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ABSTRACT

The Long-Run Economic Consequences of Iodine Supplementation*

We present evidence on the impacts of a large-scale iodine supplementation program in Tanzania on individuals' long-term economic outcomes. Exploiting the timing and location of the intervention, we document that in utero exposure to the program increased completed years of education and income scores in adulthood. We find no increase in total employment, but a significant change in the occupational structure. Cohorts exposed to the program are less likely to work in agricultural self-employment and more likely to hold skilled jobs that typically demand higher levels of education. Together, these results demonstrate that iodine deficiency can have long-run implications for occupational choices and labor market incomes in low-income regions.

JEL Classification: I15, I18, J24, N35

Keywords: iodine supplementation, long-run, educational attainment, labor market outcomes

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1 Introduction

Preventing and eradicating nutritional deficiencies are basic concerns among policymakers in the developing world. Leading health authorities have recommended interventions targeting early life nutrition, which have been shown to be highly cost-effective and with potential implications for well-being through the life cycle.[1] Although the prevalence of nutritional deficits has globally decreased in the last decades, deficiencies in micronutrients such as Vitamin A, iron, and iodine are still far too common. Iodine deficiency alone is estimated to affect more than 240 million children worldwide (Andersson et al., 2012). The deficiency of this micronutrient during pregnancy, in particular, can cause permanent brain damage and harm the mental development of the offspring (Pharoah and Connolly, 1987; Zimmermann et al., 2008). In fact, it is considered the leading cause of preventable mental retardation in the world and thus a major target of global health authorities. In this paper, we investigate some of the long-term consequences of a large-scale iodine supplementation program in Tanzania, a setting where almost one-half of the population suffered from iodine deficiency.

Existing research on the long-run impacts of iodine deficiency has focused nearly exclusively on the United States (Feyrer et al., 2017; Adhvaryu et al., 2018) and Switzerland (Politi, 2014). However, the highest prevalence of iodine deficiency is in Sub-Saharan Africa and Asia, where many children grow up in conditions of extreme poverty. Though the biological benefits of iodine and other nutritional interventions are well known (Isa et al., 2000; Cao et al., 1994; Pharoah and Connolly, 1987), it is unclear whether such interventions can improve individuals' long-term economic prospects in low-income settings. In the presence of credit constraints and the absence of a well-functioning public education system, even relatively large changes in cognitive capacity may have limited or only short-lived impacts on human capital. While some studies have examined the short-term impacts of iodine deficiency on student outcomes such as test scores (Field et al., 2009; Deng and Lindeboom, 2019), they do not speak directly to the question of whether such gains are sustained or whether they will ultimately have long-term impacts on the subsequent success of children in the labor market later in life. A good examination of this question is critical not only for prevention and cost-effective policy design but for our understanding of whether individuals in the world's poorest regions are caught in nutrition-based poverty traps (Strauss and Thomas, 1998).

[1]The potential consequences of nutritional deficits early in life have been recognized by policymakers. With respect to iodine deficiency, the focus of this paper, the World Bank (1996, p. 6) notes that "if children suffering from these ailments [iodine deficiency] enter school at all, they are likely to enter late, to have more difficulty learning, and to leave prematurely." More recently, the United Nations Children's Fund (2008, p., 5) goes further and states that iodine deficiency "affects a child's ability to learn, and later in life, to earn... therefore preventing children, communities and nations from fulfilling their potential."

This paper sheds new light on this question by examining whether and to what extent iodine supplementation early in life affects completed education, employment outcomes, and occupational attainment in one of the poorest regions in the world. In 1986, the government of Tanzania launched a large-scale iodine supplementation program to control the high prevalence of iodine deficiency in the country. The program distributed iodized oil capsules (IOC) in afflicted districts, giving high priority to women of childbearing age and young children.[2] Depending on the dosage, supplementation with IOC can offer protection against fetal iodine deficiency for approximately four years, so children conceived just before or around three years after the campaign are likely to have received some fetal protection. Distribution campaigns were scheduled to begin in 1986, with additional doses taking place in subsequent years. However, there were several delays in many districts due largely to administrative issues —i.e., the time required for the central government and local offices to coordinate a distribution system. Between 1986 and 1994, approximately 5 million mothers and children received supplementation with IOC, and follow-up surveys suggest that prevalence rates of goiter fell by more than half soon after program implementation (Peterson, 2000).

We exploit variation in *where* and *when* the campaigns took place, as well as the expected duration from the dosage, to form the basis of our research design. Depending on the date of the distribution campaign, some cohorts were exposed *in utero* to the program, whereas others were exposed only after birth. Individuals exposed in utero or even during the first years of life had more to gain from the program in comparison to those who were already "too old" at the time of the campaign. Our empirical strategy, therefore, compares cohorts based on their place of birth, and their year of birth relative to the IOC campaign to identify the long-run impacts of the program in a difference-in-differences framework. The variation in the timing of the campaigns and date of birth allows us to calculate the impacts of the program at different ages of exposure —i.e., from *in utero* to the first years of life.

We begin by estimating the impact of the program on completed education. We find that individuals who were exposed in early-life to IOC acquired an additional 0.17 years of education and are 2 percentage points more likely to complete secondary school, effects that represent respectively about 2.5 and 12 percent relative to the baseline. Examining the timing of these impacts, we find that the results are driven by exposure during the *in utero* period, with little to no differential benefits from exposure after birth. This result is consistent with the view that an inadequate supply of iodine during the *in utero* period can

[2]Supplementation was not encouraged in people over 45 years to reduce risks of iodine-induced hyperthyroidism in this population (Peterson, 2000).

have irreversible consequences independently of subsequent supplementation.[3]

We then examine the effects of the program on labor market outcomes. The estimates suggest that *in utero* exposure to IOC increases occupational skill scores, increases the probability of being employed in a position belonging to a skilled occupation, and reduces the likelihood of self-employment in agricultural activities. In contrast, we do not find statistically significant or meaningful impacts on the likelihood of working. This suggests that iodine supplementation affected individuals' labor market outcomes by shifting employment shares from agriculture towards jobs that typically demand more skilled labor and have higher economic returns. The estimates indicate that skilled employment rates increased by about 1.1 percentage points (or 5 percent relative to the sample mean).

A limitation of our labor market analysis is that we have no information on income in the main dataset, and other sources of data where this information is available have limited sample sizes or lack other important information to identify the extent to which each cohort was exposed to the program. To provide estimates of program impacts on potential income wages and place our results in perspective, we predict income for an individual in our main sample based on his/her occupation, gender and location of residence, as in Bleakley (2010). We find significant gains in this occupation-based income score. Exposed cohorts have occupational-income wages that are about 2.1 percent higher relative unexposed individuals. For comparison, one of the most successful interventions targeting explicitly education in the context of a developing country impacts wages by 1.5 to 2.7 percent (Duflo, 2001). Overall, these results suggest that the improvements in occupational attainment induced by the IOC program translate into statistically meaningful differences in income wages.

While our estimates suggest strong impacts on education and labor market outcomes, we find no evidence that the program had a meaningful effect on long-run physical disabilities. Although point estimates are in general negative, they are small and statistically indistinguishable from zero. These results are in agreement with the medical literature indicating that iodine deficiency does not have a first-order effect on physical health outcomes (Allen and Gillespie, 2001). We take this finding as suggestive evidence that the program affected education and labor market outcomes by affecting the middle of the "health" distribution rather than the extreme left tail of the physical/cognitive health distribution.

Our quasi-experimental results rest crucially on the assumption that the outcomes of individuals in districts with varying IOC status would have had similar trends in the absence of the program. We provide a variety of evidence supporting the plausibility of this identifying assumption. In particular, we show that the timing of the campaigns is uncorrelated with a number of baseline district characteristics. This is consistent with the

[3]See, for instance, Cao et al. (1994), Zimmermann et al. (2008) and Hetzel and Mano (1989).

narrative that the variation in IOC status across districts stemmed largely from logistical issues rather than from expectations about future local development prospects (Peterson, 2000). Another natural concern with our empirical strategy is that different quality parents may try to influence the timing of conception in response to IOC campaigns, potentially changing the composition of women giving birth. We show that program exposure does not predict a number of predetermined maternal characteristics or cohort sizes, which is in line with previous research documenting that parents were unlikely to selectively change their fertility behavior in response to IOC campaigns (Adhvaryu and Nyshadham, 2016). Finally, the lack of correlation between program exposure at older ages and subsequent adult outcomes suggests that the estimated effects are not driven by preexisting trends in the outcomes of interest. Collectively, these findings support the validity of our empirical strategy and reinforce our interpretation that iodine deficiency early in life has important implications for adult outcomes.

This study relates to a large literature that examines the link between initial conditions and the long-run evolution of human capital accumulation (for reviews, see Currie and Almond (2011), Almond and Currie (2011), and Almond et al. (2018)). Recent studies have focused on the long-term consequences of physical health conditions in early-life (Gensowski et al., 2019), antibiotic availability in infancy (Bhalotra and Venkataramani, 2015), cash or in-kind transfer programs (Aizer et al., 2016; Hoynes et al., 2016), and changes in key environmental conditions (Bhalotra et al., 2017; Karbownik and Wray, 2019). As mentioned above, evidence on the long-run consequences of iodine deficiency comes from developed economies, such as the United States and Switzerland. We add to the literature by providing new evidence on this question using data for Sub-Saharan Africa, a region characterized by the very high prevalence of iodine deficiency.[4] Hence, our findings are relevant for understanding the long-term impacts of early life iodine deficiency in areas where this is a major social problem. Compared to previous studies, we find effects that tend to be much larger in magnitude. For example, Feyrer et al. (2017) and Adhvaryu et al. (2018) find limited effects on long-run educational attainment in the United States.[5] This confirms the

[4]The prevalence of iodine deficiency was much higher in Tanzania than Switzerland and certainly the United States. For instance, the average prevalence of iodine deficiency, as proxied by goiter prevalence, was 40 percent in Tanzania before the supplementation program, whereas it was approximately 20 percent in Switzerland (Politi, 2014). On the other hand, while the data reported in Adhvaryu et al. (2018) drawn from Love (1920) are likely to underestimate the real prevalence of iodine deficiency in the United States, it is still likely to be lower than that in Tanzania. As noted by some scholars, iodine deficiency was considered serious public health in a few specific areas, particularly in Northern Michigan counties (Levin, 1919; Markel, 1987).

[5]Although not formally reported in the published work, Feyrer et al. (2017) mention on footnote 35 that they do not find any effect of iodization on education levels, either using data from the census or military records.

importance of studying the effects of such nutritional intervention in a setting with a high prevalence of iodine deficiency and low levels of human capital accumulation.

We are aware of other studies examining the consequences of nutritional interventions in low-income settings, including Hoddinott et al. (2008) and Maluccio et al. (2009) for four villages in Guatemala, Linnemayr and Alderman (2011) for three regions in rural Senegal, and Carneiro et al. (2020) for Northern Nigeria. These studies examine multifaceted interventions that directly affect childhood nutrition in general as well as other inputs recognized as important in the production function of infant health (such as health information or cash transfers).[6] Given the design of these interventions, it is not possible to identify the role of specific deficiencies in shaping children's future outcomes. Identifying the role of specific nutritional deficiencies is important for cost-effective policy design in areas where such deficiencies are endemic. Moreover, with exception of Hoddinott et al. (2008) and Maluccio et al. (2009), these studies tend to focus on children's short-run education outcomes such as test scores and do not examine the long-run repercussions on labor market outcomes. We focus on the role of a large-scale intervention targeting a specific nutritional deficiency, which covered nearly the universe of the afflicted population. Importantly, we document longer-run implications of this type of intervention by examining labor market outcomes in adulthood.

Finally, the findings of this paper also connect to a large literature examining the factors that facilitate a structural shift of employment out of agriculture in developing countries. Existing work has emphasized the contemporaneous impacts of new agricultural technologies (Bustos et al., 2016), trade liberalization episodes (McCaig and Pavcnik, 2018), and changes in relative prices of factors of production across sectors (Hornbeck and Naidu, 2014). Our results suggest that early-life interventions to foster human capital investments can contribute to the transition from low productivity farming to more productive, diverse jobs in a setting in Sub-Saharan Africa, exactly the region with the greatest potential for structural change. Since these investments take a long time to materialize into increased human capital, they could contribute to structural transformation only in the long run.

The rest of the paper is organized as follows. Section 2 provides background information about the consequences of iodine deficiency and the supplementation program. In sections 3 and 4, we describe our data and empirical strategy respectively. Section 5 presents the

[6] Behrman et al. (2003) and Linnemayr and Alderman (2011) study an intervention that provides nutritional supplements as well as other basic health services such as immunization and deworming campaigns. They focus on child schooling and health as their outcomes. Carneiro et al. (2020) examine the effects of a similar intervention which, in addition, provides information to mothers and fathers on practices related to pregnancy and infant feeding as well as unconditional cash transfers to mothers. They explore the outcomes of children aged 0-60 months.

empirical results, including robustness checks. Section 6 includes an interpretation of the magnitude of the results. Section 7 concludes.

2 Background

2.1 Iodine Deficiency and its Consequences

Iodine is essential to the functioning of the human body. It is used in the production of thyroid hormones, which function as regulators of metabolism and nervous system activity. It is naturally derived from the ocean and soil, so the most important sources of iodine in the human diet are found in plants, animals, drinking water, and seafood. In areas where the concentrations of iodine in the soil is low, typically in mountain regions susceptible to erosion, and geological shields (large areas of exposed Precambrian rocks), and in those regions far from the ocean, the population could not obtain adequate iodine from their diet for maintaining the production of thyroid hormones.

Insufficient iodine intake is often manifested as goiter, a disease that results in the enlargement of the thyroid gland to become more effective in the production of thyroid hormones. Low levels of iodine intake have been also linked to impaired mental function, and severe and prolonged iodine deficiency can cause a reduction in thyroid hormone synthesis (a condition known as hypothyroidism) and retarded physical development (cretinism).[7] These diseases associated with a diet lacking in iodine are collectively known as iodine deficiency disorders (IDD).

