomes for older children, and put this comparison in contrast between counties with low pre-UPE enrollment rates and counties with high pre-UPE enrollment rates. Using older children as the comparison group is essential because I could not say that UPE has any impact if any cross-area difference in change in outcomes found for primary-school-age children is also found for older children. Allowing for the possibility that counties that differ in prior enrollment rates could also systematically differ in their trends across ages, I investigate whether there is a break in the trends that coincides with the introduction of UPE, assuming that without UPE, any difference in trends would stay similar across all ages. I find that UPE increases school enrollment and decreases labor supply of primary-school-age children, in particular girls. (In principle, primary school covers ages 7 to 13. However, due to the overwhelming incidence of late enrollment, I consider ages 14-16 among those to be affected as well.) The impact on labor supply seems to be approximately the same as the impact on school enrollment, suggesting that child labor plays a relatively important role in household income (i.e. relative to child schooling), and appears to have some displacement effect on schooling. The results for years of schooling as the outcome variable suggest that the increase in school enrolment seems to translate into more time in school.

One concern with this approach is the availability of only two years of census data: ideally we would want to have many years of data to make sure that any break we may see between 1991 and 2002 is not observed in previous periods, but such data is not available. As an alternative, I look at attainment of grades 1 to 3 across ages, from age 7 to age 40. The idea is that changes in school enrollment in primary-school ages would be replicated in changes in grade attainment observed afterwards, so that, if UPE has an impact, we would see that there was a more positive change between 1991 and 2002 in grade attainment among those expected to have started school or been enrolled in grades 2 or 3 after 1997 in areas with lower pre-UPE enrollment rates. The grades considered for this exercise are limited up to grade 3 only, because of the likelihood that higher grades would reflect a more complicated course of study that may involve periods of drop-out and re-enrollment, causing more imprecision in inferences about past enrollment.

I also use 1989 and 1999 census data of Kenya, a neighboring country, to replicate the regressions, and then compare the results for Kenya with those for Uganda. To the extent that my findings reflect a significant effect of UPE in Uganda, no similar pattern should exist in Kenya. However, one caveat is in order: in 1988, Kenya re-introduced school levies to cover the cost of instructional equipment and materials (Bedi et al, 2002). Ideally we would want there to be no significant policy or event in place at the time that may have caused changes in school enrollment in Kenya. The re-introduction of school levies in fact should be expected to have an impact that contrasts the impact of school fee elimination in Uganda.

The paper proceeds as follows. Section 2 provides some background information on the Ugandan UPE program. Section 3 lays out the empirical framework of the analysis. Section 4 describes the data and some summary statistics. Empirical results are presented in Section 5, and Section 6 concludes.

3.2 Background

Uganda introduced UPE for all primary grades (from grade 1 at age 7 to grade 7) simultaneously in 1997, amid the background of a spiraling AIDS crisis and civil war in the north of the country. In fact, several previous crises since the country's first elections as an independent nation in 1962 had shattered the economy and education system. Recognizing the education failings, the government of Uganda in 1987 convened an Education Policy Review Commission, which had the mandate to make policy recommendations for all levels of education, and which, in their report to the government in 1989, recommended universalization of primary education as soon as feasible. Elimination of school fees at the primary level was likely accelerated by the first direct elections for President of Uganda, which took place in the spring of 1996. In December 1996, after being elected, President Musveni announced that school fees would be eliminated in January 1997, coincidental with the new school year. An enumeration and advertising campaign was undertaken, and school fees were waived for all primary school students, regardless of their ages and how many siblings were also attending school (Grogan, 2008).Support funds for Uganda's UPE programme was provided by the World Bank, the Netherlands government, The United States Agency for International Development (USAID), Denmark and Britain.

The announcement of UPE in late 1996 committed the government to paying tuition fees at the rate of 5,000 Ugandan shillings per pupil per annum in the first three years of schooling, and 8,100 Ugandan shillings for the fourth to the seventh classes. Other costs of schooling, such as transportation and uniforms, remained the responsibility of families. Although I could not find statistics of these costs for Uganda, a Kenya study on the impact of distributing school uniforms on education by Evans et al. (2009) notes that uniforms cost about 400 Kenyan shillings, which is approximately 10,000 Ugandan shillings. Expenses net of tuition therefore seem to remain high. However, uniforms do not appear to be effectively mandatory at all in Uganda, so ultimately non-tuition expenses faced by households may not be prohibitive. To put the fees in the context of local salaries, in 1999, a teacher in a government-aided school in Uganda earned about 75,000 shillings per month[4] (Uganda Ministry of Education and Sports, 1999), and in 2005, the average Ugandan earned about 50,000 shillings per month (BBC News, 2005[5]), and during these times, the poorest income quintile of the population earned about one-third of the average income[6]. Deininger (2003) studies Ugandan community-survey data of 1992 and 1999, and notes that urban households were opting out of the public system in favor of more expensive private schools, and therefore the UPE program was mainly beneficial to poor rural households that sent their children to public schools at reduced costs. It is this differential impact across areas that I exploit in my analysis.

3.3 Empirical Strategy

Ideally, I would like to estimate the following structural equation:

(1) $S_{cdyk} = \alpha_0 + YR02 + \theta_c + YR02 * \lambda_d + \alpha_1 P_{cdyk} + \varepsilon_{cdyk}$

[4]To roughly reconcile this figure with student fees, note that besides tuition fees, Ugandan school often also charge fees for parent-teacher associations, which can amount to 10,000 shillings per student per annum. It would be very useful to know how these parent-teacher association fees vary across areas, but unfortunately I have not been able to uncover this information.

[5]http://news.bbc.co.uk/2/hi/africa/4112560.stm

[6]http://earthtrends.wri.org/pdf_library/country_profiles/eco_cou_800.pdf

where S_{cdyk} is the outcome of individual k in county c of district d (there are 161 counties in 57 districts in Uganda) and in year y; $YR02$ is a dummy for year 2002, taking into account the average change between the years 2002 and 1991 across all counties; θ_c's are county fixed effects, accounting for fixed differences across counties; λ_d's are district fixed effects, so that $YR02 * \lambda_d$ controls for district-level changes over time; P_{cdyk} is the price of education faced by child k in year y; and ε_{cdyk} are the error terms. Because inequalities in Uganda predominantly follow a North-South pattern, the term $YR02 * \lambda_d$ takes care of the concern that non-UPE changes in socio-economic conditions or policies that correlate with the North-South pattern and that are related to schooling may drive the results. The impact of P_{cdyk} thereby is identified within districts.

However, estimation of equation (1) is not possible given that UPE was introduced nationwide, and although it is highly possible that UPE has effectively caused differences in the schooling prices faced by different groups of students, I cannot measure such differences. Moreover, even if I could measure schooling costs, α_1 would not generally have a causal interpretation because P_{cdyk} is likely to be a choice variable due to the choice of school by the student's household. The OLS coefficient conflates the true impact of schooling costs and the impact of omitted variables that may be correlated with both schooling costs and the outcome. In the case of the Ugandan UPE program, implicit variation in changes in the price of education faced by the student arises from a sharp policy shift, and thus could be considered exogenous. Thus, even without the benefit of a randomized experiment, a causal interpretation can be obtained by combining the expected variation in treatment intensity across areas, and the variation in treatment exposure across cohorts. It is important to note though that this obtained causal estimate would not be exactly the same as α_1 in equation (1) in that it would not measure the average impact of an additional unit of schooling cost on the outcome; it would only tell us whether changes in schooling price have significant effects.

Specifically, in the place of equation (1), to measure UPE's impact, I exploit the possibility that lowering the cost of schooling may be particularly relevant for households unable to pay for public school before the program's implementation. Given the universal nature of the intervention, my empirical strategy uses county-level data, and tests the hypothesis that counties with lower primary-school enrollment rates among 7-14 year-olds (hereafter referred to as LPE counties) prior to UPE would have more to gain in schooling from the elimination of primary school fees than counties with higher prior enrollment rates among 7-14 year-olds (hereafter referred to as HPE counties). (Note that the comparison I am trying to make is in terms of absolute percentage points (to identify UPE's contribution to schooling convergence between areas), not in terms of percentage change, which would be a stronger comparison of UPE impact, given that the baseline is higher for HPE counties.) In other words, I compare the difference in outcomes between LPE and HPE areas in 1991 with that difference in 2002 for each age, and then examine how that comparison may differ from one age to another. In particular, I want to see if there is a break in the trend of this comparison at some point between primary-school ages and older ages. This empirical strategy relies on the key identifying assumption that, in the absence of UPE, any difference between age groups across the distribution of prior enrollment rates would have stayed the same before and after UPE.

I expect the trend break to happen at the whereabouts of age 15, given that 14 is the official maximum primary-school age, and Ugandan children are known to often enrol late in school, as mentioned before. Although it is true that many children older than primary school ages may also take advantage of the new opportunity[7], I assume that children of the appropriate ages are more extensively impacted in a significant enough way, and that the greater a child's age than primary-school age, the less likely he/she is to use UPE. Thus we may expect to see some impact on children a couple of years older than 14, which is the official maximum age for primary school, but any impact is expected to be significantly lower for ages further beyond that.

To make it easy to compare the impact of UPE across ages, I employ the following estimation equation for each pair of ages from age 7 to age 24:

$$(2)\ S_{cdyk} = \beta_0 + YR02 + \theta_c + YR02 * \lambda_d + \beta_1(YR02 * ENR91_c) + \varepsilon_{cdyk}$$

where P_{cdyk} in (1) has been replaced with $(YR02 * ENR91_c)$ with $ENR91_c$ being the 1991 county-level enrollment rate of children aged 7-14 (primary school ages). The key variable is $(YR02 * ENR91_c)$, which measures the differential impact

[7] In sub-Saharan Africa, substantially more children of secondary school age attend primary rather than secondary school. (http://www.un.org/millenniumgoals/2008highlevel/pdf/newsroom/Goal%202%20FINAL.pdf)

of UPE among counties within a district, as district-level changes are controlled for. The expectation is that when schooling is the dependent variable, a discreet change in β_1 would happen around ages 15-16, and $\beta_1$would be significantly more positive some these ages onward (recall an earlier note above on late enrollment). The opposite is expected when work is the dependent variable. To account for possible serial correlation over time within areas, I allow for an arbitrary variance–covariance matrix in the error structure within each county.

