# Anticipating unintended consequences of policy: Learnings from Indonesia's child labor reform*

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**Abstract**

We study the causal effects of a labor law that governs child workers on labor market outcomes and the well-being of individuals. We exploit the timing of the national legislation to identify the causal effects of child labor reform using the Regression Discontinuity Design. We find that individuals who entered adulthood after the reform are less likely to have participated in the labor market during childhood. The reform also lowers the likelihood of poor health and improves the probability of working in paid jobs when children have reached adulthood. Our heterogeneity analysis highlights the importance of complementing regulation with enforcement and support programs to minimize unintended consequences that plagued many similar reforms.

*Keywords:* child labor, unintended consequences, harm minimization, rdd, adult outcomes, regulation, enforcement

*JEL Codes:* C21, J08, J80, I15, I25, J21, O15

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# 1 Introduction

When confronting a policy challenge, one of the most instinctive approaches for policymakers is through regulation. While regulation is a crucial device to direct and manage the behavior of its subjects and has long been prescribed to correct some market failures, such as asymmetric information or imperfect competition, many regulation fails to adequately address the underlying incentive of the actors involved, resulting in government failures(Grand, 1991).

Child labor is a topic where this issue has been prominent. Despite being one of the most popular instruments in combating child labor, the effectiveness of raising the minimum working age or banning child labor is not established in the literature. Studies on this topic do not demonstrate a clear consensus in terms of the direction of the effect or whether there were any effects at all. Studies in high-income economies such as Spain and the United States show that a higher minimum working age reduces the incidence of child labor and improves educational attainment (Del Rey et al., 2018; Fagernas, 2014; Lleras-Muney, 2002). However, this seems to hold only in developed countries with much more stringent regulation enforcement.

The experience of developing countries is much more diverse. There are countries like Mexico that reproduce the same result as in developed countries (Kozhaya and Martinez Flores, 2022). Yet, not far to the south of Mexico, in Brazil, one study found no impact of the policy on the incidence of child labor in Brazil (Bargain and Boutin, 2021), while another study found the policy reduces the probability of employment among boys (Piza and Souza, 2017). Edmonds and Shrestha (2012) and Boockmann (2010) posit that the non-existent impact of higher minimum working age is a common discovery in many other low- and middle-income economies, largely due to lenient regulation enforcement. Bharadwaj et al. (2020) finds evidence of the perverse effects of a child labor ban with their empirical study of India. The study showed that the child labor ban decreases child wages and increases child labor supply. Furthermore, Lakdawala et al. (2025) found that enforcement, not work safety, appears to be the main reason households reduce child labor in Bolivia.

Indonesia is another interesting case in this policy dilemma. In the early 2000s, Indonesia engaged in a series of reforms to suppress child labor, culminating in the 2003 Manpower Act, which raised the minimum working age from 15 to 18 years old and clamped down on many exemptions in the previous law. Yet, unlike India, Indonesia complemented the child labor ban with enforcement and support programs that seek to help households cope with the unintended economic pressure of the child labor ban. In addition to the legislation, the reform also gave birth to the 20-year enforcement program called the National Action Plan. In 2022, Indonesia completed the 20-year fight against child labor. While the incidence of child labor is much lower now than 20 years ago, how much of this achievement is attributable to this policy effort lacks empirical verification.

Beyond the immediate impact on child labor incidence and educational attainment, the long-term implications of working as a child also remain an open question. Unfortunately, this is an area that the aforementioned studies on child labor reform barely touched.[1] To shed some light on this question, we also investigate the impact of the reform on health and labor market outcomes after the children reach adulthood. To the best of our knowledge, this study is the first attempt to study the long-term impact of child labor reform in developing countries.[2]

The enactment of the 2003 Manpower Act created a natural separation of cohorts of children into different policy regimes. Children who have not turned 18 by March 2003 will enjoy a higher degree of protection and government support to stay out of the labor force and stay in school compared to the previous cohort of children. I exploit this discontinuity by utilizing the Regression Discontinuity Design (RDD) to unravel the causal effect of this reform on a wide-ranging set of outcomes.