Although important during the lifecycle, adequate levels of iodine are particularly determinant for fetal brain development. *In utero* exposure to insufficient levels of iodine, especially during the first trimester of pregnancy, can significantly influence the density of neural networks formed in the developing brain, affecting the development of the central nervous system and causing long-lasting cognitive damage.[8] This can even occur with mild insufficient iodine intake and is hardly reversed with subsequent supplementation (Zimmermann and Boelaert, 2015). While the consequences of in utero iodine deficiency have been well documented, there is much less consensus in the medical community on whether or not low iodine intake during the postnatal period has permanent and irremediable consequences on brain development. While some studies have found limited benefits of iodine supplementation during the first years of life (Cao et al., 1994), some stress that infant iodine nutrition

[7] With hypothyroidism, the body processes slow down and the metabolism becomes sluggish. This can result in weight gain, impaired memory, slow speech, reduced heart function, increased blood cholesterol, and fatigue.

[8] See, for instance, Zoeller and Rovet (2004), Cao et al. (1994), Hetzel and Mano (1989), and Pharoah and Connolly (1987).

during breastfeeding might prevent impaired neurological development (Azizi and Smyth, 2009).

2.2 Iodized Oil Capsule (IOC) Distribution in Tanzania

Tanzania was one of the first countries to implement a large-scale intervention for iodine supplementation in the world.[9] Like many other African countries, Tanzania suffered from very high levels of IDD. According to a report by the World Health Organization (1993), the average prevalence of goiter, a widely used *proxy* for iodine deficiency prevalence, across districts in Tanzania was about 40 percent in the 1980s, with several districts having rates over 70 percent.

The program distributed iodized capsules (IOCs) at different points in time over a period of 8 years, focusing on districts that had a goiter prevalence above 10 percent. Priority was given to women of childbearing age, as fetal brain development crucially depends on iodine, then, in decreasing order of priority, distributed to children 1-5 years of age, older children, and adult men 15-45 years old. During the duration of the program around 5 million women of childbearing age and children received IOCs.

The supplementation program was scheduled to start simultaneously in all treated districts in 1986, and additional doses were set to take place in subsequent years in each district. However, there were several delays in many districts due to technical administrative and coordination problems between the central government and local offices. Initially, local offices were responsible for organizing the distribution teams and coordinating with community leaders the days for distribution. The central government provided allowances to cover fuel and other direct costs of distribution. The time required for local offices to set a distribution team and coordinate with community leaders, altogether with the timing of additional funds from the central government was a major source of the initial delays. These initial delays made capsules to expire and consequently generated new delays. These problems were reduced when the government centrally set the distribution teams and all of the basic distribution expenses. Data about the program rollout are shown in Appendix Table A.1.

Supplementation with IOC has been shown to be effective in rapidly correcting and preventing iodine deficiency —and indeed follow-up surveys suggest that the program reduced iodine deficiency prevalence by more than half immediately after program implementation (Peterson, 2000). Once ingested, it provides fetal protection against iodine deficiency for over 4 years (Buttfield and Hetzel, 1967), so children conceived just before or around three years after campaign are likely to have received *in utero* protection. This specific feature of

[9] This section draws on original program from Peterson et al. (1999).

the program, alongside variation in when the iodine supplementation campaigns took place in each district, motivates the empirical strategy that we outline below.

3 Data and Measurement

3.1 Census Microdata

Our primary source of data is the 2012 Tanzanian Census, the most recent population census available. We use the 10 percent randomly drawn sample available from the Integrated Public Use Micro Sample (IPUMS). These data provide information on an individual's education, occupation, status in employment (class of worker), and a set of demographic characteristics such as age and gender. Crucially for the purposes of this study, census enumerators collected information on the district of birth. This information, along with the year of birth, allows us to infer the extent to which individuals were exposed to the program. We limit the sample to individuals born between 1977 and 1994, who are in their 20s and 30s at the time they are observed in the census. These individuals have typically completed their education decisions, and many are part of the labor force. Descriptive statistics of the variables used in the paper are shown in Appendix Table A.3. We focus on census data rather than household surveys because they allow us to generate a relatively large sample in each district and birth cohort.

Our main measure of educational attainment is total years of completed education, which is capped at 15 in the original data.[10] We also examine separately total years spent in primary and secondary school, and indicators for different educational levels, which helps understand possible heterogeneities and distributional impacts of the program.

With respect to labor market outcomes, we look at employment and occupational attainment measures. In particular, we define indicators for the probability of a worker being employed in a position belonging to a given type of occupation and calculate the average total years of education in an individual's occupation category. We also consider an indicator for skilled occupation following the International Labor Organization (ILO, 2012).[11] Finally, we also look at the extensive margin of labor by defining an indicator for the probability of working in the previous 12 months.[12] Ideally, one would also examine individuals'

[10] This cap corresponds to "university and other related." This prevents us from analyzing separately changes in total years spent in tertiary education. However, rates of tertiary education in Tanzania are low. In our sample, only 3 percent of individuals completed some tertiary education.

[11] According to the ILO (2012), the following occupation categories correspond to skilled work: legislators, senior officials, managers, professionals, clerks, sales, services, and skilled manual. Unskilled occupations correspond to self-employed agriculture, crafts and related trade workers, and unskilled manual.

[12] During the census interview, respondents are asked their employment status both last week and year. We focus on individuals' employment status during the previous 12 months because it is less likely to be

wage earnings, but unfortunately, the census did not collect this information.[13]

The baseline sample consists of around 1.2 million records, a number that varies across outcomes because of missing observations. The estimation sample is notably smaller for our measures of occupational attainment (about 0.8 million records), as information on an individual's occupation is available only for those who are employed at census time. Since the key identifying variation occurs at the district- and birth cohort-level, we collapse the data into cell groups by the district of birth, year of birth, and gender to ease the computational burden.[14] The resulting cell means are used as dependent variables in our regressions, which are weighted by cell population size. The total number of cells is 4068 (113 districts, 2 genders, 18 birth cohorts).

3.2 Measuring Program Participation

The medical literature suggests that iodine deficiency has particularly irremediable consequences on brain development if deprivation occurs during the first trimester of pregnancy. As a result, we define a treatment variable that measures the extent to which an individual was exposed to the program during the first trimester of pregnancy. For simplicity, we use the terms "first-trimester exposure", "prenatal exposure", and "program exposure" interchangeably. Our calculations follow in general the same idea and assumptions as Field et al. (2009).

We obtain data on campaign dates from Field et al. (2009), which are listed in Appendix Table A.1. We have information on the year of the campaigns, but not the month. Therefore, we assume that the calendar month in which a given campaign starts follow a uniform distribution. We also assume that each campaign took a three-month period to reach all individuals in a district based on reports by the Tanzanian Food and Nutrition Centre (TFNC). Previous studies indicate that the minimum level of iodine in the body to provide

influenced by seasonal employment. However, the conclusions are unaffected if we instead use employment status in the last week.

[13]Some household surveys do provide information on income or other measures of socioeconomic status. However, most of these surveys do not provide information on an individual's district of birth, and assigning program exposure based on the current district of residence could be problematic due to selective migration and measurement error. Moreover, by their nature, our research design would have low statistical power in household surveys due to small sample sizes. For example, in the Tanzanian Panel Survey, there are only 6000 observations with information on income, out of which only 489 observations have a positive probability of program exposure. The use of these surveys to evaluate the supplementation program is unlikely to generate reliable estimates of its effects.

[14]The results are virtually identical if we instead use the underlying individual-level microdata to estimate our regressions, which is unsurprising given the source of variation we use. We employ frequency weights given by the IPUMS in all of our analyses. When aggregating these data, we first expand the sample using the individual frequency weights, yielding an expanded sample consisting of about 12 million records. This expanded sample is then collapsed into cell-group means.

full protection against fetal iodine deficiency is 6.5mg, with partial protection until reaching 4.2 mg. Field et al. (2009) assume that 85 percent of iodine from 380 mg supplements is extracted in urine in the first month and the remaining 57 mg is depleted hyperbolically. Under this assumption, individuals conceived within 25 months after the campaign would have full protection, while those conceived between 26 and 52 months after the IOC would have partial protection. We also follow this assumption, although our results are very similar when we assume different depletion patterns.

Adjusting for the timing of distribution periods over the year, these assumptions imply a probability of program exposure during the first trimester of pregnancy for each birth month.[15] Note that the source of uncertainty in our measure of program exposure is given by the absence of exact campaign month and the depletion rate of iodine in the mother's body to provide protection against fetal iodine deficiency (i.e., individuals conceived in subsequent years are less likely to receive full protection).

These probabilities of program exposure implicitly assume that each treated district had only a single IOC campaign. Since there were on average two IOC campaigns per district, taking place 2-4 years after the previous campaign, these calculations may substantially underestimate the likelihood of fetal protection for some individuals and thus the overall program's impacts. This issue is illustrated in Figure 1, which shows the differences in program participation in districts with one and two IOC campaigns, respectively. To address this issue, we calculate the probabilities to account for overlapping exposure by assigning the IOC campaign that maximizes the probability of program participation for individuals born after a given number of IOC campaigns.[16]

With information on month and year of birth, one would assign a likelihood of exposure during the first trimester of pregnancy to each individual. However, we have no information on an individual's month of birth. Therefore, we average monthly probabilities to obtain birth-year-specific probabilities of program exposure (shown in Appendix Table A.2). Of course, this assignment is likely to suffer from measurement error, particularly for individuals born just before or after program adoption. If this measurement error is uncorrelated with actual program participation, then it would introduce an attenuation bias in our estimates and thus our results should be viewed as lower bounds of the true effects of the program. A

[15]In Appendix Table A.10, we present results from a specification that uses exposure dummies for the years around program adoption rather than the parametric measure of exposure. We present these results for our core outcomes and find evidence that is consistent with our baseline specification.

[16]For individuals whose probability of exposure is zero, there could be at least two campaigns consistent with this treatment. In the parametric regression estimates, this has no implication in terms of calculations. However, to compute non-parametric event study estimates, we have to assign a campaign year to these individuals. In these cases, we assign individuals to the nearest campaign from the birth year. The results are virtually unchanged if we instead assign them to the first campaign in the district.

possible concern is that different families may have timed date of birth in response to the program, potentially changing the composition of exposed individuals and introducing a selection bias. In this case, our estimates of program effects may be biased in an ambiguous direction. As documented in Section 5.5, there is no evidence that the program led to systematic changes in fertility choices. Furthermore, we find that a number of predetermined maternal characteristics are uncorrelated with program exposure, which is consistent with the absence of changes in the composition of exposed individuals due to changes in the timing of conception.

3.3 Other Data

We compile a set of socioeconomic and demographic district-level characteristics from the 1988 census, which is the closest available census since the first iodine supplementation campaign. We also collect data on the district's elevation and distance to the nearest coastline, which could be correlated with natural access to iodine. We use these variables to control for possible differences in trends associated with these characteristics in our basic specification. These variables are described in Appendix A.

In supplementary analyses, we also use data from the 1999 Tanzanian Demographic Health Survey (DHS). The DHS is a nationally representative survey of women ages 15-49. We use these data to check for the balance in predetermined child and maternal characteristics (see Section 5.5). These data provide information on month and year of birth, so we match individuals to monthly likelihoods of fetal exposure to the program. The conclusions are the same if we instead use annualized likelihoods of exposure. We use these data at the individual (rather than district) level.

4 Empirical Strategy

Our empirical strategy exploits variation in *where* and *when* the iodine supplementation campaigns took place to identify their long-run effects. As an illustration, suppose that iodine deficiency early in life has a negative impact on education and that the largest impacts stem from exposure during the *in utero* period. Consider two districts, one where an iodine campaign took place in 1986 and another one where the same campaign was implemented in 1987. In the first district, individuals born in 1986 have a positive likelihood of fetal exposure, while those born in 1985 were only exposed to the program during the first year of life and thus have zero probability of prenatal exposure. Therefore, one would expect the 1986 cohort to have completed more years of education relative to the 1985 cohort. In the second district, one would not expect such a systematic pattern because the 1985 and 1986 cohorts in that district were not exposed to the program during the prenatal period. By comparing the differences in educational attainment between the 1985 and 1986 cohorts in the first district vis-à-vis the differences in the second district, one would isolate the effect of prenatal iodine supplementation on education.[17]

This idea can be generalized into a regression framework employing the following specification:

$$Y_{gjt} = \alpha_g + \mu_j + \gamma_t + \pi \cdot \mathbf{IOC}_{jt} + \mathbf{Z}'_{jt}\beta + \varepsilon_{gjt} \tag{1}$$

where Y_{gjt} is one of our long-run education and labor market outcomes for cohorts born in district j in year t and gender g. The right-hand variable of interest is $\mathbf{IOC}_{jt}$, which measures the probability of program exposure during the first trimester of pregnancy (as described in Section 3.2). The district fixed effects, μ_j, control for permanent unobserved determinants of long-run outcomes across districts, while the inclusion of birth-year fixed effects, γ_t, nonparametrically adjust for general trends in the outcomes of interest. The equation also includes interactions between baseline district characteristics and linear time trends, $\mathbf{Z}'_{jt}$, to account for possible differential trends associated to these factors. The vector $\mathbf{Z}'_{jt}$ also includes specific linear time trends in Tanzania's six major regions (Coastal, Northern, Lake, Zanzibar, Southern, and Central), which controls for substantial cross-region differences within Tanzania. All regressions based on the estimating equation 1 are weighted by cell population size to adjust for differences in precision with which cell means are estimated. We use standard errors that are clustered at the district level to account for potential serial correlation. In total there are 113 districts, out of which 27 received IOC campaigns.

The key parameter of interest is π, which summarizes the magnitude of the program's impacts. The differences in treatment exposure are given by the probability of having been prenatally exposed to iodine supplementation during the first trimester, which is determined by the timing of the campaigns relative to the birth year. Positive and significant estimates would suggest that greater exposure to the program during the first trimester leads to improvements in completed schooling and occupational attainment.