Even after controlling for district-level changes, at least two important concerns remain regarding impact idenfication here. One concern is the possibility of a within-district convergence trend between LPE and HPE areas that had existed before and was only continuing between 1991 and 2002, which cannot be examined given the lack of data prior to 1991. Comparing primary-school ages with the other ages helps partly alleviate this problem by purging out average convergence trends. However, it is possible that pre-existing convergence may have happened more intensely among younger age groups even in the absence of UPE since older ages may have been expected to invest less in education, and start working at some point soon after primary school. Another concern is that between 1991 and 2002, some concurrent change in the socio-economic conditions may have happened at the county level in such a pattern that coincided with the distribution of pre-UPE primary-school enrollment, and disproportionately benefited younger children. This would cause the identifying assumption of the econometric model to be violated.

I try to address the above-mentioned problems in three ways. First, to the extent that the LPE-HPE differential impact may be correlated with changes that may be differential between low- and high-socioeconomic statuses (SES) across different ages, I include in one specification an interaction between pre-UPE county-level SES and year 2002 for each age group. (In fact, due to limited availability of measures of income, the SES indicator that I use is the percentage of county electricity coverage. The correlation coefficient between pre-UPE county-level school enrollment and electricity coverage is 0.3, so multicollinearity may not be severe.)

Second, in order to investigate how things may have looked in Uganda in the absence of UPE, I replicate the estimations using census data of Kenya, a neighboring country, in 1989 and 1999 to see if there is any suggestive evidence of violation of the key identifying assumption, given that UPE was not introduced in Kenya until 2003.

Third, as a check on pre-existing convergence, I look at attainment of grades 1 to 3 across ages, from age 7 to age 40. The idea is that changes in school enrollment in primary-school ages would be reflected in changes in grade attainment observed afterwards, so that, if UPE has an impact, we would see that there is a more positive change between 1991 and 2002 in grade attainment among those expected to have started school or been enrolled in grades 1, 2 or 3 after 1997 in areas with lower pre-UPE enrollment rates. The grades considered for this exercise are limited up to grade 3 only because of the likelihood that higher grades may reflect more of individual ability (as opposed to school enrollment), which I do not address here, as well as a more complicated course of study that may involve periods of drop-out and re-enrollment, causing more imprecision in inferences about past enrollment. In other words, I estimate equation (2) with attainment of grade 1/2/3 as the dependent variable for each age from age 7 to age 40 to see if there is any break in the trend of the key coefficient around those ages observed in 2002 that correspond to the whereabout of ages 15-16 in 1997 when the UPE program started, as ages 15-16 are generally the last ages to be effectively impacted by UPE (taking into account late enrollment), and most of those older than those ages would potentially be too old for grades 1-3, and therefore too old to show impact of UPE in terms of attainment of those grades. If such a break in the trend of the key coefficient is indeed found, we would be less concerned about pre-existing convergence in school enrollment, as these estimations provide retrospective inferences about school enrollment.

3.4 Data

The main data I use come from the 1991 and 2002 Ugandan censuses. Figure 1 provides a nation-wide picture of school enrollment of ages 7 to 24 in Uganda in 1991 and 2002. The graphs suggest late enrollment of children at young ages, so that the rate peaks around age 11 (and stays at that level up to around age 13/14), but not earlier. For both males and females, the increase between the two census years is largest for the young ages, suggesting reductions in late enrollment. We can also see a closing in the gap between males and females up to around 15 years old, as the 2002 curve is much more similar for the two genders over these ages than the 1991 curve.

Since work variables are available only for ages 10 and above in 1991, I limit the sample size to ages 10 and above in regressions with the work variable on the left hand side. To work is defined as either to do work outside of the house (either for pay in cash or in kind, or unpaid), or to do housework. Table 1 presents some summary statistics that are organized for the purposes of first-hand comparison of changes in outcomes between 1991 and 2002 in LPE counties versus changes in HPE counties. In Table 1, 0.66 is the median county-level enrollment rate of 7-14 year-olds in 1991 (the mean is 0.63). LPE counties are those with the 1991 average county enrollment rate of 7-14 year-olds below 0.66, and HPE counties are those with the 1991 average county enrollment rate of 7-14 year-olds equal to or above 0.66. There are 161 counties in 57 districts. Figures 2 and 3 complement Table 1 by contrasting the 1991 and 2002 kernel densities of county-level outcomes of LPE counties versus HPE counties.

Higher rates of school enrollment are systematically associated with lower rates of working. (This holds true also if we look at school enrollment among 10-14 year-olds instead of 7-14 year-olds, so that the age range for schooling is compatible with the age range for work of primary-school-age children). Columns 3 and 6 contain the result of a test of the difference between 1991 and 2002 for each subsample and outcome variable. All the 2002-1991 changes in the means are statistically significant. Looking at school enrollment and work, we see larger improvements in 2002 for primary-school-age children in counties with lower enrollment in 1991, and for girls. The difference between 2002-1991 change in LPE and 2002-1991 change in HPE seems less positive for older ages (which is consistent with our expectation of UPE impact). For example, the 2002-1991 change in school enrollment for 7-14-year-old boys is 0.22 in LPE and 0.16 in HPE (so that the difference is 0.06), whereas for 15-20-year-old males is 0.18 in LPE and 0.15 in HPE (so that the difference is 0.03). On the other hand, the 2002-1991 change in work for girls of ages 10-14 is -0.28 in LPE and -0.14 in HPE (so that the difference is -0.14), whereas for females of ages 15-20 is -0.16 in LPE and -0.13 in HPE (so that the difference is -0.03). The pattern holds if I set the older age range to 15-18, in case one is concerned that ages 19 and 20 are too old for comparison with primary-school ages. However, a caveat here lies in the difference between comparison in terms of change in absolute percentage points and comparison in terms of percentage change.

Comparing the test results in column 3 across subsamples shows that females appear to have also gained by more in years of schooling. However, unlike for school enrollment, only primary-school-age females in LPE counties gained more than in HPE counties, whereas the opposite is true both for primary-school-age males, as well as for males and females of ages 15-20. It is perhaps worth noting that 2002 may be somewhat too soon to examine changes in years of schooling for older ages. For example, those observed at ages 17-20 in 2002 were 12-15 years old in 1997 when

UPE was put in place, having only very few years in the primary-school-age range to respond to UPE, and thereby not showing a significant difference. One thing that stands out in the statistics is that in both 1991 and 2002, the average number of years of schooling for 15-20 year-olds is more than twice the number for 7-14 year-olds. Given that on average, the years of schooling took place at the primary level (less than 7 years), this pattern is consistent with late enrollment, so that the number of (primary) schooling years appears highly skewed towards older ages, even when the average is only 3-4 years of schooling among 15-20 year-olds, such as in 1991.

To complement the means statistics in Table 1, Figures 2 and 3 respectively present the 1991 and 2002 distributions of county-level schooling and work outcomes for males and females in primary-school ages and older ages. Each figure is a panel of graphs, and each graph contrasts the distribution of the county-level outcome in LPE counties (red line) with that in HPE counties (blue line). In general, the distributions of HPE counties tend to be quite less skewed and more concentrated around the mean than the distributions of LPE counties, suggesting more inequality among LPE counties themselves. No systematic difference in the form of distribution between males and females stands out. The gap between LPE and HPE distributions for females does not seem different between primary-school ages and older ages, whereas this gap is much larger among primary-school-age males than older males. Males seem to catch up across areas at older ages but females do not. For both primary-school-age males and females though, the gap in school enrollment and work between LPE and HPE counties closes up quite clearly in 2002. But that is not the case for years of schooling.

In Table 2, I also provide summary statistics for Kenya based on its 1989 and 1999 census data, in order to have a sense of the differences in context between the two countries, which will need to be kept in mind as their results are compared. The most detailed level of geography available for Kenya is the district, i.e. less detailed than for Uganda. Kenyan primary school spans ages 6/7 to 14/15, quite similar to Ugandan primary school. The median county-level school enrollment rate of 7-14 year-olds in 1989 is 0.87, while the mean is 0.74, suggesting more skewedness than in Uganda. The inequality in school enrollment and years of schooling between low and high 1989-enrollment rate areas in Kenya appears to worsen in 1999, indicating divergence, as opposed to convergence in Uganda. Interestingly, school enrollment decreased in all subsamples, while years of schooling increased for high 1989-enrollment rate areas, suggesting that divergence may be a relatively new phenomenon, so that the current year's enrollment may be low, but the accumulated number of years of schooling is still higher than in 1989 due to those years gained after 1989 and before the current year. Also, both school enrollment and work decreased by more for low 1989-enrollment rates area (in fact, except for 15-20 females, work increased for all the other subsamples in high 1989-enrollment rate areas), which is in fact consistent with the re-introduction of school levies in 1988 to cover the cost of instructional equipment and materials. In general, primary-school-age boys and girls in Kenya seem to be considerably more likely to work than their Ugandan peers.

3.5 Empirical Results

Tables 3, 4 and 5 report results from regressions that are estimated based on equation (2), for each pair of ages from age 7 to age 24, with school enrollment, work and years of schooling as the outcome variables, respectively. Besides the 2002 year dummy and the interaction between the 2002 year dummy and the 1991 county enrollment rate ($YR02 * ENR91_c$), all regressions also include county fixed effects to account for fixed differences across counties, and (district * 2002) fixed effects to account for district-level changes over time, as formulated in equation (2). The key coefficient of interest is β_1, the coefficient of ($YR02 * ENR91_c$), which measures how 2002-1991 changes happened differentially across the distribution of 1991 county-level school enrollment. All standard errors are clustered at the county level, and individual sample weights are applied. Tables 3-5 are complemented by Figure 4, which graphs β_1 across ages for each outcome variable to show more clearly changes in the trend of the coefficient.

The results show that, as hypothesized, when school enrollment is the outcome variable, β_1 is significantly negative for ages up to 15-16, especially for girls, and then becomes significantly less negative from ages 17-18 onward (even positive for ages 21 through 24). As mentioned before, although the official maximum age for primary school is 14, it is common for Ugandan children (and Sub-Saharan African children in general) to start school one or two years late. It therefore seems reasonable for the expected break in the trend of the coefficient β_1 to happen between ages 15-16 and ages 17-18. Since district-level changes over time are controlled for (many of the omitted (district * 2002) coefficients are statistically significant, indicating that district-level changes are important), the estimates signify the within-district impact of UPE. The negative sign implies that those counties with higher enrollment rates in 1991 saw smaller improvements in 2002, which is the differential impact that we expect to see if UPE is effective and helps reduce regional inequality in school enrollment. The opposite sign is observed when work is the outcome variable, consistent with there being a displacement relationship between work and schooling.