In this study, we find evidence that the reform leads children to better adulthood to a certain extent. In the immediate impact, individuals who entered adulthood after the reform are less likely to have worked as children. In the male subsample, we find evidence that the reform increased the likelihood of attending and graduating from senior high school. In terms of the long-term impact, we find that the reform makes the younger cohort of children less likely to have poor health and more likely to be employed in paid work during adulthood. Yet, we do not find evidence that

[1] The notable works are Bellés-Obrero et al. (2022) and Bellés-Obrero et al. (2023), who found that Spain's minimum working age legislation led to a reduction in mortality rate, delayed fertility, and improved infant health.

[2] The studies that have ventured into this area rarely exploit policy reform to understand the long-term impact of child labor. They also demonstrate mixed findings for some outcomes. See Beegle et al. (2009) and Lyon and Rosati (2014) for impact on educational achievements. See Lyon and Rosati (2014), Jayawardana et al. (2023), and Beegle et al. (2009) for impact on long-term physical and mental health. See Emerson and Souza (2011), Ilahi et al. (2009), and Beegle et al. (2009) for adult labor market outcomes.

it helps them acquire better-quality jobs or higher earnings. Through heterogeneous analysis, we find that the impact is concentrated among boys and regions with early enforcement, highlighting the importance of aligning regulation with enforcement and support programs.

We organize the rest of the paper as follows. Section 2 lays the landscape of the child labor problem in Indonesia and dissects the child labor reform in more detail. Section 3 describes the data used in the study and the empirical strategy to identify causal effects. Section 4 presents the analysis of the results. We dedicate Section 5 specifically for robustness checks to firm up the identifying assumption of my empirical design. Finally, Section 6 provides some commentaries on the implications of the results, and Section 7 concludes the paper.

# 2 Institutional context

## 2.1 Child labor laws and household decision

Child labor reforms, particularly child labor bans, often misfired or were ineffective because the child labor supply is typically the result of household decision-making. As such, any change in child labor law will affect market wages for adults and children differently, influencing the within-household incentives. Basu and Van (1998) and Basu (2005) provide a theoretical prediction that a child labor ban, despite the best intentions, may result in unintended consequences of a higher incidence of child labor. Considering the heightened risk of hiring child labor due to the new law, firms might offer lower wages for child workers. In response to lower wages for each child worker, poor households might opt to send more children to work to maintain household income. Some households might need to withdraw their children from school to do so. These within-household decisions are out of reach of the law, especially when enforcement is lacking.

Households facing lower market wages for children may not be so inclined to send their children to work if there are other sources of income to compensate for the income loss (e.g., transfers) or if the opportunity costs of the children going to work increase. In line with this, Edmonds and Pavcnik (2005) noted that reducing the real and opportunity cost of schooling – i.e., interventions that aim to alter the household's incentive calculus directly – offers more promise than regulation to tackle child labor issues.