The estimating equation 1 assumes that the largest effects of the program operate from exposure during the prenatal period. We can test this assumption by using a flexible event-

[17] This difference-in-differences comparison assumes that differential benefits of iodine supplementation during the first versus second year of life are negligible. The event-study results shown below provide evidence consistent with this view.

study framework to estimate the effects of exposure at different ages:

$$Y_{gjt} = \alpha_g + \mu_j + \gamma_t + \sum_{\tau=-7}^{4} \pi_\tau \cdot \mathbb{1}\left(t - T^*_{jt} = \tau\right) + \mathbf{Z}'_{jt}\beta + \varepsilon_{gjt} \tag{2}$$

where $\mathbb{1}(\cdot)$ are indicators equal to 1 when the year of campaign, T^*_{jt}, is $\tau = -7, -6 \ldots, 4$ years since the year of the campaign, T^*_{jt}, in district j. Values of τ between -7 and -1 indicate exposure to a given campaign after birth, while values of τ greater or equal to zero denote some exposure during the prenatal period. π_τ is normalized so that it is equal to zero for cohorts who were 8 years of age or more when the campaign occurred in their district. All the exposure indicators are set equal to zero for individuals in districts where no campaign took place. Point estimates of π_τ provide a detailed picture of the program's effects in a transparent and nonparametric way.

The key assumption in this analysis is that the outcomes of individuals born in districts with varying IOC status would have had similar trends in the absence of the program. Under this identifying assumption, our empirical framework yields estimates of the causal effects of iodine supplementation on long-run outcomes. With the inclusion of district and time fixed effects, we exploit variation in the timing of campaigns *net* of any district-level factors that are time-invariant and overall trends that might affect the outcomes. Furthermore, the additional set of controls accounts for differential trends in the outcomes across regions over time and across districts with potential differences in baseline district characteristics. These controls absorb residual variance and reduce the risk of spurious estimates of the program's impacts driven by other factors (although, as we show below, the conclusions are essentially the same without these controls).

In Appendix Table A.4, we provide the first piece of evidence supporting the plausibility of the identifying assumption. It presents estimates of the relationship between the timing of IOC campaigns and baseline district characteristics (also illustrated graphically in Appendix Figure A.1).[18] Out of 13 variables, none is correlated with the year in which each district conducted an IOC campaign. Moreover, the estimated coefficients are small in magnitude. For instance, a difference of 8 years between IOC campaigns would be associated with a difference of 0.24 percentage points in the share of the population with four years of education or less, which represents only 0.4 percent of the sample mean. This lack of association is consistent with the narrative that the variation in IOC status across districts stemmed largely from logistical issues rather than from expectations about future local development prospects.

[18] This test uses all the information on campaigns. For example, if a district had two iodine campaigns in different years, then this district is included twice in the sample. The conclusions are the same if we instead use the first year of the campaign for each district.

In addition, and most importantly, the results from the flexible event-study specification 2 allow us to more directly judge the plausibility of the identifying assumption. If the identifying assumption is valid, then the estimated effects of the program should be small and statistically insignificant for individuals exposed to iodine supplementation campaigns at older ages, when the benefits of the program are if anything negligible, as suggested by the medical literature (Zimmermann et al., 2008; Hetzel and Mano, 1989). Large and significant estimates would suggest that our results are driven by pre-existing trends in the outcomes of interest. As we shall see, the results from these event studies are broadly consistent with the validity of the identifying assumption.

A possible threat to the estimation strategy is that during the campaigns, health workers might provide health information and encourage both prenatal and postnatal investments, which may in turn affect later-life outcomes independently of iodine supplementation. However, existing research suggests that such other aspects of the program were unlikely to be important in practice. In their detailed analysis of the program's costs and coverage, Peterson et al. (1999) do not report any information indicating that such additional health information or other health services existed. Consistent with this, Adhvaryu and Nyshadham (2016) document that the program is not correlated with changes in measures of physical health at birth, such as birth weight and size, which is inconsistent with what one would expect if there were other prominent health services during pregnancy (Behrman and Rosenzweig, 2004; Bharadwaj and Lakdawala, 2013).[19]They also show that investments at birth are not associated with program exposure, which is again inconsistent with the presence of other concurrent health services. These findings increase our confidence in the research design. But we also rely on the fact that, to the extent that such other aspects of the program existed, they should affect all cohorts born around a given campaign and are therefore very unlikely to generate the sharp patterns that we document below.

Another issue, which is inherent to any analysis of this sorting, is that parental investments may respond to changes in birth endowments, and this, in turn, may exacerbate or dampen the baseline impacts of the program (Almond and Mazumder, 2013; Almond et al., 2018). If parents respond by devoting more (less) investments in higher endowed children, then the role of the intervention or biological mechanism will be magnified (dampened). Adhvaryu and Nyshadham (2016) provide suggestive evidence that parents devote

[19] Adhvaryu and Nyshadham (2016) use data from the TDHS. Unfortunately, these data provide no information on prenatal care during the relevant period of study. However, given the evidence in the medical literature that iodine deficiency has no first-order effects on physical health, the lack of a correlation between program exposure and birth outcomes provides compelling evidence that any relevant additional health information or health services associated with the program are unlikely to play a major role. In Section 5.4, we provide additional evidence that is consistent with this view by showing that the program does not affect long-run health outcomes.

more investments in children who were exposed to the program *in utero*. Our reduced-form estimates cannot separately identify the importance of each mechanism, but rather their combined influence. From a policy perspective, this is an important parameter that captures the overall effects of iodine supplementation, including the changes in parental investments that typically accompany the changes in birth endowments in developing countries (Almond and Mazumder, 2013). After presenting the basic results, we discuss other possible threats to the research design and empirically evaluate their relevance.

5 Results

5.1 Educational Attainment

We begin by graphically examining the relationship between IOC campaigns and long-run educational attainment. Figure 2 presents coefficients and 95 percent confidence intervals from estimating equation 2. It shows that there are significant positive impacts on completed years of education for individuals born after IOC campaigns. The estimates fall sharply for individuals born immediately before the program. This suggests that there are little to no differential benefits from exposure after birth. The lack of statistically significant effects of exposure at older ages suggests the absence of pre-existing trends and yields support for the identifying assumption.

Table 1 presents formal regression estimates based on equation 1. These results are consistent with the graphical evidence but can be parsimoniously displayed in tables. Column (3) shows results from the preferred specification (1) that includes the full set of controls. The coefficient of interest, π, is estimated at 0.174 and statistically significant at the 1 percent level. This suggests that individuals with full IDD protection during the prenatal period completed an additional 0.17 years of education. Relative to the sample mean this implies a 2.5-percent increase in educational attainment.

The other columns investigate the robustness of the results to alternative specifications. Column (1) estimates a model that only includes controls for gender, district, and year of birth fixed effects. Column (4) uses a specification that controls for region-by-year fixed effects (rather than region-specific linear time trends), which uses only within-region variation to estimate the program's impacts. These alternative specifications do not substantially alter the key coefficient of interest, providing further support for the plausibility of the identification assumption. Our preferred specification reported in column (3) that includes region-specific linear trends and interactions of baseline district characteristics with a linear trend (besides the basic set of fixed effects) yields more precise estimates compared to alternative specifications.

To uncover more detail about the relationship between IOC campaigns and long-run education, we repeat the baseline specification separately for the probability of completing exactly different levels of education. Table 2 shows that ICO campaigns improved educational attainment by increasing the fraction of individuals moving from primary to secondary education. Indeed, exposure to the program is associated with a decline of 2.1 percentage points in the probability of having completed exactly primary school, which mirrors the increase in secondary school completion rates (column 3). This effect represents a 12-percent increase relative to the sample mean. In contrast, there is no evidence that the program is associated with changes in tertiary education, which is perhaps unsurprising given the limited supply of tertiary education in Tanzania.

In columns (5)-(6), we separately examine the intensive margin of primary and secondary attainments. We find larger effects on secondary than on primary attainment. Individuals with full exposure to the program spent 0.32 more years in secondary education, which reflects in part individuals acquiring at least one year of secondary school. In contrast, the gains in primary education are about 0.07 years, and marginally statistically significant at the 10 percent level.

These results suggest that, in the absence of the program, many individuals would not have transitioned from primary to secondary education. This finding makes sense in our context. The transition from primary to secondary is the typical time when many students drop out of school. Improvements in cognitive abilities may increase the returns to attending secondary school, or improve the chances of passing in the Primary School Leaving Examination (PSLE), which is required to attend any secondary school in Tanzania. By contrast, primary attainment is already high —85 percent of individuals have at least one year of primary education —so the scope for gains in this margin is much more limited.

Overall, the program led to gains in educational attainment, and this occurred by increasing transition rates from primary to secondary education. These effects operate primarily from exposure during the prenatal period. This is broadly consistent with the view that an inadequate supply of iodine during the *in utero* period can have irreversible consequences independently of subsequent supplementation (Zimmermann et al., 2008; Hetzel and Mano, 1989).

5.2 Labor Market Outcomes

Having established that the program led to robust and persistent improvements in educational attainment, we turn next to explore impacts on labor market outcomes. If iodine deficiency severely reduces individual capacity at work, then exposure to the program could increase labor supply and employment rates among treated cohorts. We find, however, limited evidence that greater exposure to the program is associated with changes in employment. Column (1) of Table 3 shows that the relationship between these variables is statistically insignificant at conventional levels of significance. Note that this result is not driven by large and uninformative standard errors. Using 95 percent confidence intervals based on our estimates, we are able to rule out changes in employment rates larger than 3 percentage points as a result of full exposure to the program, which is small relative to the sample mean of 73 percent. These results are supported by Appendix Figure A.4, where we use the event-study specification to estimate the impacts of the program at different ages of exposure. As shown in the figure, there are no statistically significant differential trends in employment rates either before or after birth.

In the rest of the columns, we examine the program's impacts on occupational attainment. We begin by examining the impacts of the program on a measure of occupational skill score, defined as the average years of schooling of the individuals in the same occupation, status in employment, and gender cell.[20] An increase in this index would suggest that individuals are engaging in activities that generally demand more skilled labor. Since we find that the program affects long-run completed education, we use the 2002 Tanzanian census for skill assignments to reduce any possible mechanical link.[21] Column (2) documents that greater exposure to the program *in utero* leads to an increase in the occupational skill scores. Moving from no to full exposure would increase this index by about 0.04 years of schooling, or 0.7 percent relative to the sample mean.

In columns (3), we alternatively define the occupational skill score as the fraction of workers in each category of occupation, status in employment, and gender cell that has completed at least one year of secondary school. This is the margin of education where the effects of the program are the largest, so this index could be a more natural measure to understand the mechanism of employment. Consistent with the results above, we find positive effects on this index. The estimates indicate that moving from no to full exposure would increase the fraction of workers with at least a secondary school degree in the job individuals hold by about 0.4 percentage points, an estimate that is statistically significant at less than 1 percent level.

We next examine the effects of the program on the probability of holding a job in a given occupation. In Columns (4)-(7), we define skilled and unskilled occupations using

[20] We obtain the same conclusions if we only use the occupation (rather than with status in employment and gender) categories to define this variable.

[21] The classification of occupations in the 1988 census are not comparable to that in the 2012 census, which prevents us from using the 1988 census for skill assignments. We find very similar patterns if we rather use the 2012 census or IPUMS samples from other African countries to rank occupations according to their average level of education.

the categorization of occupations' skills of the ILO (2012). A greater likelihood of fetal exposure to the program is associated with a significant increase in the probability that an individual holds skilled employment. Full exposure to the program would imply an increase in this variable of approximately 1.1 percentage points. This estimate represents a 5-percent increase in the probability of holding a skilled job.

In columns (5)-(7), we break the unskilled category into its subcomponents. There is no evidence of a meaningful change in the probability of working in a "craft" or related trade occupation, or in the likelihood of holding an elementary occupation. Instead, the increase in skilled employment arises primarily from a reduction in agricultural self-employment, one of the occupations with a greater fraction of less-educated workers (i.e., those who did not go to secondary school). Indeed, the reduction in the probability of holding a job in this occupation is nearly identical in magnitude to the increase in the probability of holding a skilled job. Figure 3 illustrates graphically this transition from self-employment in agriculture to skilled work.[22]

Summary. The results in this section suggest that the program significantly affected individuals' occupational choices, with limited impacts on overall employment rates. Iodine supplementation *in utero* facilitated the acquisition of education and this in turn helped exposed cohorts to transition towards jobs that demand more skilled labor and have higher economic returns.

The null results on the extensive margin may seem surprising at first glance, given the significant gains observed in completed schooling. However, existing literature examining long-run outcomes suggests that education is not the most likely mechanism for the extensive margin of labor. For instance, Chetty et al. (2014) documents that improvements in teacher quality fail to predict employment probabilities in adulthood, despite there are significant gains in education and income. Similarly, Chay et al. (2014) find that black cohorts who were born following the 1964 Civil Rights Act in the South, which promoted health investments in early life, experienced large gains in education and earnings in adulthood, but not in employment rates.

Most remarkably, it is unclear that employment is an unambiguous measure of an individual's socioeconomic status in our setting. In the context of a poor economy, where a large fraction of the labor force is self-employed in family businesses in subsistence and

[22]Given the shift out of agriculture, a question that arises is if there is also increasing migration to urban areas. However, we find no evidence that this is the case when we estimate models where the dependent variable is an indicator for living in an urban area. This suggests that the transition from self-employment in agriculture to skilled work did not occur primarily through increasing migration to urban areas in our setting.

low-paid sectors, being employed may not automatically mean higher socioeconomic status. Similarly, in these contexts, improvements in socioeconomic status could not primarily reflect in higher employment rates, but rather in higher transition rates from unpaid or low-paid jobs to higher-paid positions.[23]

5.3 *Gender Differences*

Table 4 reruns our main specification separately for males and females. Since we estimate separate regressions by gender, we allow for gender-specific trends in the outcomes of interest. We find that, although both male and female schooling significantly increases with program exposure, female schooling increases more than male, with coefficient estimates of 0.13 and 0.20, respectively. The differences in estimated coefficients are not statistically significant (with a p-value of 0.106), so we take the magnitude and direction of these differences as suggestive (rather than definitive) evidence of larger effects for females.[24] This gender difference, though insignificant, is consistent with the hypothesis in the medical literature that female fetuses are more sensitive to iodine deficiency *in utero* than males (Friedhoff et al., 2000).[25]

We also examine whether there are significant differences in labor market outcomes in columns (3)-(16). We look both at the extensive and intensive margins. The employment results are not statistically significant both women or men. Regarding the intensive margin, we find estimates that tend to be larger for males than for females, though these differences are insignificant in all the cases. The direction of these differences is difficult to reconcile with the biological mechanism discussed above. One possibility is that there are indeed differential gains in cognitive capacity by gender, but the effects on labor market outcomes depend ultimately on market conditions. This is consistent with the lower employment rates for females relative to males (68 versus 78 percent) and the wage gap of about 40 percent

[23] The lack of significant effects on employment also makes in light of the non-significant finding on disability that we document in Section 5.4, as some physical disabilities can directly limit work capacity.