The pattern of the results for years of schooling, shown in Table 5, is quite similar to the school enrollment pattern, with a break in the trend of β_1 at age 17-18. This means that the LPE-HPE differential impact of UPE is small for those who were 12-13 years old in 1997 when UPE was implemented. This in turn implies that school enrollment did not increase significantly after 1997 for the then-12-13 years old, which in fact is not surprising, considering that UPE had just started, and they had only a couple of years remaining in the usual primary-school age range. When looking at the school enrollment estimates, one may think that the lack of a significant response in terms of school enrollment among the 17-18 years old in 2002 (12-13 years old in 1997) may be because they had responded significantly enough to UPE in the years before. However, the years of schooling estimates suggest that that is not the case.

Figure 4 clearly shows a change in the trend of β_1 across ages for school enrollment and work, with the strongest change observed for females. However, the trend of β_1 for years of schooling is less clear. In the years of schooling graph, the downward slope in the first half of each line, up to age 13-14/15-16, implies that the differential UPE impact between LPE and HPE counties increases as the age increases in that range. This perhaps reflects the fact that, since those boys and girls are still in school, the older ages naturally have had more time to respond to UPE than the younger ages. For example, 7-8 year-olds have had only about one year of schooling to show any impact of UPE, whereas the 13-14 have had 6-7 years of accumulated impact of UPE. It would be a much better analysis of the years of schooling outcome if we could look at the individuals in full adulthood, when they have potentially completed all their schooling.

To the extent that the LPE-HPE differential impact may be correlated with changes that may be differential between low- and high-socioeconomic status (SES) across children of different ages, I estimate an additional specification that includes an interaction between pre-UPE county-level SES and year 2002 for each age group. Due to lack of a direct measure of income, the SES indicator that I use is the percentage of electricity coverage in the county. The patterns of the key coefficient β_1 are graphed in Figure 5. Comparing Figures 4 and 5, we can see that the key findings are preserved. To address concerns about the county-level correlation between the baseline SES proxy and the intervention, I provide Figure 6, which graphs the coefficients of ($YR02$ * 1991 county-level electricity coverage) to see if they bear the same patterns as observed for β_1. We do not see the same break in the patterns between the age pairs 15-16 and 17-18 as apparent in Figure 5.

As for the estimated magnitude of UPE impact, the results in Tables 3 and 4 suggest that boys of ages 7-16 in a county with 50% school enrollment in 1991 gained somewhere around 3 percentage points to 6.5 percentage points more in school enrollment and 0.06 to 0.18 more in year of schooling, and cut down on work by around 3.3 percentage points to 5.5 percentage points more than those in a county with 60% school enrollment in 1991 (multiplying the estimated β_1 by 10%). For girls of the same age range, the gains in school enrollment would be around 5 percentage points to 7 percentage points and in year of schooling would be around 0.06 to 0.3 year, and reductions in work would be around 5.6 percentage points to 8.3 percentage points. These effects abruptly lose statistical significance starting from ages 17-18. We do not find the same pattern with Kenya data, as demonstrated in Figure 7. In the Kenya case, for work as the outcome variable, β_1 is significantly positive for ages up to 15-16, as in the case of Uganda; however there is not a significant break afterwards, and the coefficient continues to be positive for the older ages, and even becomes larger than for the younger ages, in the case of boys.

Another way to interpret the estimated coefficients is to consider them in terms of impact on kids who were out of school. In the first column of table 3, referring to 7-8-year-old boys and girls, the coefficients would suggest an increase of about 31 percentage points (.63 + (-.64 * .5)) in likelihood that a child in an LPE area is in school from pre- to post-period, and the corresponding increase in the HPE area would be 12 percentage points (.63 + (-.64 * .8)). In both cases, about 3 out of 5 children who were not enrolled in base year were enrolled the next time around, suggesting that for ages 7-8, there is minimal gains in LPE areas. However, the above calculations suggest that for older ages, the difference in absolute value between β_1 and the coefficient of $YR02$ becomes larger, indicating larger differential gains for children in LPE areas.

I further complement the above results with tests of the significance of the difference in UPE impact between age groups, to see if the change in the coefficient of $(YR02 * ENR91_c)$ around ages 15-16/17-18 is indeed statistically significant. To perform these tests, I pull all age groups together into one sample, and estimate the following modified version of equation (2):

$$(3)\ S_{cdyka} = \delta_0 + \gamma_a + YR02 + \theta_c + YR02 * \lambda_d + YR02 * \gamma_a + \theta_c * \gamma_a + \delta_1(YR02 * ENR91_c) + \delta_2(YR02 * ENR91_c * \gamma_a) + \varepsilon_{cdyka}$$

where a is the age group of the individual, and γ_a are age group dummies. γ_a and $\theta_c * \gamma_a$ purge out age-group-specific fixed differences overall and at the county level, whereas $YR02*\gamma_a$ takes into account the average change over time in the outcome of each age group. The coefficients that I focus on in the tests are δ_2, which shows how the differential impact of UPE in LPE counties versus HPE counties varies between age groups. Specifically, I test the sign and statistical significance of the difference in δ_2 between each age group and its subsequent age groups. The test results are reported in Table 6, with age group 7-8 as the baseline group. The tests for school enrollment and work show that the change in trend is indeed significant around ages 13-14 to 17-18 (after which ages the trend flattens out, as indicated by the zero's, which imply that the null hypothesis of no difference cannot be rejected) for all and for females, but not for males separately, thereby reinforcing the indication found earlier that the UPE impact is more significant for females. There also seems to be a significant change in the trend for years of schooling at ages 17-18, despite the fact that the graph in Figure 3 does not show a clear change.

As a further robustness check, I pull together ages 7 to 40 and estimate equation (3) with attainment of grade 1, 2 or 3 as the dependent variable. The idea is to see whether those who are old enough so that their school enrollment was not affected by UPE when they were young would reveal a pattern of more positive ($YR02 * ENR91_c$) when the outcome is attainment of grade 1, 2, or 3. That is, lack of impact on school enrollment when young is expected to be likely to translate into lack of impact on attainment of the lowest grades, because attainment of these grades depends relatively more on simply being enrolled than higher and more difficult grades that may require more efforts. The estimated key coefficients are graphed in Figure 8. The graphs show that the coefficients appear to shift up around age 20, i.e. age 15 in 1997 if observed in 2002, and stabilize at the higher level thereafter. This is consistent with children of ages 15 and younger in 1997 gaining more in school enrollment thanks to UPE in LPE counties than in HPE counties. This differential impact seems to disappear for older cohorts. I also test the significance of this change in the trend of the key estimated coefficients (as I did in Table 6) by testing the sign and statistical significance of the difference in δ_2 between each age and its subsequent ages, at 5-age intervals. The test results are reported in Table 7, confirming that the patterns flatten out around ages 20-25, consistent with ages 15-20 and over in 1997 not benefiting from UPE in terms of school enrollment. For example, in the female grade-1 attainment column, the difference between δ_2 for age 30 (25 years old in 1997 if 30 years old in 2002) and δ_2 for age 20 (15 years old in 1997 if 20 years old in 2002) is statistically significant. However, the difference between δ_2 for age 30 and δ_2 for age 25 (20 years old in 1997 if 25 years old in 2002) is not significant, and neither is it for between older ages. This is consistent with there being a UPE impact on 15-year-olds in 1997 but not 20-year-olds and older people in 1997.

3.6 Conclusion

The empirical evidence found in this paper suggests that the UPE program in Uganda caused a decrease in labor supply of primary-school age children that accompanies an increase in their school enrollment. To the best of my knowledge, this is the first paper to consider outcomes other than school enrollment and attainment of a UPE program. The results also suggest that UPE has helped improved equality in school enrollment across areas even within a district.

UPE programs are alternatives to some other recently popular programs aimed at improving education in developing countries such as cash transfer programs. While evaluations of cash transfer programs have looked at many effects on the household, household-level impacts of UPE programs have not really been studied. This paper has abstracted from within-household effects of UPE, as well as UPE's possible impact on the quality of schooling, such as by raising the student/teacher ratio, due to data limitations. More well-rounded knowledge of such impacts would help policymakers be more informed in designing and implementing optimal society-wide policies. It would also be interesting to study how UPE programs affect aggregate outcomes such as labor productivity, wages, immigration, and trade.

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Figure 1. School Enrollment by Age in 1991 and 2002

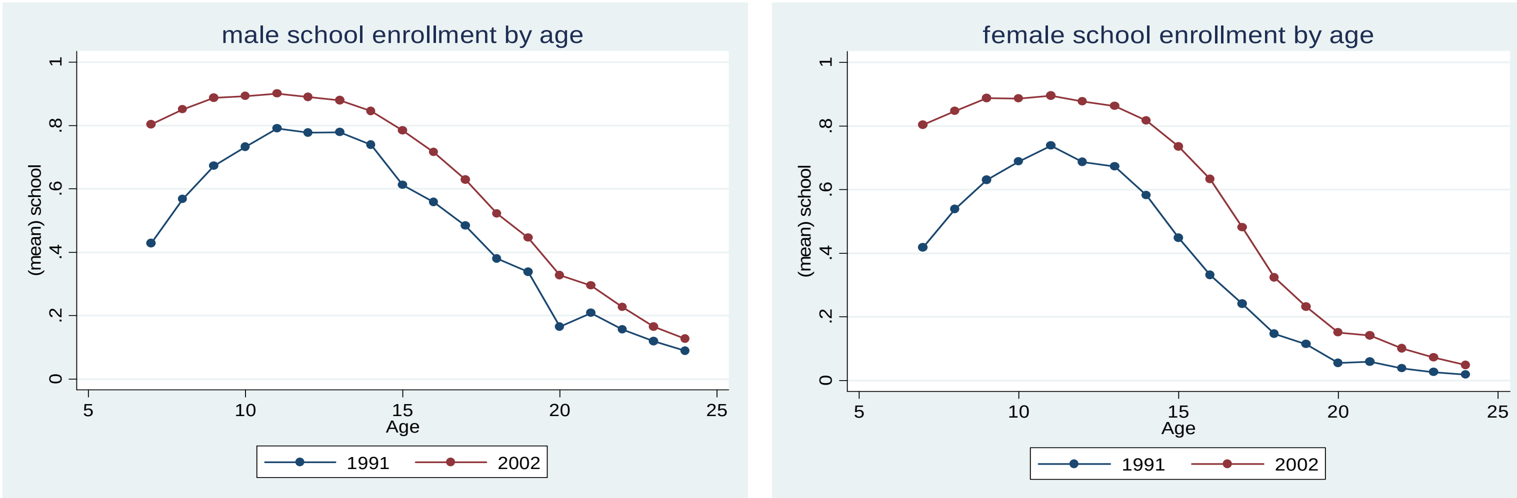


Figure 1 illustrates Uganda's nation-wide school enrollment rates of males and females of ages 7 to 24 in 1991 and 2002.