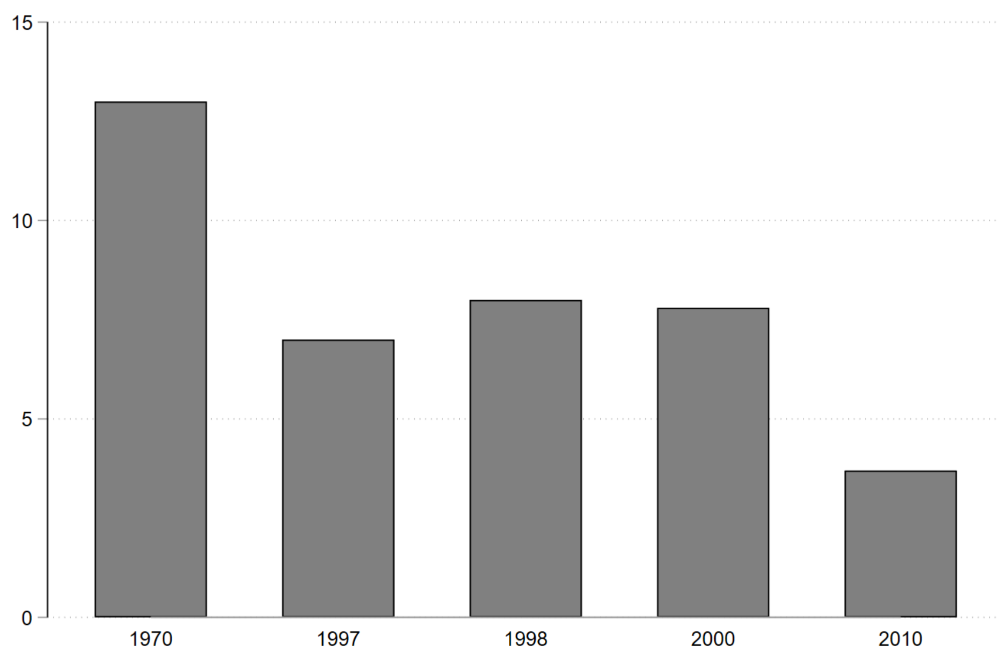
## 2.2 Child labor and education

The trade-off between children's schooling and market work means that the issue of child labor is inextricably linked with schooling participation. One of the reasons why much attention has been given to addressing the child labor issue in Indonesia is not only because it is detrimental to children's welfare, but also through its interplay with educational outcomes, something which the Indonesian government has been desperately trying to improve.

The official figures show that the incidence of child labor has been gradually but steadily declining since the 1970s (Bessell, 2009). Back then, 13% of children aged

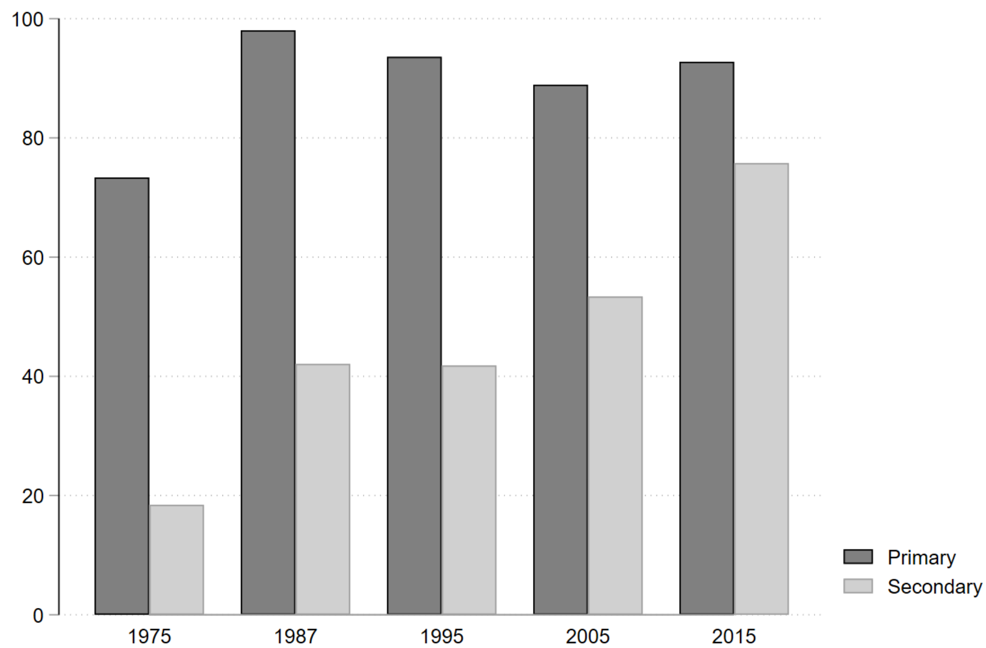
10-14 were engaged in work. This period was characterized by oil boom-driven high economic growth coupled with massive school construction projects, which boosted years of schooling for the younger generations (Duflo, 2001). By the beginning of the Asian Financial Crisis (AFC) of 1997, the incidence of child labor among 10-14-year-olds had fallen to 7%. However, the AFC, also possibly exacerbated by the lax 1997 Manpower Law in response to it, increased the incidence to 8% in 1998. The recovery period then brought it back down to 7.8% by 2000 (Bessell, 2009; Suryahadi et al., 2005). In 2010, Bureau of International Labor Affairs (2022c) notes that the rate has drastically fallen to 3.7%. Figure 1 visualizes this evolution.