[24] This is in line with previous work conducted both in Tanzania (Field et al., 2009) and the United States (Adhvaryu et al., 2018; Feyrer et al., 2017). Unlike Field et al. (2009), who examine grade attainment among children ages 10-13, our results cannot be explained by gender differences in the age of school entry because we are analyzing long-run completed education. Differences in the age of school entry may explain differences in treatment effects in mid-term schooling outcomes if female outcomes are more likely be observed when the program's impacts are the largest. But these differences should dissipate over time when individuals have completed their schooling decisions.

[25] The lack of significance in the difference between the estimated effects by gender does not allow us to confirm the hypothesized biological mechanism. A possibility is that the biological mechanism is driven by the left tail of the health distribution, and the program effect is mainly in the middle of the distribution, so it is plausible that the gender differences would not be present. Further research is needed to clarify these relationships.

in favor of males.[26] Our results are consistent with Adhvaryu et al. (2018), who find that the effects of salt iodization *in utero* on the intensive margin of labor are more precisely estimated for men than for women.[27]

5.4 Are there Long-Run Health Effects Associated to IOC?

We next explore the impacts of the program on a set of physical health outcomes, which may constitute important mechanisms for long-run effects on education and labor market outcomes. We use the census data to construct indicators for any serious disability and for individual disability categories. We also consider a variable measuring the total number of disabilities. These outcomes have been examined in the previous literature about the fetal origins hypothesis (Almond, 2006; Dinkelman, 2017). While one could expect health conditions to be an important pathway, we find limited evidence supporting this hypothesis. Although point estimates are in general negative, they are small and statistically indistinguishable from zero (see Table 5). We interpret these findings as suggestive evidence that the program affected education and labor market outcomes by affecting the middle of the "health" distribution rather than the extreme left tail of the physical/cognitive health distribution. This finding is consistent with the medical literature pointing out that iodine deficiency does not have a first-order effect on extreme physical health outcomes.[28]

5.5 Selection into the Sample: Mortality and Fertility

A potential threat to the estimation strategy is that parents may influence the timing of conception to take advantage of the *in utero* benefits of the program. If higher quality parents are more likely to engage in this type of behavior, then our estimates may be driven not by iodine availability, but by changes in the composition of women giving birth. We can check for possible changes in composition by examining whether treatment intensity is correlated with predetermined child and maternal characteristics. Using data from the DHS, Appendix Table A.5 shows that program exposure fails to predict child gender, childbirth order, an indicator for the child to be a twin or multiple births, mother's age, mother's education (including years of schooling, an indicator for secondary or higher school degree, and an indicator for literacy), number of children, and type of residence location (urban

[26] We use data from the Tanzanian Panel Survey (2008/2009) to calculate the mean difference in log wage between men and women.

[27] Adhvaryu et al. (2018) find effects on employment that are larger for women than for men. They argue that the relatively smaller effects on men are largely due to the fact that employment rates among men were high (contrasting with the remarkably low female labor supply), limiting the scope for additional gains.

[28] For a review, see Allen and Gillespie (2001). Our findings are also consistent with Field et al. (2009) who find no clear impacts on short-run health conditions of children exposed to the program.

versus rural). The lack of association between program exposure and these variables is consistent with the evidence in Adhvaryu and Nyshadham (2016) that the program did not lead to systematic changes in fertility. Given this evidence, we believe that selective fertility is unlikely to play a role in driving the results above.

A related concern is that we can observe individuals in our sample only if they survive until the time they are enumerated in the census. Since life expectancy at birth in Tanzania is about 50 since at least 1976, it is little likely that attrition due to adult mortality leads to systematic changes in the composition of our sample.[29] Rather, attrition could introduce an important bias in our analysis if iodine deficiency *in utero* affects miscarriages and infant mortality risk. In particular, if program exposure was associated with large reductions in mortality, saving weaker individuals at the margin of survival, then our sample of treated individuals would be negatively selected. This would imply that our results are likely to underestimate the true effects of the program on long-run education and labor market outcomes.

We can check for the selection of treated cohorts by looking directly at changes in cohort sizes. If there were meaningful fertility and/or mortality effects as a result of the program, they should be reflected in larger cohort sizes among those with greater exposure to the program. To examine this possibility, we collapse the data at the district-of-birth and year-of-birth level and define the dependent variable as (log) cohort size in 2012. The results are shown in Appendix Table A.6, column (1). We find that greater exposure to the program *in utero* is not associated with statistically significant changes in cohort size, with an estimated coefficient that is small in magnitude.

We also examine possible changes in sex ratio, given the evidence that events and circumstances *in utero* can have gender-biased effects on fetuses (Trivers and Willard, 1973). We calculate the ratio of male to female cohorts by district and year of birth using our main estimation sample from individuals who were interviewed in the 2012 census. Columns (2) of Appendix Table A.6 shows small and statistically insignificant effects of *in utero* exposure to the program on sex ratios.

Overall, there is no indication that selection into the sample due to either mortality or fertility is a significant problem in our setting.[30]

[29] See `https://www.theglobaleconomy.com/Tanzania/Life_expectancy/`, last accessed on May 25, 2019.

[30] This does not imply that the program would not affect stillbirths and infant mortality risk. Rather, our results suggest that if such effects exist, they must not be large enough to introduce a systematic bias in our estimates.

5.6 Additional Robustness Checks

In this section, we conduct several sensitivity checks to evaluate the robustness of our key findings. For parsimony's sake, we focus the indicator for skilled work when looking at labor market outcomes in view of the evidence in Section 5.2.

Our baseline measure of program exposure assumes that conception took place nine months before birth. We also assume a uniform distribution in birth timing within years. In practice, not all pregnancies last nine months and some births are more likely to occur in certain months of the year. In Appendix Table A.7, we examine the robustness of our core results to these assumptions. We check the sensitivity of our results to gestational age using an alternative exposure variable that is just under the full term. We also use data from the DHS and directly account for the seasonality of birth. When implementing these changes, we obtain birth-year-specific probabilities of program exposure that are extremely similar to the baseline. Given this, the point estimates and standard errors remain unchanged when using these alternative measures.

A possible concern with the results is that some campaigns took place at the beginning of the 1990s when northwestern districts received more than 0.5 million refugees from the civil conflict of Burundi and Rwanda (Baez, 2011). One could be concerned that our results are driven by changes in economic conditions in these districts rather than by iodine availability. We investigate this concern in Appendix Table A.8. Since the refugees were settled in camps relatively close to the borders with Burundi and Rwanda, we can use the distance to the border as a measure of refugee intensity (as in Baez (2011)). Columns (2)-(3) estimate the baseline specification but add interactions between distance to the border and linear birth cohort trends. In columns (4)-(6), we rather exclude districts within cutoff distances from the border. Point estimates and significance are hardly affected by these sensitivity checks.

Our analysis exploits variation across birth cohorts and districts. However, one could be concerned that the estimates are driven by particular areas or cohorts, calling into question the generalizability of our findings. To address this issue, we rerun the baseline specification repeatedly, but each time excluding a specific group of observations. The results are shown in Appendix Figure A.2, which plot the estimated effects from separate regressions in which a specific group of observations is excluded. The results are remarkably stable to models that exclude different birth cohorts, or birth-districts.

In all of our regressions, standard errors are clustered at the district level to account for possible serial correlation across cohorts. A possible limitation of these standard errors is that they do not account for possible spatial correlation in the residuals, which could affect inference if program exposure is correlated over space across districts. Such a correlation could arise if the campaigns were sequentially implemented in groups of neighboring districts to minimize the operational costs of the program. As a simple robustness check, column (2) of Appendix Table A.9 shows two-way clustered standard errors at the district and year of birth to adjust flexibly for both serial and spatial correlation in error terms. While these standard errors are slightly larger, the conclusions remain unchanged.

In column (3)-(7), we also compute spatially-corrected standard errors using the procedure proposed by Conley (1999). This procedure calculates a spatially weighted covariance-variance matrix, with declining weights from 1 to 0 until a given cutoff is reached. Using different cutoff distances varying from 100 to 800 kilometers, we find that Conley standard errors are generally lower than the baseline. In sum, statistical significance is largely unaffected under these alternative covariance-variance matrixes that address spatial correlation in error terms.

To further check the robustness of the results, we conduct a test that measures how likely are we to observe an impact if, instead of using the actual probability of fetal exposure to the program, we used randomly assigned treatment probabilities. We perform this permutation test by randomly assigning each of the birth cohorts across districts a campaign year, and then estimating model 1 using the implied placebo treatment probability. This procedure is repeated 1000 times. A high fraction of absolute placebo coefficients that are larger than the absolute actual coefficient would suggest that our results are likely to have arisen by chance rather than by the existence of a true causal relationship. Appendix Figure A.3 plots the placebo coefficients, with the actual line representing the baseline coefficients. Reassuring, the true coefficients fall above the 95th percentile of the distribution of placebo coefficients, suggesting that our results are very unlikely to be an artifact of the underlying data.

6 Interpretation

6.1 Translation of Intent-to-Treat Effects into Treatment-on-the-Treated Effects

In our baseline specification, we define our key treatment variable as the probability of having been prenatally exposed to the program during the first trimester. Since not all mothers received iodized oil during a given campaign, our estimates represent intent-to-treat (ITT) impacts. An approach to obtain approximate estimates of the treatment-on-the-treated (TOT) effects would be to adjust the key treatment variable for program participation using the information on program coverage rates in each campaign. Ideally, one would use the fraction of mothers who receive iodized oil during each campaign. In the absence of ideal data, we use overall program coverage rates. Note that these coverage rates are conservative, as women of childbearing age were a major target of these campaigns and thus coverage rate for this population is likely to be higher.

Columns (2), (5) and (8) of Table 6 present the results from estimating model 1, but using the coverage-adjusted treatment indicator as the key independent variable of interest. In columns (1), (4) and (7), we replicate the baseline estimates for ease of comparison. Quantitatively and qualitatively, the results are in line with the baseline, showing that greater exposure to the program is associated with improvements in long-run education and occupational attainment. The point estimates are very similar and statistically indistinguishable from the baseline, likely because coverage rates were remarkably high across campaigns.

A possible limitation of this exercise is that, while much of the variation in the exact timing of iodine supplementation campaigns is likely to be idiosyncratic, coverage rates could actually be endogenous. Distribution efforts could have been more salient in areas with greater iodine deficiency prevalence or in areas experiencing unobserved shocks that are potentially correlated with subsequent cohort outcomes.[31] As an alternative way to scale the estimates, we implement a two-stage least squares (2SLS) approach. Specifically, we use the baseline likelihood of program participation, which assumes that the program reached everyone in each targeted district, as an instrumental variable for the coverage-adjusted likelihood of program participation. As a result, the parameter of interests will be identified entirely by the variation in the timing of campaigns, but the magnitude can be interpreted as the marginal effect of program participation.

The results of this alternative approach are shown in columns (3), (6) and (9) of Table 6. The 2SLS estimates suggest that full exposure to the program would increase educational attainment by 0.25 years and the likelihood of having a skilled occupation by 1.6 percentage points. The estimates also indicate a decline of about 1.4 percent in the probability of engaging in agricultural self-employment. These estimated effects are slightly larger in magnitude and more precisely estimated than the respective OLS estimates. While these estimates differ somewhat in magnitude, they provide bounds of the likely TOT effects of the program on long-run education and labor market outcomes.

6.2 Magnitude

The results presented so far indicate that the program led to improvements in completed education and labor market outcomes. A simple way to place our estimates in perspective is to compare them to the baseline differences in outcomes between districts at the bottom and top of the outcome distribution. Using the 1988 Census, we observe that the difference

[31] Empirically, we find that program coverage rates are significantly higher in districts with a greater share of the population that is under five, which is perhaps unsurprising given that this population was explicitly targeted by the program.

in average years of education between the districts in the 25 and 75th percentiles of the education distribution was about 0.94 years. The 2SLS results in the previous subsection imply that the estimated effect of the program on years of education is approximately equivalent to 26 percent of the baseline schooling difference between low and high education districts.

The analogous calculations for labor market outcomes are similarly striking. We find that the impact of the IOC program on the likelihood of holding a skilled occupation is equal to 36 percent of the baseline difference between districts with low and high skilled employment rates. Likewise, our estimates also imply that full exposure to the program generates an impact that is approximately 10 percent of the difference between districts with high and low rates of agricultural self-employment. These simple calculations suggest that if our estimates are causal, they would represent relatively large effects in magnitude.

6.3 Impact on Occupational Income

To further place our labor market results in perspective, we can construct an income score based on average incomes across occupations in Tanzania and estimate the effects of the program on this outcome. To construct this income score, we use data from the Tanzanian Panel Survey (2008-2016) and implement a two-step procedure that is similar in spirit to Bleakley (2010). We first estimate a regression of log income wages on survey-year dummies to remove year-specific effects. We then collapse the residuals from this regression by occupation, gender and region of residence, yielding about 75 distinct cells on which this index varies. We match this occupation-based earnings score onto our census sample. By construction, this index measures occupational income wages in logs. There is a great deal of variation in this index, with a standard deviation of approximately 0.78 logs. Remarkably, the difference between the 25 and 75 percentiles of the earnings score distribution is approximately 1.1 logs, and the difference between the 10 and 90 percentiles is about 2.2 logs.