Figure 2. Distribution of County-level Outcomes of Counties with Higher vs. Counties with Lower Average Enrolment Rates in 1991

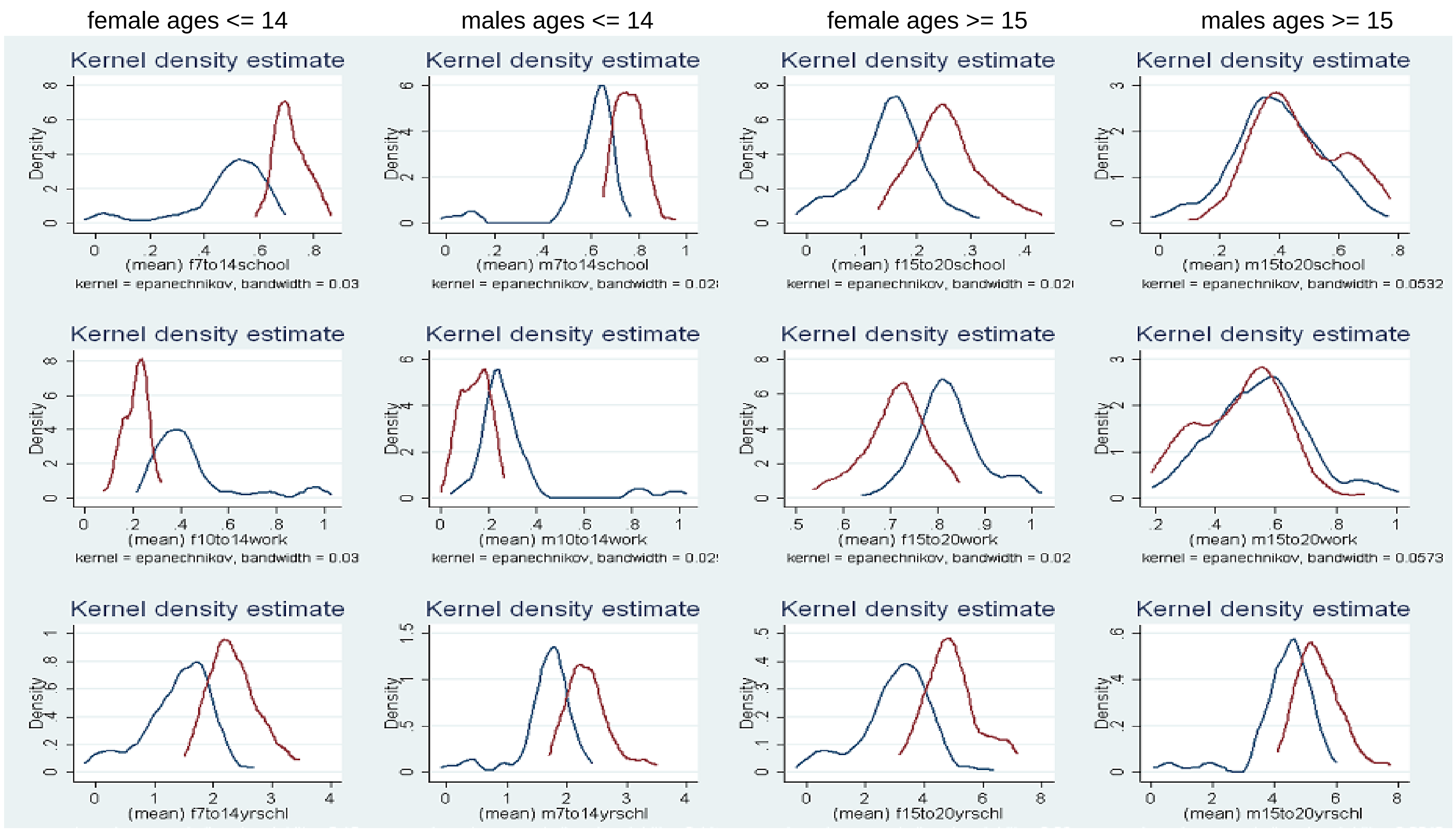


Figure 2 illustrates the 1991 distribution of county-level mean outcomes. The red line depicts the distribution among counties that had county-level enrolment rates below the 1991 median county-level 7-14-year-olds' enrolment rate of 0.66, and the blue line depicts the distribution among counties that had county-level enrolment rates equal to and above 0.66. The first row of graphs is for school enrolment as the outcome variable, the second row for work, and third row for years of schooling.

Figure 3. Distribution of County-level Outcomes of Counties with Higher vs. Counties with Lower Average Enrolment Rates in 2002

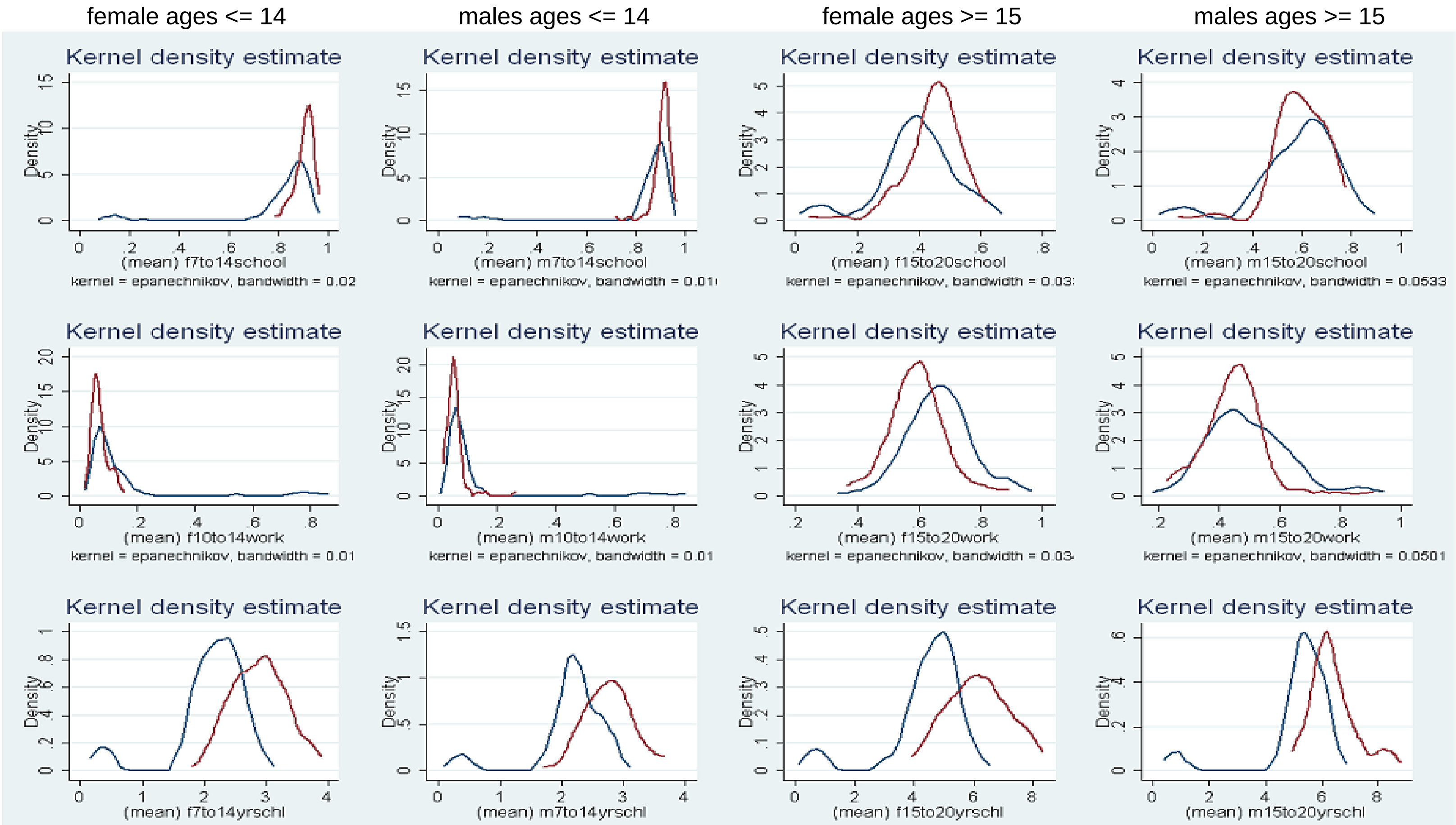
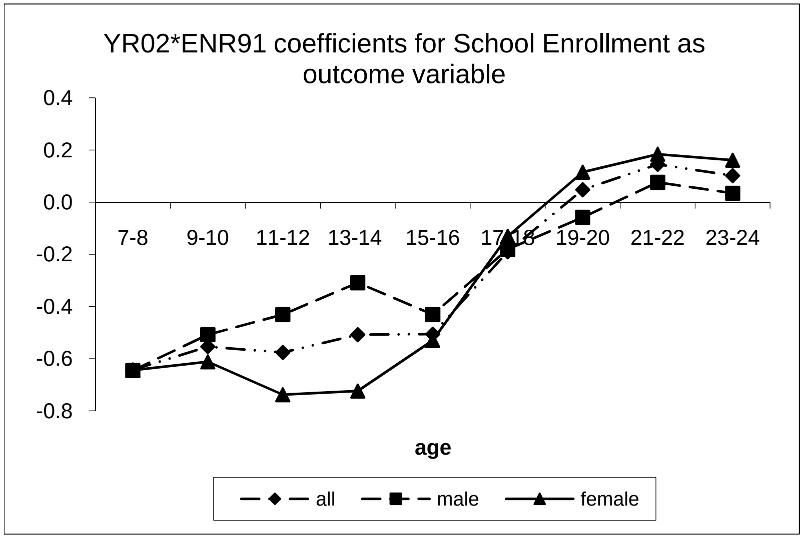
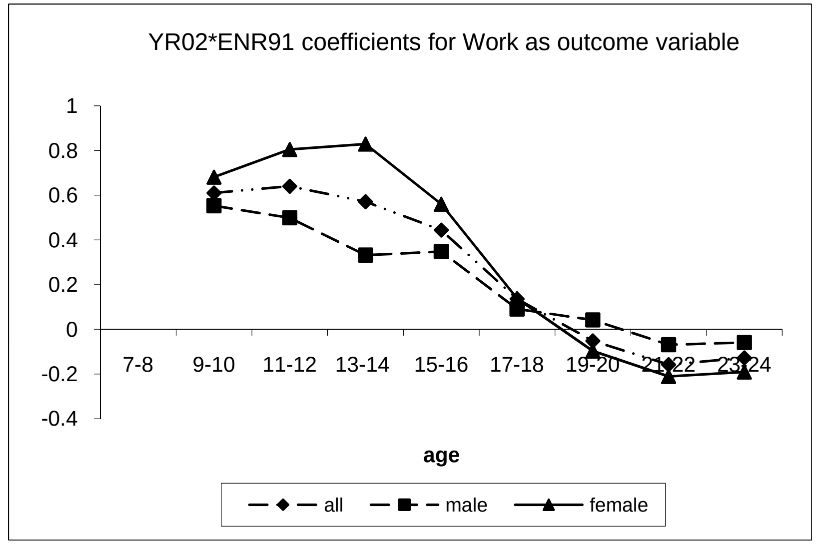