Figure 1: Percentage of child labor among 10-14 year olds




Source: Compiled from Bessell (2009), Suryahadi et al. (2005), and Bureau of International Labor Affairs (2022c).

Figure 2: Net school enrollment rate




Source: World Development Indicators.

This seems to be a rosy story for Indonesia. Indeed, compared to other countries in similar income groups, Indonesia's child labor problem is mild (Manning, 2000). Yet when we put it in the context of improving educational attainment, Indonesia's child labor problem remains dire. If we extend the age group to 10-17-year-olds, the percentage of working children in 2018, 2 years before the COVID-19 pandemic, jumps to 7.05% (Windiarto et al., 2019). Even this number is already an increase from 5.99% in 2015 (Windiarto et al., 2019), and the COVID-19 pandemic must have made it more severe (Yulisman, 2021). This shows that the larger part of the child labor issue lies with older, high secondary-age children. To further illustrate this point, if we narrow down the age range of older children to 15-17-year-olds, ILOSTAT data shows that the incidence of child labor was at 14.66% in 2019, a year before the COVID-19 pandemic.

Meanwhile, Indonesia faces a serious challenge in raising its secondary school enrolment rate significantly. Unlike primary school, the secondary school enrollment rate is nowhere near universal (Education Policy and Data Center, 2018). Figure 2 shows that the 2015 secondary enrolment rate is only around the level of the primary enrolment rate back in 1975.[3] Since the passing of AFC, the annual increment to the secondary enrolment rate is only around 1 percentage point.[4] If the secondary level is decomposed further into junior and senior secondary, we will see an even lower enrollment rate for senior secondary school (Suharti, 2013). In addition to enrollment, Indonesia also faces the issue of school retention as students drop out midway through education (Suharti, 2013). These numbers indicate that involvement in the labor market remains an impediment for older children to attend and complete school. It is also why the policy focus of this paper is of particular importance.[5]

[3] The 2015 primary enrolment rate is extrapolated from the 2016 figure.

[4] Primary school enrollment rate reached its peak in the mid-1980s but then fell until the early 2000s, possibly due to the oil bust of the 1980s and then the AFC. It has picked up again until now.

[5] Indonesia has a nine-year compulsory schooling policy (six years of primary and three years of junior secondary schooling) in place since 1994. However, Lewis and Nguyen (2020) raised skepticism over its effectiveness in raising the completion rate. Whether it had any impact on child labor incidence is another unanswered question.

## 2.3 Child labor reforms in Indonesia

As the previous section suggests, child labor has plagued Indonesia severely in the past. While child labor is a symptom of poverty, the government's policies also played a role in exacerbating the maladies. During Indonesia's manufacturing revolution in the 1980s, the government repealed the prohibitions for children to work (Bessell, 1999).

Before the reform, the prevailing workforce regulation was the 1997 Manpower Act (Law 25/1997), which stipulated that the minimum working age was 15 years old. Yet scholars have widely documented the extremely limited effective legal protection towards child labor as the law allowed many exemptions and loopholes that employers could exploit (Bessell, 1999). Regulatory enforcement was almost nonexistent under this policy framework (Bureau of International Labor Affairs, 2001). The lax control might be somewhat deliberate as the government's compromise against the backdrop of AFC (Bessell, 1999). Households needed an economic buffer, which the government might scarcely provide at the time.

In the early 2000s, Indonesia finally began a series of reforms to suppress child labor. It began when Indonesia ratified two ILO conventions on child labor. The first is on the minimum working age of 15[6] and the second is on eliminating the "worst forms of child labor"[7]. The convention describes the four types of worst forms of child labor as (1) slavery, (2) prostitution and pornography, (3) illicit activities, and

[6] ILO Convention 138/1973 ratified by Indonesian Law 20/1999.