Table 7 presents estimates of the effect of the IOC program on the occupational income score. Column (1) presents results from our baseline specification 1. The coefficient of interest is estimated at 0.0146 and highly significant at conventional levels of significance. It implies that full exposure to the iodine program leads to an increase of approximately 1.4 percent in the occupation-based income score. In column (2), we adjust the treatment variable for program coverage rates in each campaign to obtain approximate estimates of the TOT effects. The coefficient of interest is now 0.021 and remains significant at less than 1 percent of significance. Column (3) shows 2SLS estimates where our baseline measure of exposure is used as an instrumental variable for the coverage-adjusted variable of program exposure. This does not materially affect the point estimate, which now stands at 0.0213.

The scaled estimates suggest that full exposure to the program leads to an increase of about 2.1 percent in the occupation-based income wages. For comparison, some of the most successful interventions targeting early childhood in developing countries, such as constructing primary schools (Duflo, 2001), or reducing the prevalence of chronic infections (Bleakley, 2010; Baird et al., 2016) increase adult earnings by 2 to 18 percent. Overall, the results in this subsection suggest that the gains in occupational attainment translate into statistically meaningful differences in potential income wages. Our estimates are likely to represent lower bounds of true impacts because they do not consider the potential gains income within occupations via increased productivity, which may be important.

7 Conclusion

In this paper, we provide new evidence on the long-run repercussions of iodine deficiency. Exploiting variation in the timing of iodine supplementation campaigns across districts of Tanzania, we find strong and robust evidence that iodine deficiency early in life has important consequences for both completed education and occupational status in adulthood. We document that the program led to a decline in agricultural employment and an increase in skilled work, with little changes in overall employment rates. This suggests that the program affected individuals' occupational status in adulthood by shifting employment shares from lower-paid occupations towards jobs that typically demand more skilled labor and have higher economic returns.

The results of our paper contribute to the existing literature investigating the importance of early life circumstances in explaining long-run economic outcomes, in particular those focusing on the effects of early nutritional deficits. We provide new evidence on the relevance of the micronutrient iodine for long-term development in the context of the pervasive prevalence of iodine deficiency. Our results therefore may be particularly relevant for many countries in the developing world which still struggle to reduce the prevalence of iodine deficiency.

Finally, from a broader perspective, our results relate to the debate about the degree to which nutrition-based poverty traps may hamper individuals in the least developed regions of the world from escaping poverty. The findings of this study suggest that underconsumption of iodine is an additional barrier to economic growth in developing countries, as it deters many individuals from acquiring human capital.

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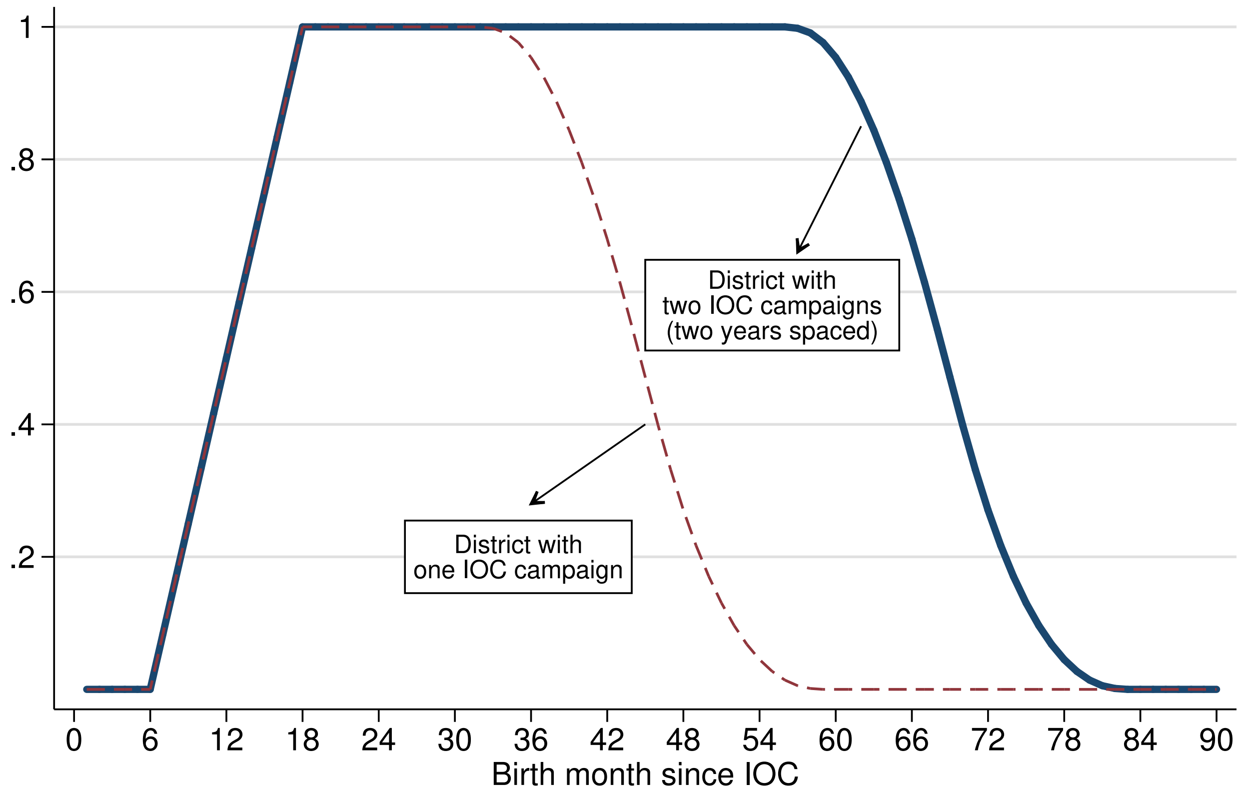
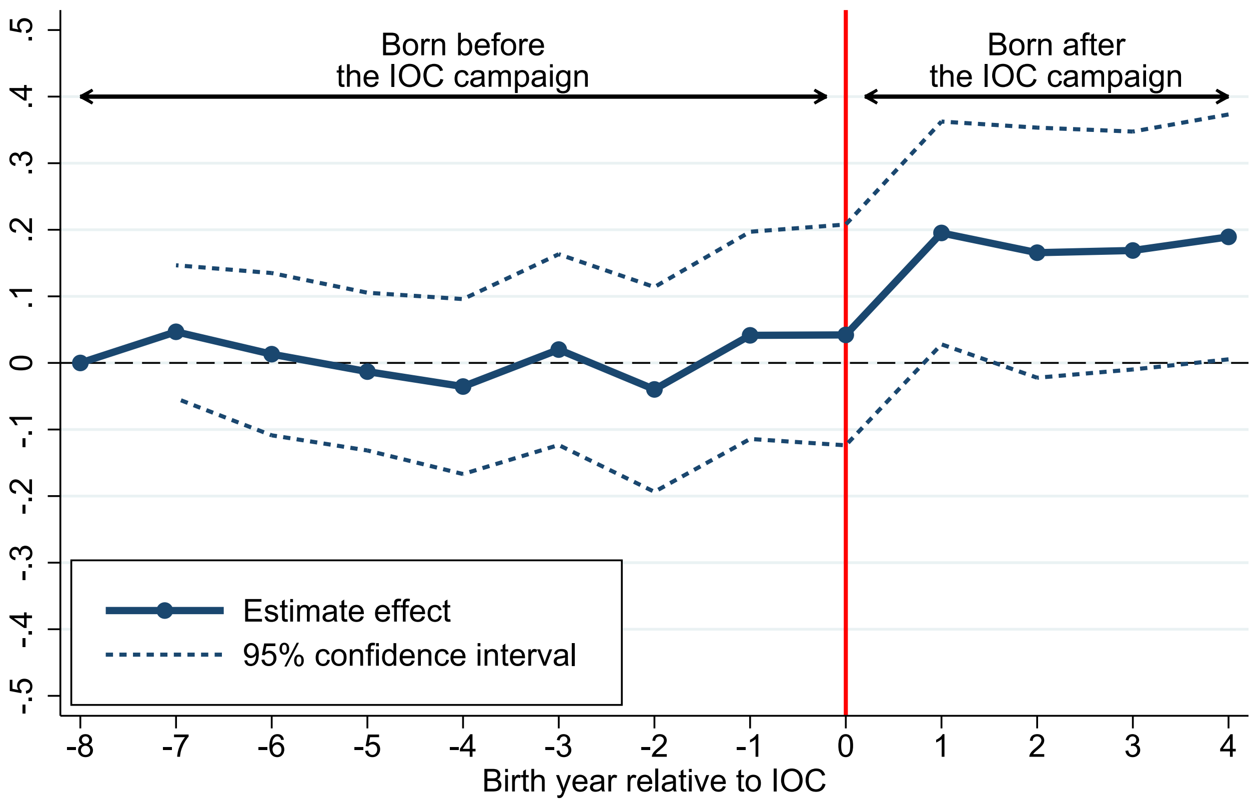
Figure 1: Likelihood of Program Participation



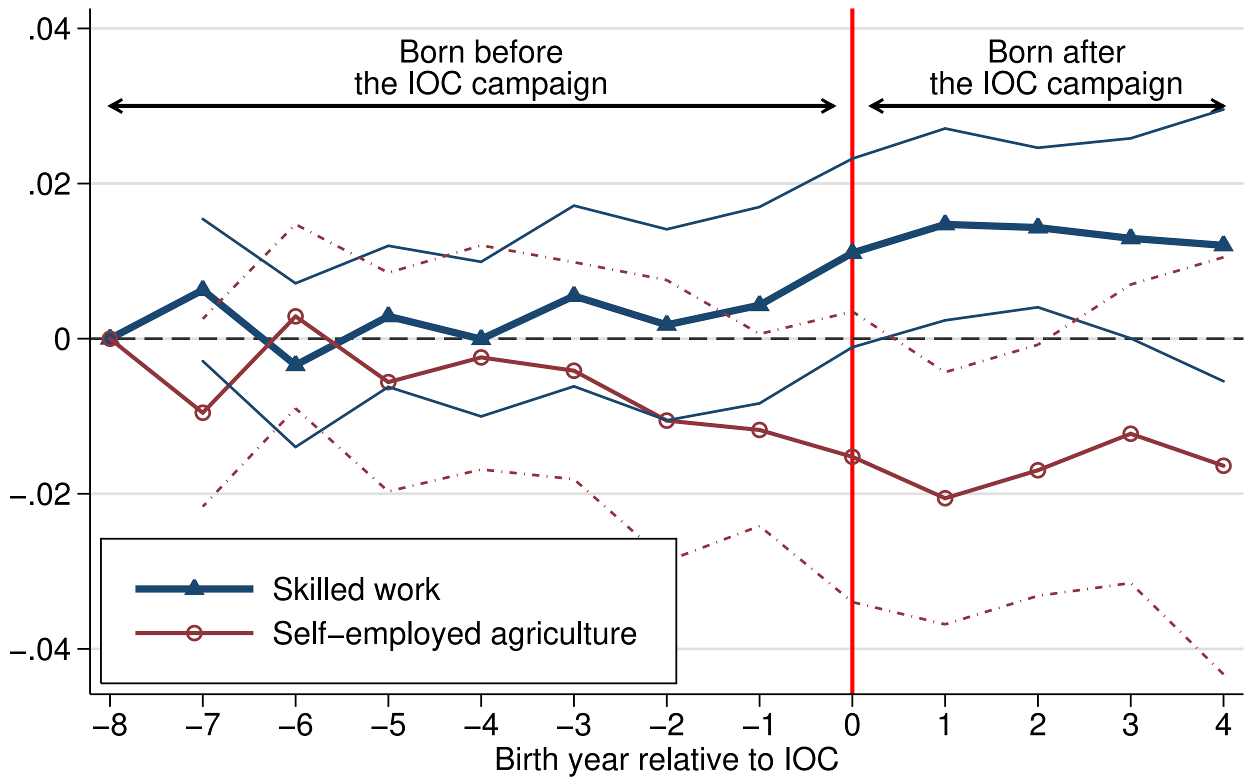
Notes. This figure shows the probability that an individual born in a given month was exposed to the program during the first trimester of pregnancy.

Figure 2: Estimates of the Effects of IOC on Education



Notes. This figure plots estimates by age using the specification 2. Standard errors are clustered at the birth-district level. Dashed lines show 95-percent confidence intervals for each estimate. The model includes fixed effects for birth-district, birth-year, and gender, as well as baseline district characteristics (those displayed in Table A.4) interacted with a linear trend in year of birth. The model also includes birth-region linear cohort trends. Data includes individuals born in Tanzania between 1977 and 1994 who are observed in the 2012 census. Regressions estimated on data collapsed to cells defined by birth-district × birth-year × gender and regressions are weighted by cell sizes.

Figure 3: Estimates of the Effects of IOC on Employment in Skilled and Agricultural Activities



Notes. This figure plots estimates by age using the specification 2. See Figure 2 for details on the baseline specification. Skilled occupations correspond to: legislators, senior officials, managers, professionals, clerks, sales, services, and skilled manual.