Figure 3 illustrates the 2002 distribution of county-level mean outcomes. The red line depicts the distribution among counties that had county-level enrolment rates below the 1991 median county-level 7-14-year-olds' enrolment rate of 0.66, and the blue line depicts the distribution among counties that had county-level enrolment rates equal to and above 0.66. The first row is for school enrolment as the outcome variable, the second row for work, and third row for years of schooling.

Figure 4. Change in Outcomes in Uganda





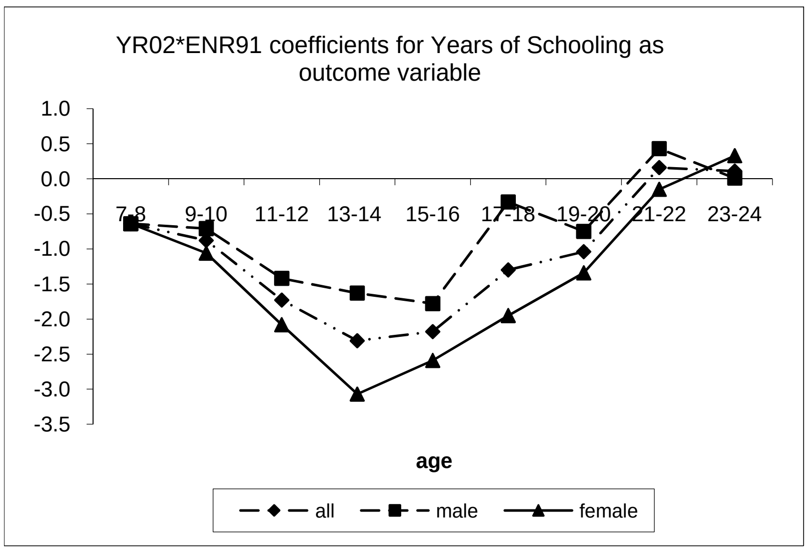
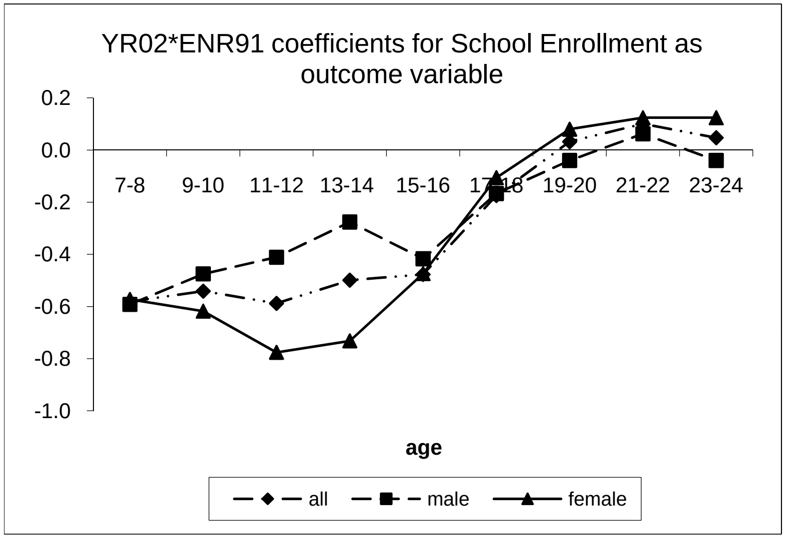
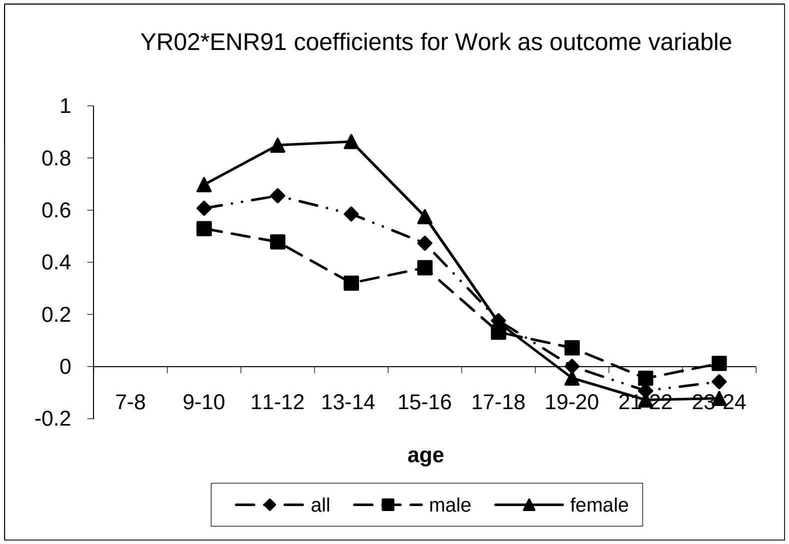


Figure 4 graphs the coefficients of (year 2002 * 1991 county-level enrolment rate of ages 7-14) from regressions that are estimated for pairs of ages from age 7 to age 24. Besides the 2002 year dummy and the interaction between the 2002 year dummy and the 1991 county enrollment rate, all regressions also include county fixed effects and (district * 2002) fixed effects. Individual sample weights are applied. The work variable is available only for ages 10 and higher.

Figure 5. Change in Outcomes in Uganda, Controlling for Income Proxy





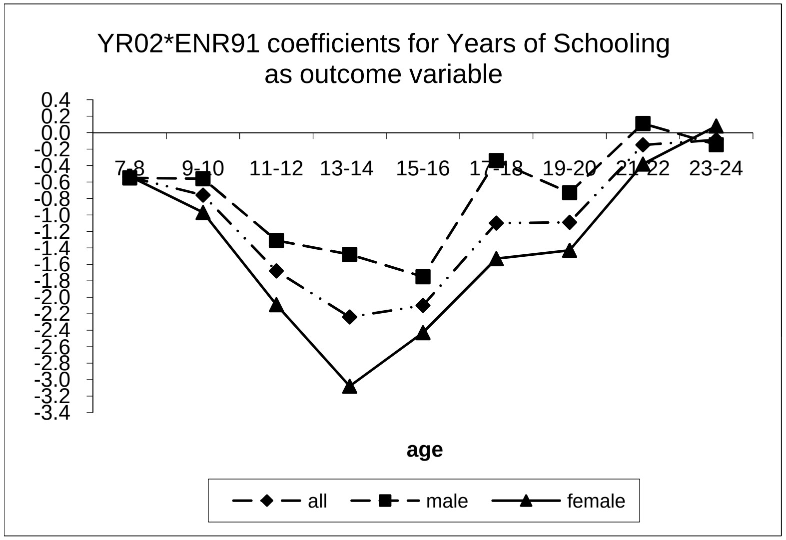
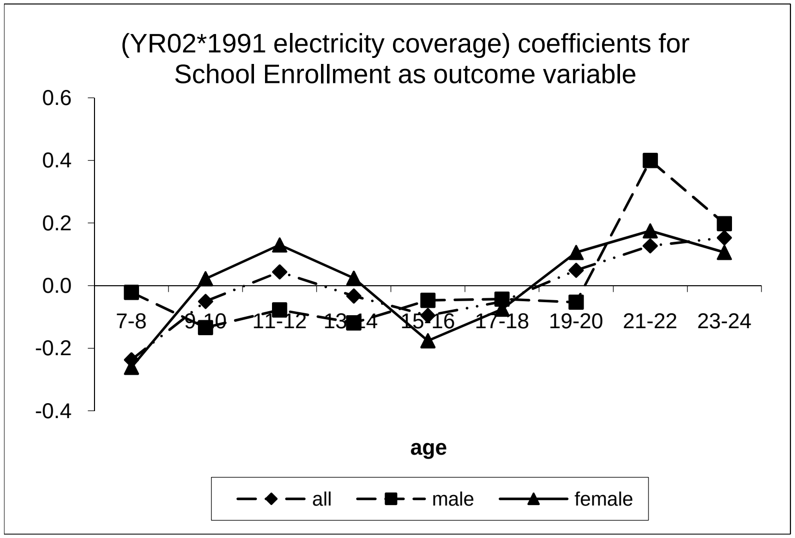
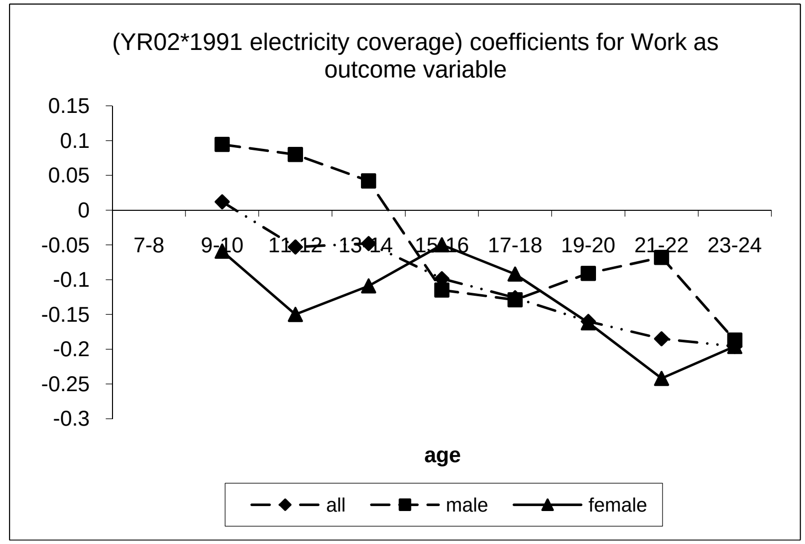


Figure 5 graphs the coefficients of (year 2002 * 1991 county-level enrolment rate of ages 7-14) from regressions that are estimated for pairs of ages from age 7 to age 24. Besides the 2002 year dummy and the interaction between the 2002 year dummy and the 1991 county enrollment rate, all regressions also include county fixed effects, (district * 2002) fixed effects, and the interaction between 2002 and the 1991 county-level share of electricity coverage, used as a proxy for county-level income. Individual sample weights are applied. The work variable is available only for ages 10 and higher.