[7] ILO Convention 182/1999 ratified by Indonesian Law 1/2000.

(4) work that might be harmful to children's health, safety, and morals as defined by the local regulation.

Yet it was not until 2002 that Indonesia launched the National Action Plan for the Elimination of the Worst Forms of Child Labor as a concrete policy manifestation of the ILO convention ratifications. This Action Plan further details the thirteen employments that fall under the last criterion of the worst forms of child labor. They are[8]:

- Prostitution
- Mining
- Pearl diving
- Construction work
- Work in the fishing platform
- Garbage scavenger
- Production and activities involving explosives
- Working in the street
- Domestic assistant
- Cottage industries
- Plantation
- Timber
- Industries and activities involving hazardous chemicals

[8]The government specified some of the sectors further in Minister of Manpower and Transmigration Decree 235/MEN/2003.

Table 1: Programs within the National Action Plan for the Elimination of the Worst Forms of Child Labor

| **Program** | **Description** |
| --- | --- |
| Information campaign | · Aimed to mainstream the negative views on child labor.<br>· Conducted by the government and NGO counterparts.<br>· Targeted toward the general public and firms.<br>· Through bulletins/magazines, seminars, workshops, talk shows, short films, and cultural activities (e.g., theatre). |
| Institutional strengthening | · Establishment of local Action Plan Committees.<br>· Establishment of local report centers, reporting hotlines, and counseling centers for victims.<br>· Establishment of a dedicated unit within the police force to handle child trafficking.<br>· Capacity building for local authorities and organizations (NGOs, labor unions) to monitor and handle reports of child labor. |
| Education support | · School operational support fund/Bantuan Operasional Sekolah (BOS) and scholarships.<br>· Direct enrollment of child labor victims into local non-formal education units.<br>· Data collection and monitoring of children at risk of dropping out, who have dropped out, and who are at risk of joining the labor force. |
| Social assistance | · Conditional cash transfer – The Family Hope Program/Program Keluarga Harapan (PKH). |
| Labor inspection | · As stipulated by Law 21/2003, which ratified ILO Convention 81 concerning labor inspection.<br>· Executed by trained labor inspectors. |

Source: Sekretariat KAN-PBPTA (2008) and Bureau of International Labor Affairs (2004).

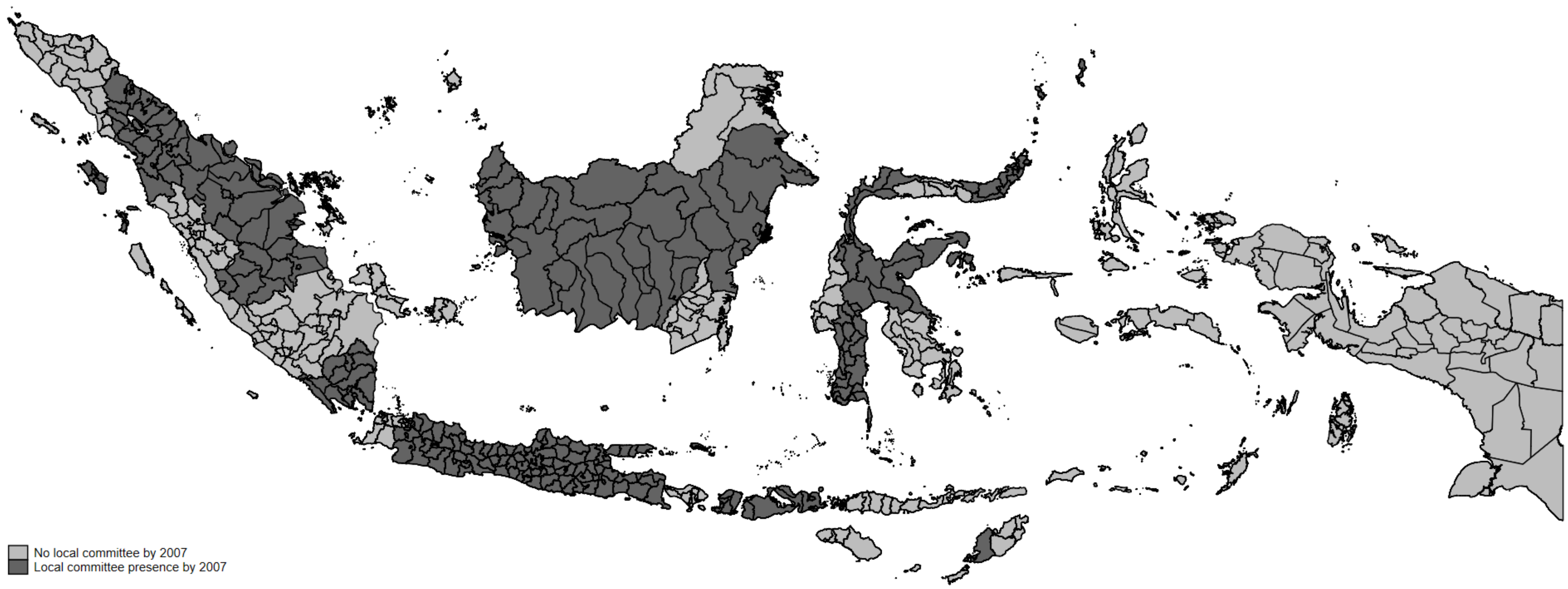
However, as stipulated in Sekretariat KAN-PBPTA (2008), the government also introduced its own five priority types of worst forms of child labor. They are (1) work in offshore fishing, (2) prostitution, (3) footwear, (4) mining, and (5) trading of illicit substances.