Table 1: Estimated Effects of IOC on Long-Run Education

	Dependent variable is total years of education			
	(1)	(2)	(3)	(4)
IOC Exposure	0.1230	0.1629	0.1744	0.1561
	[0.0622]*	[0.0603]***	[0.0570]***	[0.0582]***
FE district, birth year, gender	Yes	Yes	Yes	Yes
1988 district char. × linear cohort		Yes	Yes	Yes
Region × linear cohort			Yes	
Region × birth year FE				Yes
Mean dep. variable	6.93	6.93	6.93	6.93
Number of observations	1,228,563	1,228,563	1,228,563	1,228,563
Number of cells	4,068	4,068	4,068	4,068

Notes. Models presented are weighted least-squares estimates of equation 1. The unit of analysis is at the birth-district × birth-year × gender level. Weights are given by cell size. The key independent variable, IOC Exposure, measures the probability of having been exposed to an iodine campaign during the first trimester of pregnancy. All columns include fixed effects for birth-district, birth-year, and gender. Columns (2)-(4) include 1988 district characteristics interacted with a linear trend in year of birth. These district characteristics include all those in Table A.4. Columns (3) include birth-region linear time trends. Column (4) include birth-region × birth-year fixed effects. Standard errors in brackets are clustered by the district of birth.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 2: Estimated Effects of IOC on Educational Levels

	Probability of completing exactly:				Completed years of:	
	No education or preschool	Primary school	Secondary school	Tertiary Education	Primary school	Secondary school
	(1)	(2)	(3)	(4)	(5)	(6)
IOC Exposure	-0.0056	-0.0214	0.0248	0.0022	0.0706	0.3218
	[0.0055]	[0.0124]*	[0.0113]**	[0.0029]	[0.0383]*	[0.1185]***
Mean dep. variable	0.15	0.59	0.21	0.03	5.88	3.04
Number of observations	1,228,563	1,228,563	1,228,563	1,228,563	1,228,563	1,228,563
Number of cells	4,068	4,068	4,068	4,068	4,068	4,068

Notes. Models presented are weighted least-squares estimates of equation 1. The unit of analysis is at the birth-district × birth-year × gender level. Weights are given by cell size. The key independent variable, IOC Exposure, measures the probability of having been exposed to an iodine campaign during the first trimester of pregnancy. Dependent variables in columns (1)-(4) are indicators for completing exactly different levels of education. All regressions include fixed effects for birth-district, birth-year, and gender. Additional controls include baseline district characteristics interacted with a linear trend in year of birth. District characteristics include all those shown in Table A.4. The regressions also control for birth-region linear time trends. Standard errors in brackets are clustered by district of birth.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 3: Estimated Effects of IOC on Employment and Occupations

		Occupational skill scores		Probability of holding a job in:			
	Any employment last year	Mean years of education in main occupation	Fraction of high education workers in main occupation	a skilled occupation	self-employement in agriculture	an elementary occupation	a craft or related-trade occupation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
IOC Exposure	-0.0125	0.0379	0.0039	0.0109	-0.0099	-0.0035	0.0025
	[0.0111]	[0.0159]**	[0.0016]**	[0.0041]***	[0.0049]**	[0.0023]	[0.0019]
Mean dep. variable	0.73	5.59	0.10	0.21	0.60	0.11	0.06
Number of observations	1,246,242	843,915	843,915	845,064	845,064	845,064	845,064
Number of cells	4,068	4,068	4,068	4,068	4,068	4,068	4,068

Notes. The unit of analysis is at the birth-district $\times$ birth-year $\times$ gender level. Weights are given by cell sizes. The dependent variable in column (2) is defined as the average years of schooling of the individuals in the same occupation, status in employment, and gender cell. The dependent variable in column (3) is the fraction of workers in each category of occupation, status in employment, and gender cell that has completed at least one year of secondary school. The occupational skill scores are created using information from the 2002 Tanzanian census. Skilled occupations in column (4) correspond to: legislators, senior officials, managers, professionals, clerks, sales, services, and skilled manual. All the regressions include the baseline controls: fixed effects for birth-district, birth-year, and gender; baseline district characteristics interacted with a linear trend in year of birth; and birth-region linear time trends. Standard errors in brackets are clustered by district of birth.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 4: Estimated Effects of IOC on Education and Income by Gender

					Occupational skill scores			
	Total years of education		Any employment last year		Mean years of education in main occupation		Fraction of high education workers in main occupation	
	Males	Females	Males	Females	Males	Females	Males	Females
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IOC Exposure	0.1355	0.2076	-0.0059	-0.0181	0.0458	0.0280	0.0046	0.0030
	[0.0540]**	[0.0665]***	[0.0120]	[0.0116]	[0.0172]***	[0.0196]	[0.0019]**	[0.0018]
Mean dep. variable	7.33	6.58	0.77	0.68	6.11	5.07	0.12	0.09
Number of observations	558,247	670,316	566,648	679,594	412,242	431,673	412,242	431,673
Number of cells	2,034	2,034	2,034	2,034	2,034	2,034	2,034	2,034
p-value of test H_0: $\beta_{male} = \beta_{female}$		0.1065		0.1164		0.3504		0.3765

	Probability of holding a job in:							
	a skilled occupation		self-employement in agriculture		an elementary occupation		a craft or related-trade occupation	
	Males	Females	Males	Females	Males	Females	Males	Females
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
IOC Exposure	0.0133	0.0076	-0.0149	-0.0039	-0.0017	-0.0052	0.0033	0.0016
	[0.0055]**	[0.0043]*	[0.0059]**	[0.0063]	[0.0037]	[0.0033]	[0.0033]	[0.0024]
Mean dep. variable	0.23	0.20	0.56	0.64	0.11	0.11	0.09	0.03
Number of observations	412,916	432,148	412,916	432,148	412,916	432,148	412,916	432,148
Number of cells	2,034	2,034	2,034	2,034	2,034	2,034	2,034	2,034
p-value of test H_0: $\beta_{male} = \beta_{female}$		0.2960		0.1182		0.5021		0.6813

Notes. The unit of analysis is at the birth-district × birth-year level. Weights are given by cell sizes. The key independent variable, IOC Exposure, measures the probability of having been exposed to an iodine campaign during the first trimester of pregnancy. All regressions include fixed effects for birth-district and birth-year. Additional controls include baseline district characteristics interacted with a linear trend in year of birth. District characteristics include all those shown in Table A.4. The regressions also control for birth-region linear time trends. Standard errors in brackets are clustered by district of birth.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 5: Estimated Effects of IOC on Disability in Later Life

	Any disability	Number of disabilities	Components of disability: Mental	Hearing	Physical	Sight
	(1)	(2)	(3)	(4)	(5)	(6)
IOC Exposure	-0.0003	-0.0006	-0.0001	0.0004	-0.0002	-0.0007
	[0.0008]	[0.0010]	[0.0005]	[0.0004]	[0.0005]	[0.0006]
Mean dep. variable	0.02	0.02	0.006	0.006	0.005	0.009
Number of observations	1,246,242	1,246,242	1,246,242	1,246,242	1,246,242	1,246,242
Number of cells	4,068	4,068	4,068	4,068	4,068	4,068

Notes. The unit of analysis is at the birth-district × birth-year × gender level. Weights are given by cell sizes. The dependent variable in columns (1) and (3)-(6) are dummy indicators for any disability and disability types respectively. All the regressions include the baseline controls: fixed effects for birth-district, birth-year, and gender; baseline district characteristics interacted with a linear trend in year of birth; and birth-region linear time trends. Standard errors in brackets are clustered by district of birth.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 6: Estimated Effects of IOC on Education, Skilled Occupation, and Agricultural self-employment
(Treatment Adjusted for IOC Coverage Rates)

	Total years of education			Skilled occupation			Agricultural self-employment		
	Baseline	OLS	2SLS	Baseline	OLS	2SLS	Baseline	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
IOC Exposure	0.1744			0.0109			-0.0099		
	[0.0570]***			[0.0041]***			[0.0049]**		
IOC Exposure × coverage rates		0.2244	0.2593		0.0139	0.0160		-0.0122	-0.0145
		[0.0868]**	[0.0872]***		[0.0061]**	[0.0059]***		[0.0067]*	[0.0071]**
Mean dep. variable	6.93	6.93	6.93	0.21	0.21	0.21	0.60	0.60	0.60
Number of observations	1,228,563	1,228,563	1,228,563	845,064	845,064	845,064	845,064	845,064	845,064
Number of cells	4,068	4,068	4,068	4,068	4,068	4,068	4,068	4,068	4,068

Notes. The unit of analysis is at the birth-district × birth-year × gender level. Weights are given by cell sizes. Columns (1), (4), and (7) replicates the baseline specification. Columns (2), (5), and (8) adjust the key treatment variable for coverage rates of each campaign. Columns (3), (6), and (9) use IOC Exposure as an instrumental variable for IOC Exposure × coverage rates. All the regressions include the same controls as the baseline specification. Standard errors in brackets are clustered by district of birth.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 7: Estimated Effects of IOC on Occupational Income

	Baseline	OLS	2SLS
	(1)	(2)	(3)
IOC Exposure	0.0146		
	[0.0055]***		
IOC Exposure × coverage rates		0.021	0.0213
		[0.0078]***	[0.0079]***
Number of observations	843,309	843,309	843,309
Number of cells	4068	4068	4068

Notes. The dependent variable is a occupation-based earnings score, measured in logs. See Section 6.3 for details on the construction of this index. The unit of analysis is at the birth-district × birth-year × gender level. Weights are given by cell sizes. Columns (1) reports results from estimating equation 1. Column (2) adjusts the key treatment variable for coverage rates of each campaign. Column (3) uses IOC Exposure as an instrumental variable for IOC Exposure × coverage rates. All the regressions include the same controls as the baseline specification. Standard errors in brackets are clustered by district of birth.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

For Online Publication

Online Appendix to "The Long-Run Economic Consequences of Iodine Supplementation"

Daniel Araújo, Bladimir Carrillo, and Breno Sampaio

March 19, 2021

A Appendix

A.1 Census district data 1988

We use the 1988 Tanzanian census to construct a set of baseline district characteristics. We obtained a 10 percent sample from the Integrated Public-Use Microdata Series (IPUMS).

These variables include counts of the population; share of the population that is married; share of the population over 15 with four years of education or less; share of the population over 15 with nine years of education or more; share of population under five years of age; share of the population over 25 years of age; share of households that own their housing unit; share of households with access to electricity; share of the labor force in agriculture; and share of the labor force that is male.

A.2 Geographical Variables

In our baseline specification, we include interactions between linear cohort trends and district elevation and also with distance to the ocean. We calculate distances to the nearest coastline for each district using a high-resolution GIS map of Africa. We draw information on elevation from Shuttle Radar Topography Mission (SRTM) Global Digital Elevation Model (Reuter et al., 2007). This dataset provides worldwide estimates for elevation (measures in meters on the sea level) spaced at 30 arc-seconds (approximately 0.8 kilometers). We use geospatial software to construct a measure of average elevation for each district.

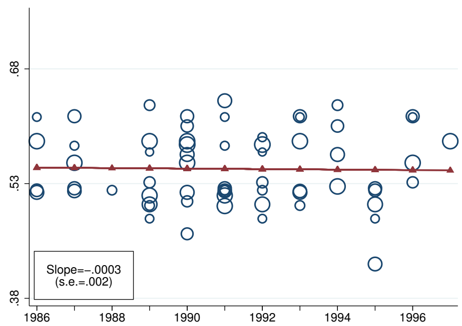
A.3 District Definitions

Using district identifiers, we merge the census data with the key policy variable. A difficulty with this process is that district boundaries have substantially changed over time. Indeed, some districts were split into two or more districts during the beginning of the 2000s, increasing the number of districts by about 40 percent.[1] Fortunately, it is possible to match current districts to older ones without any ambiguity. We match each modern district to older districts using a high-resolution GIS map of Tanzania in 1988, the most recent census available before the changes in district boundaries. This yields 113 districts.

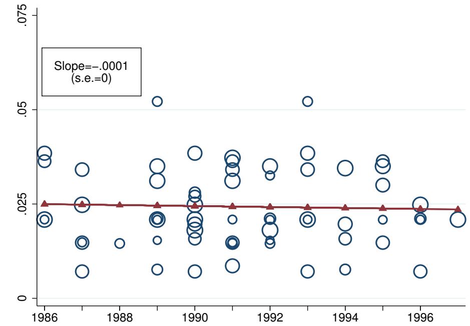
[1] While the cohorts we analyze were born before these changes, census enumerators coded information on place of birth using current district boundaries at census time.

List of Figures

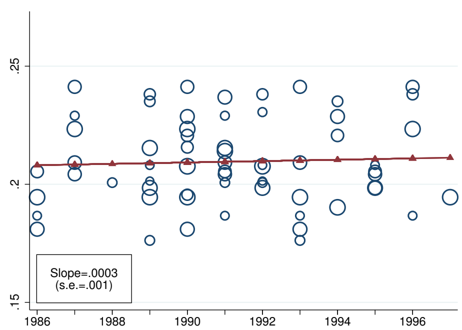
Figure A.1: Balance Tests: Relationship between Timing of IOC and Baseline District Characteristics



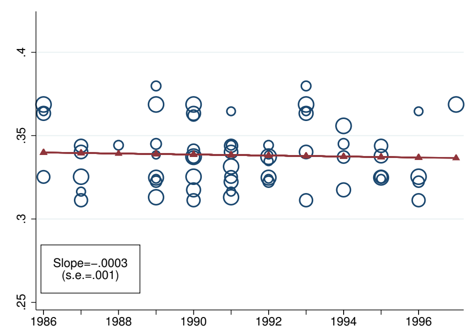
(a) Share 4 years of education or less



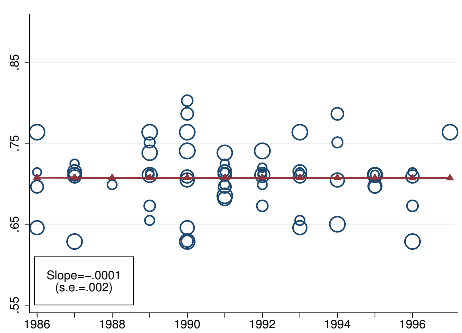
(b) Share 9 years of education or more



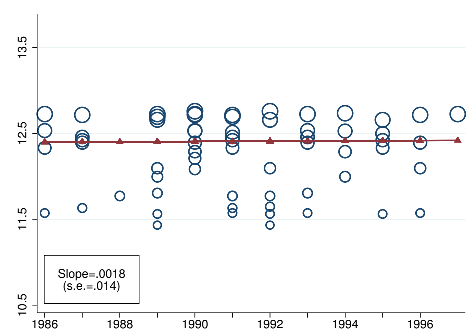
(c) Share under 5 years of age



(d) Share over 25 years of age

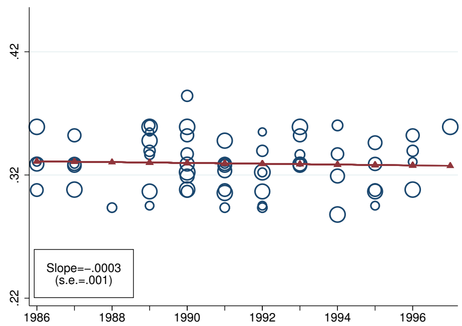


(e) Share employed

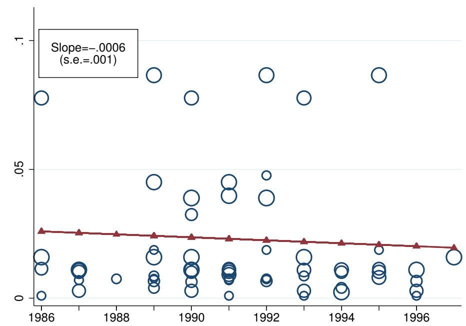


(f) Log Population

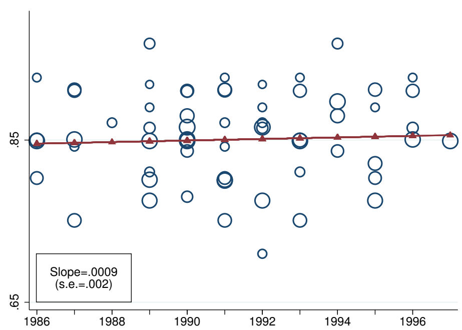
Figure A.1: Balance Tests: Relationship between Timing of IOC Campaigns and Baseline District Characteristics. —Continued