Figure 6. Change in Outcomes in Uganda, Controlling for Income Proxy





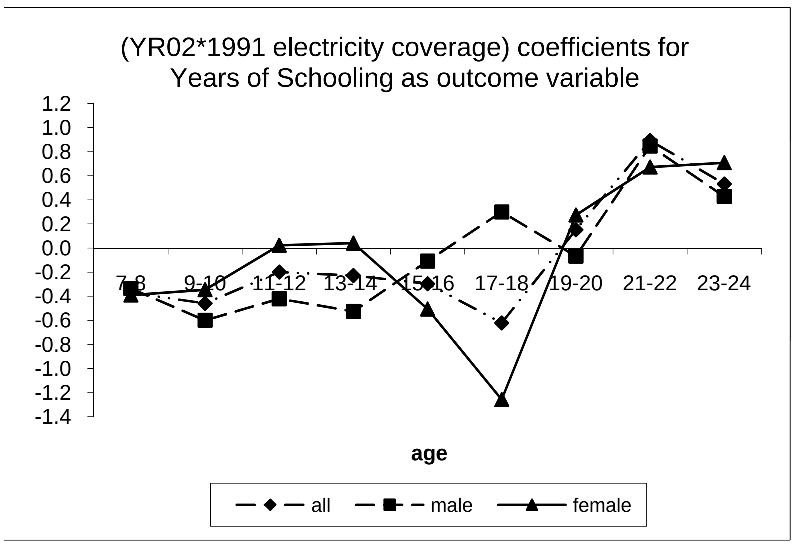
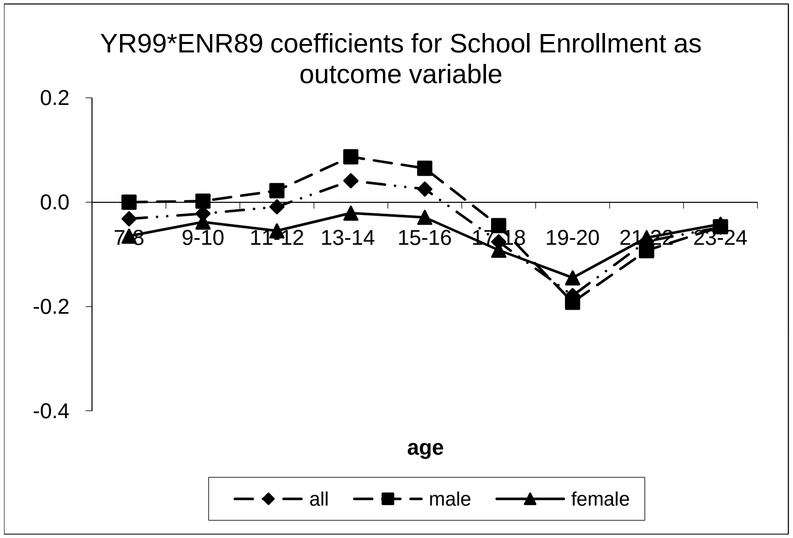
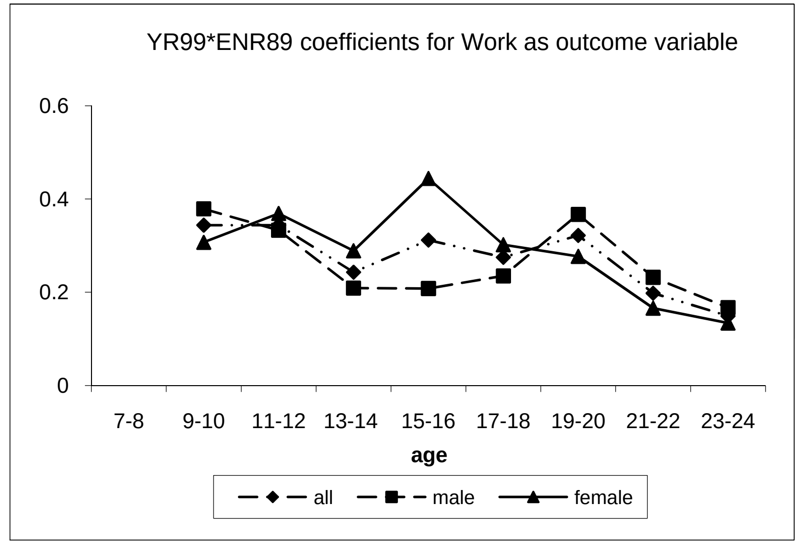


Figure 6 graphs the coefficients of (year 2002 * 1991 county-level electricity coverage) from regressions that are estimated for pairs of ages from age 7 to age 24. Besides the 2002 year dummy and the interaction between the 2002 year dummy and the 1991 county electricity coverage, all regressions also include county fixed effects, (district * 2002) fixed effects, and the interaction between 2002 and the 1991 county-level enrolment rate of ages 7-14. Individual sample weights are applied. The work variable is available only for ages 10 and higher.

Figure 7. Change in Outcomes in Kenya





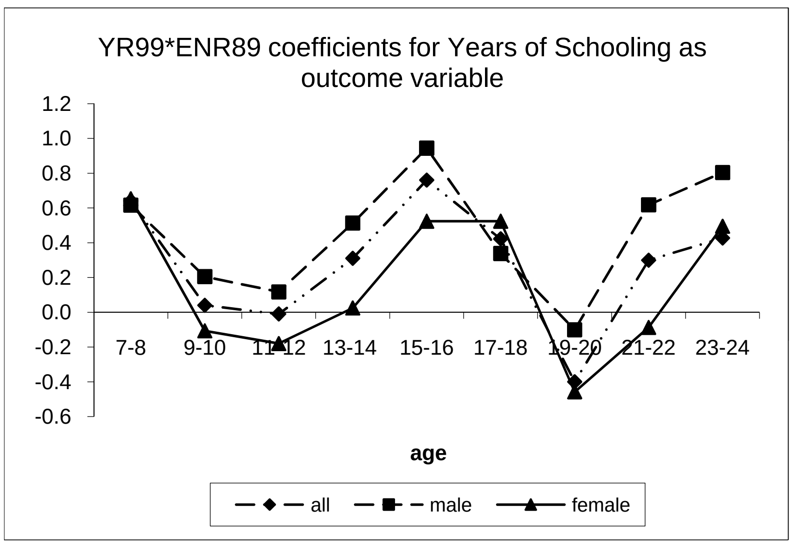
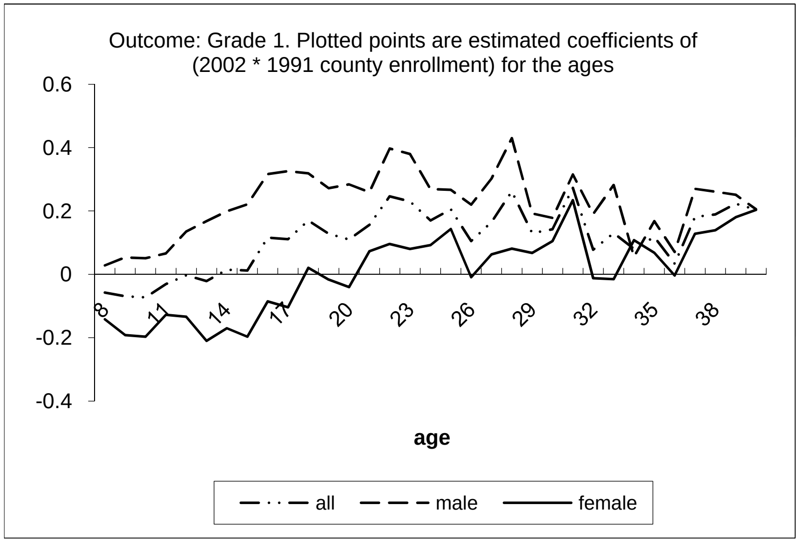
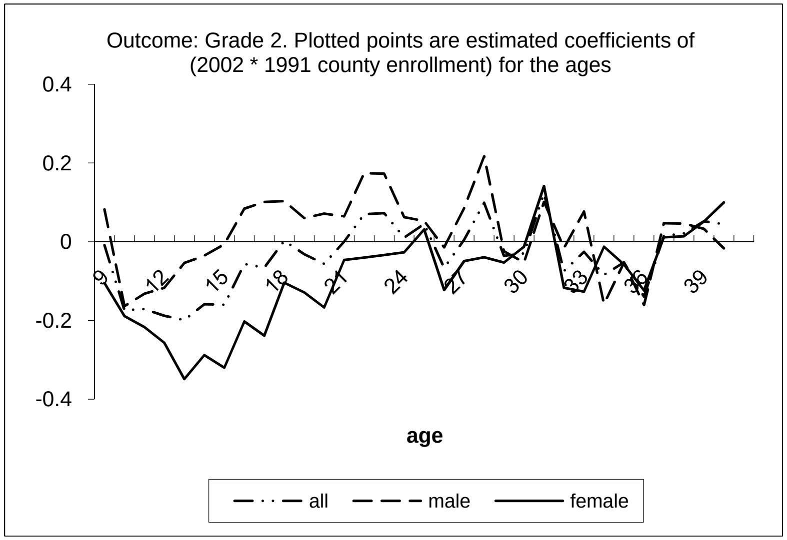


Figure 7 graphs the coefficients of (year 1999 * 1989 district-level enrolment rate of ages 7-14) from regressions that are estimated for pairs of ages from age 7 to age 24. Besides the 1999 year dummy and the interaction between the 1999 year dummy and the 1989 district enrollment rate, all regressions also include district fixed effects and (province * 1999) fixed effects. Individual sample weights are applied. The work variable is available only for ages 10 and higher.