The Action Plan was divided into two phases, and each phase was ten years long (Ministry of Manpower and Transmigration, 2015). The first phase ran from 2002 to 2012 and focused on combating the worst forms of child labor, while the second phase ran from 2012 to 2022 and expanded the focus on eradicating child labor at large. Each of the phases was divided again into subphases. The first 10-year phase was divided into two 5-year subphases. The first subphase ran from 2002 to 2007 and focused on the five priority worst forms of child labor, while the second subphase ran from 2008 to 2012 and expanded the focus to other worst forms of child labor. These two are the main subphases that intersect with the period of this study.

The Action Plan comprises many programs. Table 1 summarizes the programs. Most of them are programs specific to the need to suppress child labor, but some of them are programs integrated into other policies, such as the school operational support fund (BOS) and the conditional cash transfer PKH.[9] One of the key components of the program is bringing children back to school if they have dropped out.

[9]There have been plenty of studies evaluating the impact of social assistance on child labor incidence and schooling outcomes. See De Silva and Sumarto (2015); Hidayatina and Garces-Ozanne (2019); Jayawardana et al. (2021); Lee and Hwang (2016); Utami et al. (2024); Wardani et al. (2022). In summary, while the evidence for the positive impact on schooling outcomes is more consistent across studies, the evidence on the impact on child labor incidence is less so.

Figure 3: Geographical distribution of the Action Plan local committee by 2007




Source: Sekretariat KAN-PBPTA (2008)

The implementation of the National Action Plan programs only really took off a year later after the legislation of the 2003 Manpower Act (Sekretariat KAN-PBPTA, 2008). Despite other controversies surrounding it, the 2003 Manpower Act strengthened the legal protection of child labor as it closed down many loopholes and exemptions and, most notably, raised the minimum working age from 15 to 18.

The law also stipulates a maximum jail sentence of four years and a minimum of one year for child labor employers. The punishment can also come in the form of or be accompanied by a financial penalty ranging from 100 million IDR to 400 million IDR. As the vast majority of child labor employers are in the informal sector (Manning, 2000), this amount is a steep charge that can devastate their business and even their personal livelihoods.

As is expected from any national program in developing countries, there was heterogeneity in implementations. Sekretariat KAN-PBPTA (2008) reports which provinces and districts have established local Action Plan Committees by the end of the first subphase (2007). We call these regions "early enforcers". We argue that this is an important predictor of program success for two reasons. First, the local committee is the body responsible for executing and monitoring the program at the local level. As such, the presence of this agency at the provincial or district level can translate into swifter and more focused program implementation and response. In post-decentralization Indonesia, local government capacity is a crucial determinant of societal outcomes (Lewis, 2017a,b). The degree to which all other activities are carried out effectively or, if at all, depends on the local government's initiatives. Second, these regions, being early enforcers, speak a lot about their initiatives, capacity, and commitment to slashing child labor. As such, it can capture many unobserved regional variations.