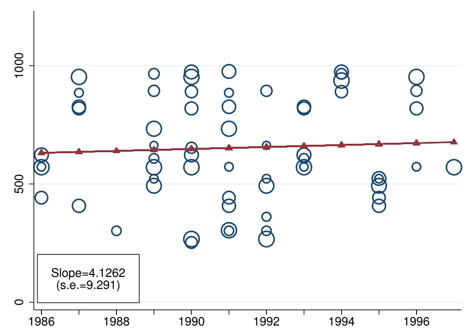
(g) Share married



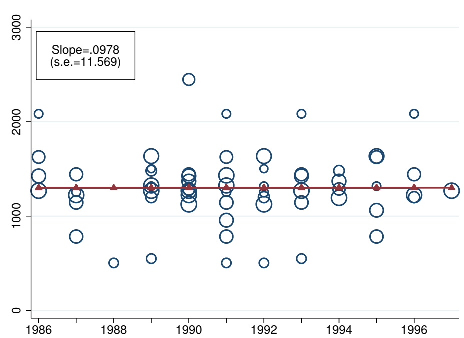
(h) Share electricity



(i) Share homeownership



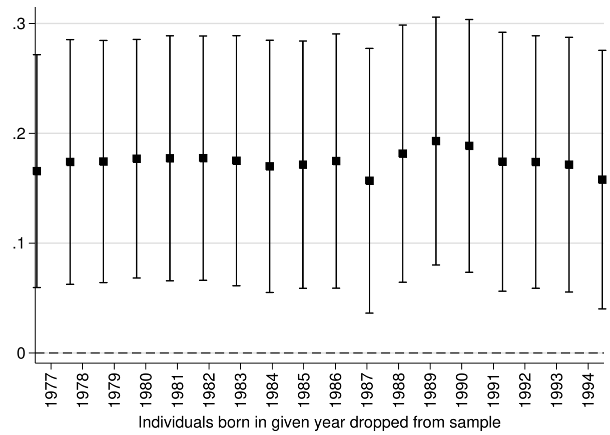
(j) Distance to the Ocean (in km^2)



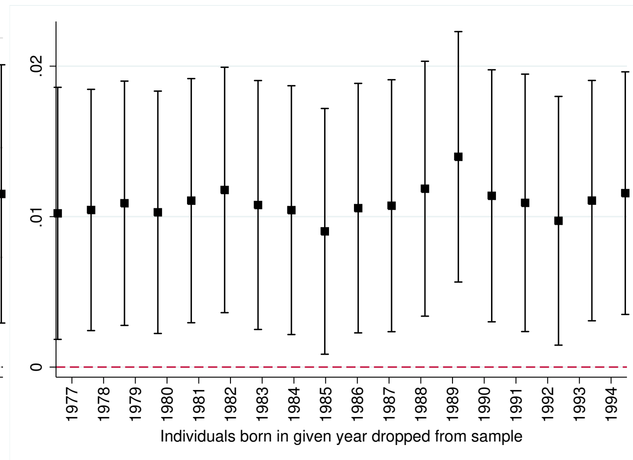
(k) Elevation

Notes. Univariate fitted values are from regressions of the dependent variable on the year of the IOC campaign. The unit of analysis is the district-by-IOC campaign. This exercises pools all program years for each district into the same sample. If a district had two campaigns in different years, then this district is included twice in the sample, that is, an observation for each campaign. The regressions are weighted by the 1988 district population. Standard errors are heteroskedasticity-robust.

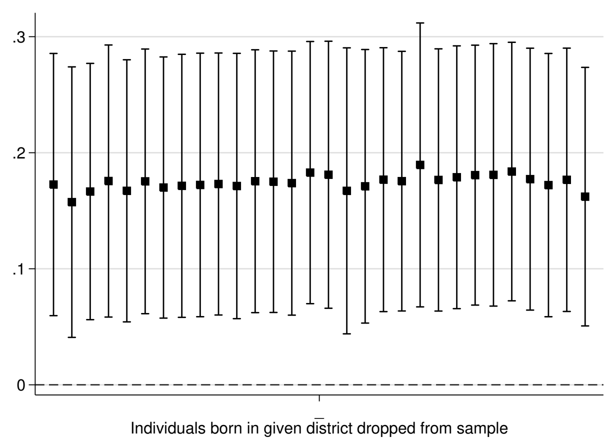
Figure A.2: Robustness to Sample Restrictions



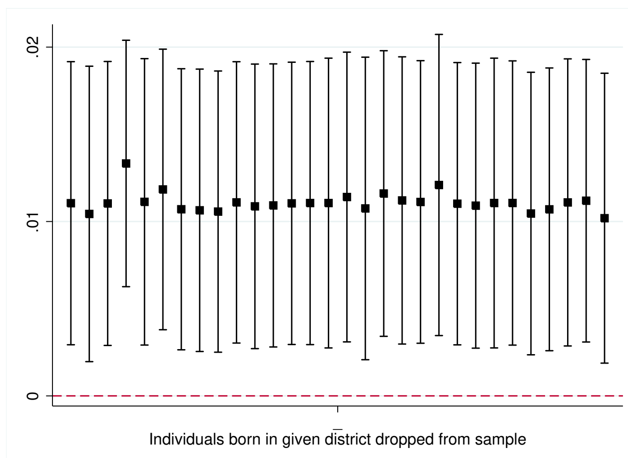
(a) Dropping birth cohorts
Total years of education



(b) Dropping birth cohorts
Skilled occupation



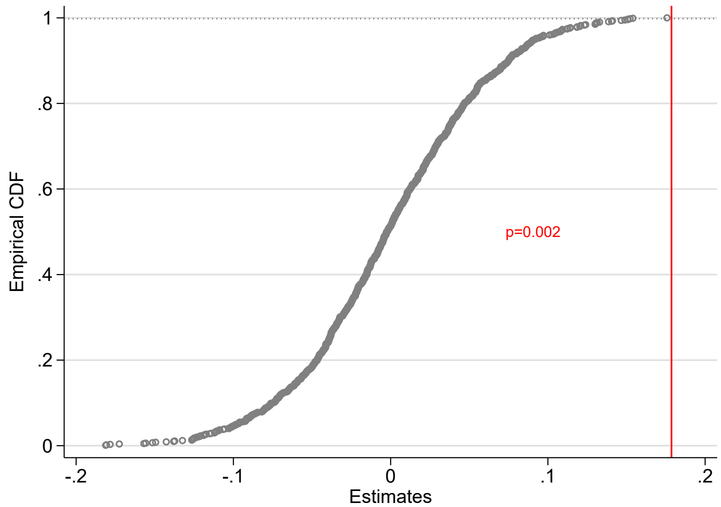
(c) Droping 30 largest districts
Total years of education



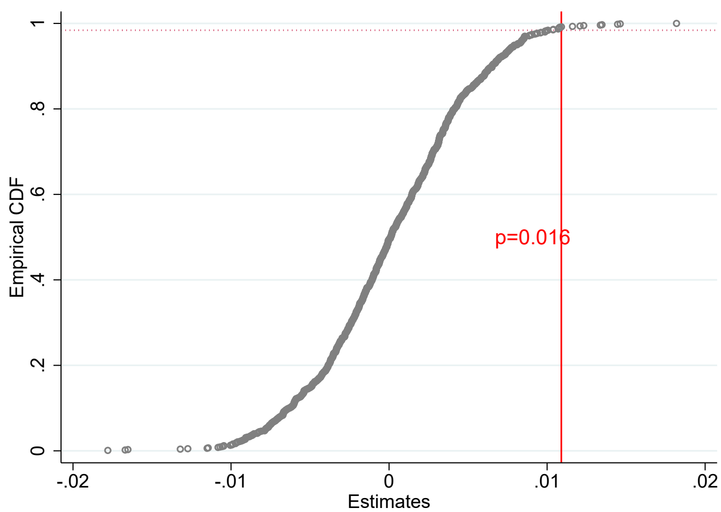
(d) Dropping 30 largest districts
Skilled occupation

Notes. This figure plots estimates based on the specification 1. See Figure 2 for details on the controls. Point estimates for the effect of in utero exposure, denoted by square markers, and corresponding 95-percent confidence intervals represented by lines, as different subsets of observations are excluded from the sample.

Figure A.3: Permutation Tests



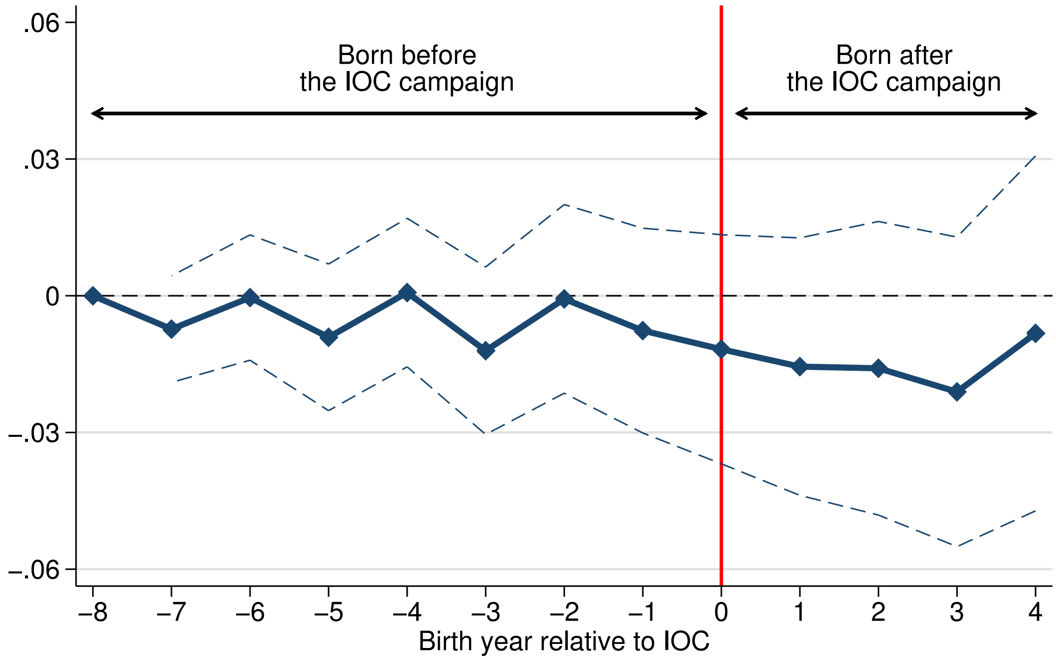
(a) Total years of education



(b) Skilled occupation

Notes. These figures plot the empirical CDF of placebo coefficients. We randomly assign each of the birth cohorts across districts a campaign year, and estimate model 1 using the implied placebo treatment probability. This procedure is repeated 1000 times. The share of the 1000 absolute placebo coefficients that are larger than the absolute actual coefficient is the p-value for the hypothesis that $\pi = 0$. The vertical lines represent the true estimates.

Figure A.4: Estimates of the Effects of IOC on Probability of Employment



Notes. This figure plots event-study estimates using the specification 2. See Figure 2 for details on the baseline specification. The dependent variable is an indicator of the probability of working in the previous 12 months.

List of Tables

Table A.1: Data on IOC campaigns for each district

		Years of Intervention					Coverage				
Region	District	Year 1	Year 2	Year 3	Year 4	Year 5	Coverage 1	Coverage 2	Coverage 3	Coverage 4	Coverage 5
Dodoma	Mpwapwa	1990	1992				0.65	0.58			
Arusha	Monduli	1992					0.71				
Arusha	Arumeru	1991					0.89				
Kilimanjaro	Rombo	1990					0.68				
Morogoro	Ulanga	1988	1991	1992			0.73	0.61	0.34		
Ruvuma	Songea Rural	1987	1991	1995			0.91	0.74	0.85		
Ruvuma	Mbinga	1995					0.92				
Iringa	Mufindi	1986	1991	1995			0.41	0.63	0.54		
Iringa	Makete	1986	1991	1993	1996		0.2	0.62	0.62	0.49	
Iringa	Njombe	1989	1992	1995			0.76	0.68	0.64		
Iringa	Ludewa	1989	1992	1995			0.59	0.62	0.47		
Mbeya	Chunya	1990					0.49				
Mbeya	Mbeya Rural	1986	1989	1990	1993	1997	0.44	0.84	0.9	0.53	0.53
Mbeya	Kyela	1989	1993				0.91	0.57			
Mbeya	Rungwe	1986	1990	1993			0.35	0.73	0.49		
Mbeya	Ileje	1989	1992				0.94	0.71			
Mbeya	Mbozi	1989	1991				0.67	0.63			
Rukwa	Mpanda	1987	1991	1993			0.79	0.6	0.72		
Rukwa	Sumbawanga	1987	1990	1993	1996		0.76	0.89	0.72	0.51	
Rukwa	Nkansi	1987	1991				0.89	0.49			
Kigoma	Kibondo	1989	1992	1996			0.73	0.75			
Kigoma	Kasulu	1987	1990	1996			0.5	0.66	0.49		
Kigoma	Kigoma Rural	1991					0.91				
Kagera	Karagwe	1990	1994				0.96	0.85			
Kagera	Bukoba Rural	1994					0.78				
Kagera	Biharamulo	1990	1994				0.96	0.38			
Kagera	Ngara	1989	1994				0.29	0.51			

Notes. These data are from Field et al. (2009).