Figure 8. Change in Grade Attainment Across Cohorts in Uganda





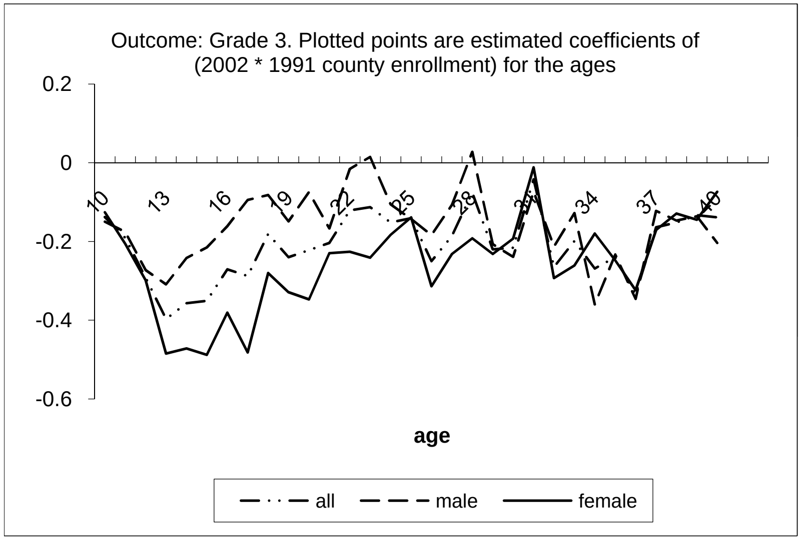


Figure 8 graphs the estimated coefficient of (2002 * 1991 county enrollment of ages 7-14) for each age from the earliest official age expected to be enrolled in the grade being considered up to age 40. The outcomes are attainment of grade 1, 2 or 3, and are binary. Besides the 2002 year dummy and the interaction between the 2002 year dummy and the 1991 county enrollment rate, all regressions also include county fixed effects and (district * 2002) fixed effects. Individual sample weights are applied.

Table 1. Summary Statistics - Uganda

		1991 county age 7-14 enrolment < 0.66			1991 county age 7-14 enrolment >= 0.66		
		(1)	(2)	(3)	(4)	(5)	(6)
		1991	2002	2002-1991	1991	2002	2002-1991
Females	age 7-14 school enrolment	0.5	0.81	0.31***	0.72	0.91	0.19***
		<0.50>	<0.39>	<0.00>	<0.45>	<0.29>	<0.00>
	age 10-14 work probability	0.41	0.13	-0.28***	0.21	0.07	-0.14***
		<0.49>	<0.34>	<0.00>	<0.41>	<0.26>	<0.00>
	age 7-14 years of schooling	1.51	2.16	0.65***	2.43	3.00	0.57***
		<1.79>	<1.84>	<0.01>	<2.00>	<2.05>	<0.01>
	age 15-20 school enrolment	0.16	0.39	0.23***	0.25	0.45	0.20***
		<0.37>	<0.49>	<0.00>	<0.44>	<0.50>	<0.00>
	age 15-20 work probability	0.82	0.66	-0.16***	0.71	0.58	-0.13***
		<0.39>	<0.47>	<0.00>	<0.45>	<0.49>	<0.00>
	age 15-20 years of schooling	3.32	4.46	1.14***	5.24	6.57	1.33***
		<3.18>	<3.20>	<0.02>	<3.18>	<3.25>	<0.02>
Males	age 7-14 school enrolment	0.61	0.83	0.22***	0.75	0.91	0.16***
		<0.49>	<0.38>	<0.00>	<0.43>	<0.29>	<0.00>
	age 10-14 work probability	0.27	0.11	-0.16***	0.16	0.06	-0.10***
		<0.44>	<0.31>	<0.00>	<0.36>	<0.24>	<0.00>
	age 7-14 years of schooling	1.74	2.16	0.42***	2.36	2.81	0.46***
		<1.79>	<1.85>	<0.01>	<1.88>	<1.97>	<0.01>
	age 15-20 school enrolment	0.4	0.57	0.18***	0.44	0.59	0.15***
		<0.49>	<0.49>	<0.00>	<0.50>	<0.49>	<0.00>
	age 15-20 work probability	0.54	0.50	-0.04***	0.50	0.45	-0.05***
		<0.50>	<0.50>	<0.00>	<0.50>	<0.50>	<0.00>
	age 15-20 years of schooling	4.54	5.15	0.61***	5.53	6.61	1.08***
		<2.95>	<3.05>	<0.02>	<2.91>	<3.00>	<0.02>

Table 1 provides the sample-weighted means of the listed outcome variables, as well as the difference in the means between 2002 and 1991.
0.66 is the median county-level enrolment in 1991. There are 161 counties in total.
Robust standard deviations in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table 2. Summary Statistics - Kenya

		1989 county age 7-14 enrolment < 0.87			1989 county age 7-14 enrolment >= 0.87		
		(1)	(2)	(3)	(4)	(5)	(6)
		1989	1999	1999-1989	1989	1999	1999-1989
Females	age 7-14 school enrolment	0.66	0.49	-0.17***	0.91	0.88	-0.03***
		<0.47>	<0.50>	<0.00>	<0.29>	<0.33>	<0.00>
	age 10-14 work probability	0.62	0.62	-0.01	0.31	0.41	0.10***
		<0.48>	<0.49>	<0.01>	<0.46>	<0.49>	<0.00>
	age 7-14 years of schooling	1.75	1.47	-0.28***	2.58	2.93	0.35***
		<2.02>	<2.05>	<0.02>	<2.14>	<2.12>	<0.01>
	age 15-20 school enrolment	0.39	0.22	-0.17***	0.53	0.41	-0.11***
		<0.49>	<0.41>	<0.00>	<0.50>	<0.49>	<0.00>
	age 15-20 work probability	0.77	0.6	-0.17***	0.55	0.52	-0.03***
		<0.42>	<0.49>	<0.01>	<0.50>	<0.50>	<0.00>
	age 15-20 years of schooling	4.43	3.68	-0.76***	6.72	7.18	0.45***
		<3.68>	<3.87>	<0.04>	<2.82>	<2.83>	<0.02>
Males	age 7-14 school enrolment	0.7	0.53	-0.17***	0.91	0.89	-0.02***
		<0.46>	<0.50>	<0.00>	<0.29>	<0.32>	<0.00>
	age 10-14 work probability	0.59	0.57	-0.02***	0.29	0.41	0.12***
		<0.49>	<0.49>	<0.01>	<0.45>	<0.49>	<0.00>
	age 7-14 years of schooling	1.78	1.52	-0.25***	2.42	2.74	0.31***
		<2.00>	<2.04>	<0.02>	<2.08>	<2.08>	<0.01>
	age 15-20 school enrolment	0.54	0.32	-0.22***	0.68	0.49	-0.20***
		<0.50>	<0.47>	<0.01>	<0.47>	<0.50>	<0.00>
	age 15-20 work probability	0.67	0.63	-0.04***	0.43	0.52	0.10***
		<0.47>	<0.48>	<0.01>	<0.49>	<0.50>	<0.00>
	age 15-20 years of schooling	5.17	4.03	-1.15***	6.82	6.94	0.13***
		<3.50>	<3.82>	<0.04>	<2.79>	<2.91>	<0.02>

Table 2 provides the sample-weighted means of the listed outcome variables, as well as the difference in the means between 1999 and 1989.
0.87 is the median district-level enrolment in 1989. There are 41 districts in total.
Robust standard deviations in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table 3. Change in School Enrollment in Uganda

LHS variable: School enrollment (binary variable)	all	male	female	all	male	female	all	male	female
		ages 7-8			ages 9-10			ages 11-12	
2002	0.63***	0.64***	0.61***	0.50***	0.51***	0.50***	0.47***	0.32***	0.62***
	(0.10)	(0.10)	(0.11)	(0.09)	(0.08)	(0.14)	(0.09)	(0.10)	(0.10)
2002 * 1991 county enrollment	-0.64***	-0.65***	-0.64***	-0.55***	-0.51***	-0.61***	-0.58***	-0.43***	-0.74***
	(0.13)	(0.13)	(0.13)	(0.11)	(0.09)	(0.15)	(0.09)	(0.07)	(0.13)
N	247,220	122,634	124,586	126,646	63,673	62,973	219,330	110,048	109,282
		ages13-14			ages 15-16			ages 17-18	
2002	0.43***	0.28***	0.63***	0.44***	0.42***	0.42***	0.14	0.15*	0.08
	(0.08)	(0.06)	(0.11)	(0.09)	(0.07)	(0.13)	(0.09)	(0.08)	(0.12)
2002 * 1991 county enrollment	-0.51***	-0.31***	-0.72***	-0.51***	-0.43***	-0.53***	-0.19*	-0.18*	-0.13
	(0.09)	(0.06)	(0.14)	(0.12)	(0.08)	(0.15)	(0.11)	(0.10)	(0.12)
N	205,089	102,970	102,119	181,460	90,774	90,686	175,504	83,909	91,595
		ages 19-20			ages 21-22			ages 23-24	
2002	-0.06	0.01	-0.10*	-0.12**	-0.06	-0.14***	-0.07	-0.01	-0.13***
	(0.06)	(0.08)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.07)	(0.03)
2002 * 1991 county enrollment	0.05	-0.06	0.12	0.15**	0.08	0.18***	0.10*	0.03	0.16***
	(0.08)	(0.11)	(0.07)	(0.06)	(0.08)	(0.06)	(0.06)	(0.09)	(0.04)
N	173,582	76,377	97,205	134,961	63,020	71,941	126,294	60,555	65,739

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3 reports results from regressions that are estimated for each pair of ages from age 7 to age 24, with school enrollment as the outcome variable. Besides the 2002 year dummy and the interaction between the 2002 year dummy and the 1991 county enrollment rate of ages 7-14, all regressions also include county fixed effects and (district * 2002) fixed effects. The key coefficient is the coefficient of (2002 * 1991 county enrollment). All standard errors are clustered at the county level. Sample weights are applied. Since the work variable is available only for ages 10 and higher, I include only age 10 in the pair of ages 9-10 so that the school and work results are comparable.