Table A.2: Probability of protection from in utero IDD relative to program year t by month of birth, 380mg IOC

	Jan	**Feb**	**Mar**	**Apr**	**May**	**Jun**	**Jul**	**Aug**	**Sept**	**Oct**	**Nov**	**Dec**	**Birth year average**
Program Year	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.028	0.083	0.167	0.250	0.333	0.072
1st Year After Program	0.417	0.500	0.583	0.667	0.750	0.833	0.917	1.000	1.000	1.000	1.000	1.000	0.806
2st Year After Program	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	0.998	0.991	0.977	0.997
3st Year After Program	0.955	0.927	0.891	0.849	0.802	0.749	0.690	0.627	0.559	0.488	0.419	0.353	0.668
4st Year After Program	0.292	0.237	0.189	0.148	0.112	0.082	0.057	0.037	0.022	0.011	0.004	0.001	0.099

Notes These data are from Field et al. (2009).

Table A.3: Summary statistics

	All		Men		Women	
	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
	(1)	(2)	(3)	(4)	(5)	(6)
Total years of education	6.931	1.575	7.332	1.417	6.589	1.622
No education/preschool	0.154	0.115	0.123	0.088	0.18	0.127
Primary school	0.592	0.132	0.583	0.136	0.599	0.129
Secondary school	0.218	0.164	0.249	0.166	0.191	0.157
Some tertiary education	0.037	0.04	0.045	0.045	0.03	0.033
Years spent in primary school	5.886	0.98	6.109	0.813	5.696	1.067
Years spent in secondary school	3.046	2.038	3.528	2.023	2.635	1.96
Any employment last year	0.73	0.18	0.779	0.184	0.689	0.166
Mean years of education in main occupation	5.595	0.749	6.118	0.49	5.072	0.58
Fraction high education workers in main occupation	0.107	0.051	0.122	0.048	0.091	0.05
Skilled occupation	0.216	0.122	0.232	0.121	0.201	0.122
Self-employment in agriculture	0.604	0.192	0.562	0.191	0.647	0.184
Elementary occupation	0.114	0.054	0.114	0.048	0.115	0.06
Craft or related-trade occupation	0.065	0.049	0.092	0.052	0.038	0.028
Any disability	0.024	0.012	0.023	0.011	0.024	0.012
Number of disabilities	0.027	0.014	0.027	0.013	0.028	0.015
Mental disability	0.006	0.005	0.006	0.005	0.006	0.005
Hearing disability	0.006	0.005	0.006	0.005	0.005	0.005
Physical disability	0.007	0.006	0.007	0.006	0.007	0.006
Sight disability	0.009	0.007	0.008	0.007	0.01	0.007
Number of observations	1,246,242		566,648		679,594	
Number of cells	4,068		2,034		2,034	

Notes. The table provides summary statistics of each outcome variable reported in the paper. These data are from the 2012 Tanzanian census.

Table A.4: Balance Tests: Relationship between Timing of IOC and Baseline District Characteristics

	Mean	Univariate OLS		
		Coefficient	*Standard Error*	R^2
	(1)	(2)	(3)	(4)
Log (population), 1988	12.25	0.0018	[0.0137]	0.000
Share of population that is married, 1988	0.33	-0.0003	[0.0011]	0.002
Share of population with 4 years of schooling or less, 1988	0.55	-0.0003	[0.0021]	0.000
Share of population with 9 years of schooling or more, 1988	0.02	-0.0001	[0.0004]	0.001
Share of population over 25 years of age, 1988	0.34	-0.0003	[0.0010]	0.002
Share of population under 5 years of age, 1988	0.21	0.0003	[0.0008]	0.002
Share of population that is employed, 1988	0.71	-0.0001	[0.0023]	0.000
Share of population that own their housing unit, 1988	0.85	0.0009	[0.0021]	0.003
Share of population with access to electricity, 1988	0.02	-0.0006	[0.0012]	0.004
Share of labor force in agriculture, 1988	0.89	0.0005	[0.0019]	0.001
Share of labor force that is male, 1988	0.46	-0.0005	[0.0010]	0.003
District altitude (in meters above sea level)	1300.78	0.0978	[11.5693]	0.000
Distance to the Ocean (in square kilometers)	652.89	4.1262	[9.2906]	0.003

Notes. Each row reports the results from a regression of the variable in the row heading on the year of the IOC campaign and a constant term. This exercises pools all program years for each district into the same sample. If a district had two campaigns in different years, then this district is included twice in the sample, that is, an observation for each campaign. Column (3) displays heteroskedasticity-robust standard errors. The regressions are weighted by the 1988 district population. *N=63*.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table A.5: Testing for Compositional Changes

	Child characteristics			Maternal characteristics					
	Birth order	Twin or multiple birth	Male	Age	Years of education	Secondary school	Literacy	Number of children	Residing in urban area
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
IOC Exposure	-0.2152	0.0044	-0.0059	-0.3674	-0.2714	0.0014	-0.0117	-0.229	0.008
	[0.2227]	[0.0165]	[0.0665]	[0.6699]	[0.2568]	[0.0130]	[0.0448]	[0.1967]	[0.0113]
Mean dep. variable	4.14	0.03	0.50	29.14	4.93	0.11	0.62	4.80	0.25
Number of observations	3,046	3,046	3,046	3,046	3,046	3,046	3,038	3,046	3,046

Notes. Estimates based on the 1999 Tanzanian Demographic Health Survey. The unit of analysis is at the child level. Treatment exposure calculated using month and year of birth, and years in which the program was rolled out in each district. Controls include fixed effects for the district and child's age. Standard errors in brackets are clustered by the district.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table A.6: Estimated Effects of IOC on Cohort Sizes and Sex Ratios

	Dependent variable is:	
	Log(Cohort size)	Male to female cohort ratio
	(1)	(2)
IOC Exposure	-0.0068	-0.0012
	[0.0150]	[0.0145]
Mean dep. variable	8.93	0.85
Number of observations	2,034	2,034

Notes. The unit of analysis is at the birth-district × birth-year level. Weights are given by cell sizes. This table presents estimates of the effect of IOC campaigns on cohort size (column 1) and sex ratio (column 2). All the regressions include the baseline controls: fixed effects for birth-district, and birth-year; baseline district characteristics interacted with a linear trend in year of birth; and birth-region linear time trends. Standard errors in brackets are clustered by the district of birth.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table A.7: Robustness to Alternative Exposure Definitions

	Baseline	Accounting for birth seasonality	Assuming 8 pregnancy months	Assuming 8 pregnancy months; Accounting for birth seasonality
	(1)	(2)	(3)	(4)
Panel A: Total years of education				
IOC Exposure	0.1744	0.1781	0.1779	0.1775
	[0.0570]***	[0.0597]***	[0.0593]***	[0.0591]***
Mean dep. Variable	6.93	6.93	6.93	6.93
Number of observations	1,228,563	1,228,563	1,228,563	1,228,563
Number of cells	4,068	4,068	4,068	4,068
Panel B: Skilled occupation				
IOC Exposure	0.0109	0.0115	0.0117	0.0116
	[0.0041]***	[0.0042]***	[0.0041]***	[0.0041]***
Mean dep. Variable	0.21	0.21	0.21	0.21
Number of observations	843,915	843,915	843,915	843,915
Number of cells	4,068	4,068	4,068	4,068

Notes. The unit of analysis is at the birth-district × birth-year level. Weights are given by cell sizes. This table presents results from alternative measures of program exposure. Column (2) accounts for the seasonality of birth when calculating birth-year-specific probabilities of program exposure. Column (3) repeats the baseline but assumes that the length of all pregnancies was 8 months instead of 9. Column (4) repeats column (3) but accounts for the seasonality of birth. Data on the seasonality of birth is obtained from the Tanzanian Demographic Health Surveys. All the regressions include the baseline controls: fixed effects for birth-district, and birth-year; baseline district characteristics interacted with a linear trend in year of birth; and birth-region linear time trends. Standard errors in brackets are clustered by the district of birth.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table A.8: Robustness to Burundi/Rwanda Migration Shock

		Additional controls:		Exclude districts within X kilometers from Rwanda/Burundi border			
	Baseline	Border distance × linear cohort trend	Inverse Border distance × linear cohort trend	≤ 50	≤ 100	≤ 150	≤ 200
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Total years of education							
IOC Exposure	0.1744	0.19	0.1817	0.1956	0.2041	0.1949	0.2232
	[0.0570]***	[0.0548]***	[0.0582]***	[0.0661]***	[0.0723]***	[0.0700]***	[0.0681]***
Mean dep. variable	6.93	6.93	6.93	6.95	6.97	7.03	7.07
Number of observations	1,228,563	1,228,563	1,228,563	1,182,955	1,140,749	1,093,432	1,060,281
Number of cells	4,068	4,068	4,068	3,924	3,780	3,672	3,600
Panel B: Skilled occupation							
IOC Exposure	0.0109	0.0098	0.0109	0.0129	0.0117	0.0129	0.0128
	[0.0041]***	[0.0047]**	[0.0041]***	[0.0046]***	[0.0048]**	[0.0051]**	[0.0052]**
Mean dep. variable	0.21	0.21	0.21	0.21	0.22	0.22	0.22
Number of observations	845,064	845,064	845,064	809,733	777,681	745,69	723,150
Number of cells	4,068	4,068	4,068	3924	3780	3672	3600

Notes. The unit of analysis is at the birth-district × birth-year × gender level. Weights are given by cell sizes. Column (1) replicates the baseline specification. Column (2) include as control the interaction between a linear cohort trend and district distance to the border with Rwanda/Burundi. Column (3) rather includes as control the interaction between a linear cohort trend and district inverse distance to the border with Rwanda/Burundi. Columns (4)-(7) repeats the baseline specification but excludes districts within the cutoff distances from the Rwanda/Burundi border. Standard errors in brackets are clustered by the district of birth.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table A.9: Robustness to Alternative Clustering Strategies

			Conley standard errors cuttoff distances in kilometers:				
	Baseline	Twoway clustering	100	200	300	600	800
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Total years of education							
IOC Exposure	0.1744	0.1744	0.1744	0.1744	0.1744	0.1744	0.1744
	[0.0570]***	[0.0581]***	[0.0192]***	[0.0228]***	[0.0228]***	[0.0198]***	[0.0228]***
Mean dep. Variable	6.93	6.93	6.93	6.93	6.93	6.93	6.93
Number of observations	1,228,563	1,228,563	1,228,563	1,228,563	1,228,563	1,228,563	1,228,563
Number of cells	4,068	4,068	4,068	4,068	4,068	4,068	4,068
Panel B: Skilled occupation							
IOC Exposure	0.0109	0.0109	0.0109	0.0109	0.0109	0.0109	0.0109
	[0.0041]***	[0.0046]**	[0.0040]***	[0.0042]***	[0.0044]**	[0.0041]***	[0.0044]**
Mean dep. Variable	0.21	0.21	0.21	0.21	0.21	0.21	0.21
Number of observations	843,915	843,915	843,915	843,915	843,915	843,915	843,915
Number of cells	4,068	4,068	4,068	4,068	4,068	4,068	4,068

Notes. The unit of analysis is at the birth-district × birth-year × gender level. Weights are given by cell sizes. Column (1) replicates the baseline results reported in column (3) of Table 1 and in column (4) of Table 3. Column (2) uses two-way clustering method developed by Cameron et al. (2011). Columns (3)-(7) reports results using Conley (1999) standard errors to account for possible spatial correlation with cutoffs varying from 100 to 800 kilometers. Robust standard errors in brackets clustered at the district level unless otherwise indicated.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table A.10: Event-study Estimates of the Effects of IOC on Education and Employment in Skilled Occupations

	Total Years of Education	Skilled Occupation
Birth year relative to IOC		
-7	0.0463	0.0062
	[0.0505]	[0.0046]
-6	0.0116	-0.0035
	[0.0615]	[0.0053]
-5	-0.0139	0.0028
	[0.0598]	[0.0046]
-4	-0.0378	-0.0001
	[0.0661]	[0.0050]
-3	0.0184	0.0055
	[0.0721]	[0.0059]
-2	-0.0422	0.0017
	[0.0773]	[0.0062]
-1	0.0388	0.0042
	[0.0788]	[0.0064]
0	0.0378	0.0109
	[0.0831]	[0.0061]*
1	0.2019	0.0156
	[0.0860]**	[0.0062]**
2	0.1612	0.0142
	[0.0943]*	[0.0052]***
3	0.1665	0.0110
	[0.0888]*	[0.0064]*
4	0.1791	0.0118
	[0.0922]*	[0.0088]

Notes The estimates presented are weighted least-squares estimates of specification 2. Standard errors are clustered at the birth-district level. The model includes fixed effects for birth-district, birth-year, and gender, as well as baseline district characteristics (those displayed in Table A.4), interacted with a linear trend in year of birth. The model also includes birth-region linear cohort trends. Data includes individuals born in Tanzania between 1977 and 1994 who are observed in the 2012 census. Regressions estimated on data collapsed to cells defined by birth-district × birth-year × gender and regressions are weighted by cell sizes.

Table A.11: List of Occupations by ISCO-88 One-Digit Classification

List of Occupations by ISCO-88 One-Digit Classification	
1: Senior officials and managers	6: Skilled agricultural workers
2: Professionals	7:Machine operators
3: Technicians and associate professionals	8:Assemblers
4: Clerks workers	9: Craft
5: Service and shop workers	

Notes. This table shows the identification of the occupations used by the Tanzanian census (IPUMS). The ISCO-88 occupational code.

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