Table 4. Change in Work in Uganda

LHS variable: Work (binary variable)	all	male	female	all	male	female	all	male	female
		ages 7-8			ages 9-10			ages 11-12	
2002				-0.55***	-0.57***	-0.54***	-0.55***	-0.40***	-0.73***
				(0.08)	(0.06)	(0.12)	(0.09)	(0.09)	(0.10)
2002 * 1991 county enrollment				0.61***	0.55***	0.68***	0.64***	0.50***	0.81***
				(0.10)	(0.08)	(0.14)	(0.08)	(0.05)	(0.13)
N				122,457	61,498	60,959	213,611	107,150	106,461
		ages13-14			ages 15-16			ages 17-18	
2002	-0.52***	-0.31***	-0.78***	-0.37***	-0.29***	-0.47***	-0.15	-0.15	-0.12
	(0.07)	(0.04)	(0.12)	(0.12)	(0.08)	(0.18)	(0.11)	(0.11)	(0.12)
2002 * 1991 county enrollment	0.57***	0.33***	0.83***	0.44***	0.35***	0.56***	0.14	0.09	0.14
	(0.09)	(0.05)	(0.13)	(0.12)	(0.10)	(0.16)	(0.13)	(0.14)	(0.14)
N	198,897	99,814	99,083	178,949	90,311	88,638	168,111	80,109	88,002
		ages 19-20			ages 21-22			ages 23-24	
2002	0.01	-0.01	-0.03	0.08	0.03	0.08	0.02	-0.04	0.07
	(0.08)	(0.10)	(0.07)	(0.06)	(0.08)	(0.05)	(0.05)	(0.08)	(0.04)
2002 * 1991 county enrollment	-0.05	0.04	-0.10	-0.16**	-0.07	-0.21***	-0.13*	-0.06	-0.19***
	(0.09)	(0.13)	(0.07)	(0.07)	(0.11)	(0.07)	(0.07)	(0.10)	(0.05)
N	166,952	73,343	93,609	128,200	59,081	69,119	120,093	56,835	63,258

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4 reports results from regressions that are estimated for each pair of ages from age 7 to age 24, with work probability as the outcome variable. Besides the 2002 year dummy and the interaction between the 2002 year dummy and the 1991 county enrollment rate of ages 7-14, all regressions also include county fixed effects and (district * 2002) fixed effects. The key coefficient is the coefficient of (2002 * 1991 county enrollment). All standard errors are clustered at the county level. Individual sample weights are applied. The work variable is available only for ages 10 and higher.

Table 5. Change in Years of Schooling in Uganda

LHS variable: Years of Schooling (continuous variable)	all	male	female	all	male	female	all	male	female
		ages 7-8			ages 9-10			ages 11-12	
2002	0.67***	0.76***	0.56***	0.99***	1.12***	0.87***	1.91***	1.68***	2.16***
	(0.15)	(0.15)	(0.16)	(0.21)	(0.19)	(0.29)	(0.27)	(0.29)	(0.31)
2002 * 1991 county enrollment	-0.64***	-0.64***	-0.64***	-0.88***	-0.71***	-1.06***	-1.73***	-1.42***	-2.08***
	(0.17)	(0.17)	(0.18)	(0.25)	(0.24)	(0.28)	(0.35)	(0.36)	(0.40)
N	245,180	121,682	123,498	232,221	116,582	115,639	218,644	109,708	108,936
		ages13-14			ages 15-16			ages 17-18	
2002	2.22***	1.84***	2.64***	2.64***	2.19***	3.24***	1.70***	1.21**	1.72**
	(0.55)	(0.46)	(0.73)	(0.46)	(0.44)	(0.61)	(0.59)	(0.49)	(0.75)
2002 * 1991 county enrollment	-2.31***	-1.63***	-3.07***	-2.18***	-1.78***	-2.59***	-1.30*	-0.33	-1.95**
	(0.57)	(0.52)	(0.66)	(0.60)	(0.52)	(0.72)	(0.72)	(0.61)	(0.87)
N	204,368	102,641	101,727	185,560	93,914	91,646	175,572	84,245	91,327
		ages 19-20			ages 21-22			ages 23-24	
2002	1.00*	1.06*	0.77	0.53	0.47	0.53	0.35	0.40	0.31
	(0.56)	(0.56)	(0.70)	(0.69)	(0.77)	(0.72)	(0.55)	(0.77)	(0.76)
2002 * 1991 county enrollment	-1.04	-0.75	-1.34*	0.16	0.43	-0.15	0.11	0.01	0.33
	(0.71)	(0.71)	(0.79)	(0.83)	(0.94)	(0.86)	(0.71)	(0.96)	(0.67)
N	174,039	77,174	96,865	132,075	60,884	71,191	124,078	58,958	65,120

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5 reports results from regressions that are estimated for each pair of ages from age 7 to age 24, with years of schooling as the outcome variable. Besides the 2002 year dummy and the interaction between the 2002 year dummy and the 1991 county enrollment rate, all regressions also include county fixed effects and (district * 2002) fixed effects. The key coefficient is the coefficient of (2002 * 1991 county enrollment). All standard errors are clustered at the county level. Individual sample weights are applied. Since the work variable is available only for ages 10 and higher, I include only age 10 in the pair of ages 9-10 so that the results here and work results are comparable.

Table 6. Test of significance of difference in (2002*1991 county enrolment) coefficient between ages in Uganda

	School Enrolment (binary)			Work (binary)			Years of Schooling		
	all	male	female	all	male	female	all	male	female
ages 23-24 vs. ages 21-22	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
ages 23-24 vs. ages 19-20	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
ages 21-22 vs. ages 19-20	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
ages 21-22 vs. ages 17-18	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
ages 19-20 vs. ages 17-18	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
ages 19-20 vs. ages 15-16	0.00	0.00	0.00	-0.18**	0.00	0.00	0.00	0.00	0.00
ages 17-18 vs. ages 15-16	0.00	0.00	0.00	-0.14**	0.00	0.00	0.00	0.00	0.00
ages 17-18 vs. ages 13-14	0.13***	0.00	0.25**	-0.24***	0.00	-0.34**	1.01***	1.10**	0.92**
ages 15-16 vs. ages 13-14	0.00	0.00	0.11**	0.00	0.00	-0.16**	0.51**	0.00	0.00
ages 15-16 vs. ages 11-12	0.11***	0.00	0.00	-0.14**	0.00	0.00	0.00	0.00	0.00
ages 13-14 vs. ages 11-12	0.06**	0.10**	0.00	0.00	0.00	0.00	0.00	0.00	-0.69**
ages 13-14 vs. ages 9-10	0.15***	0.18**	0.00	0.28***	0.00	0.36***	0.00	0.00	0.00
ages 11-12 vs. ages 9-10	0.09**	0.00	0.00	0.32***	0.25**	0.35***	0.00	0.00	0.00
ages 11-12 vs. ages 7-8	0.00	0.00	0.00	0.92***	0.80***	1.05***	0.00	0.00	0.00
ages 9-10 vs. ages 7-8	-0.10***	0.00	0.00	0.60***	0.55***	0.69***	0.00	0.00	0.00

Table 6 reports test results of significance of differences between coefficients of (2002*1991 county enrolment*agegroup) in regresssions that include a 2002 year dummy, (county*agegroup) fixed effects with agegroup being dummies for 9 age groups from age 7 to age 24, (agegroup*2002) fixed effects, (district * 2002) fixed effects, the interaction between the 2002 year dummy and the 1991 county enrollment rate, and (2002*1991 county enrolment*agegroup). For example, in the first column, "ages 23-24 vs. ages 21-22" refers to the test of the significance of the difference between the coefficient of (2002*1991 county enrolment*dummy for ages 23-24) and the coefficient of (2002*1991 county enrolment*dummy for ages 21-22). The value 0 in the test results implies that the null hypothesis of the difference between the coefficients cannot be rejected. All standard errors are clustered at the county level. Individual sample weights are applied. Since the work variable is available only for ages 10 and higher, I include only age 10 in the pair of ages 9-10 so that the results are comparable throughout. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Test of significance of difference in (2002*1991 county enrolment) coefficient between ages in Uganda - grade attainm

	Grade 1 attainment (binary)			Grade 2 attainment (binary)			Grade 3 attainment (binary)		
	all	male	female	all	male	female	all	male	female
age 40 vs. age 35	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
age 40 vs. age 30	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
age 35 vs. age 30	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
age 35 vs. age 25	-0.09**	0.00	0.00	-0.10**	0.00	0.00	-0.10***	0.00	0.00
age 30 vs. age 25	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
age 30 vs. age 20	0.00	0.00	0.15**	0.00	0.00	0.15**	0.00	0.00	0.15**
age 25 vs. age 20	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
age 25 vs. age 15	0.19***	0.00	0.34**	0.21***	0.00	0.35**	0.21**	0.00	0.00
age 20 vs. age 15	0.10**	0.00	0.16**	0.10**	0.00	0.00	0.13**	0.00	0.00
age 20 vs. age 10	0.18***	0.23***	0.16**	0.00	0.00	0.00	0.00	0.00	-0.22***
age 15 vs. age 10	0.08**	0.17***	0.00	0.00	0.00	0.00	-0.21***	0.00	-0.36***
age 15 vs. baseline age	-0.12**	0.00	-0.29***	-0.19**	0.00	-0.35**	-0.25**	0.00	-0.39**
age 10 vs. baseline age	-0.21***	-0.13***	-0.29***	-0.20**	0.00	0.00	0.00	0.00	0.00

Table 7 reports test results of significance of differences between coefficients of (2002*1991 county enrolment*age dummy) in regresssions that include a 2002 year dummy, (county*age) fixed effects with the variable age being dummies for ages from age 7 to age 40, (age*2002) fixed effects, (district * 2002) fixed effects, the interaction between the 2002 year dummy and the 1991 county enrollment rate, and (2002*1991 county enrolment*age). For example, in the first column, "age 40 vs. age 35" refers to the test of the significance of the difference between the coefficient of (2002*1991 county enrolment*dummy for age 40) and the coefficient of (2002*1991 county enrolment*dummy for age 35). The value 0 in the test results implies that the null hypothesis of the difference between the coefficients cannot be rejected. All standard errors are clustered at the county level. Individual sample weights are applied. *** p<0.01, ** p<0.05, * p<0.1

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