Figure 3 visualizes the geographical distribution of the Action Plan local committee by the end of the first phase (2007). 54% of local governments in Indonesia had already established local committees in the first few years of program implementation.

# 3 Empirical design

## 3.1 Data

We utilize data from the Indonesia Family Life Survey. IFLS is a longitudinal survey on a wide range of household and individual information that began in 1993 and has tracked the original and split households in the subsequent four waves. The data is representative of the 83% population in the 13 sample provinces in 1993 (Strauss et al., 2016). It is one of the most widely used datasets in micro-empirical studies.

We draw all observations and most of the variables from the latest wave of data (IFLS5) collected in 2014.[10] Choosing a data point farthest away from the policy reform allows us to evaluate the impact on outcomes after the children reached

[10] Table A1 and Table A2 in Appendix A present the summary statistics for outcomes and covariates, respectively, for the entire sample. We discuss covariate balance for the observations within the narrow bandwidth in Subsection 5.2.

adulthood. Our initial sample (pre-bandwidth selection) consisted of individuals aged 18-55 in 2014.[11] As such, our sample consists of working-age individuals, the youngest of whom only recently entered adulthood, and the oldest of whom is about to retire. We continue the elaboration on the sample size when discussing mass points in Subsection 3.2 on identification strategy.

To construct the main outcome variables, we use the working history module from IFLS1 (1993) all the way to IFLS5 (2014), which traces the individual's working experience from the first employment. The outcome is a dummy variable indicating whether the individual ever worked before 18 years old. The same working history module also allows us to identify the employment status and the sector of the child's work. We use this to analyze the heterogeneous impact on various employment types and sectors. Our main education outcomes are whether the individual attended and graduated senior secondary school.[12]

We examine several adult outcomes to estimate the long-term impact of the reform. First, we investigate the probability of attending college. If the reform succeeded in getting more children to complete senior high school, it might influence the probability of attending higher education. Motivated by past studies, the health outcomes are whether the individual ever missed activities due to poor health in the

[11] 55 was the official retirement age in Indonesia back in 2014. We decided not to include all the elderly in our sample because adding them would hardly impact bandwidth selection, as they will be placed on the far left end of the cutoff.

[12] The typical age range for senior secondary education in Indonesia is 16-18 years old, although early or delayed school entry or graduation is possible. We conjecture that raising the minimum working age to 18 from 15 years old must change the incentive of households on whether their children should stay in school, especially as the compulsory schooling age remains at 15 years old. See Lewis and Nguyen (2020)

past month and whether the individual exhibited any depressive symptoms in the past week prior to data collection.[13] For the adult labor market outcomes, as past studies have done, we are interested in whether the reform changes the likelihood of being employed in paid work and yearly earnings[14] when the children are adults. Adding new contributions to the literature, we test the impact on the quality of jobs in adulthood, proxied by whether the individual works in a formal job and whether the individual is working with a contract.

We employ several covariates to improve statistical precision (Cattaneo et al., 2019a). They relate to the identity of the observations (male dummy), the household socioeconomic situation during childhood (whether the main breadwinner was in formal work, access to electricity, a dummy for drinking filtered water, a dummy for owning a toilet, and household size), and the regional circumstances in which the children grew up in (urban/rural status and whether the region was early enforcer).[15]

### 3.2 Identification strategy

We employ the sharp Regression Discontinuity Design (RDD) framework to identify the causal impact of the reform. Our running variable is age at the time of the policy reform, constructed with year and month of birth. We set the cutoff to 18

[13] We follow Jayawardana et al. (2023) in constructing the short CES-D mental health score.

[14] IFLS labor module provides monthly and yearly earnings information. We choose yearly earnings to remove the seasonality effect that is more likely to be present in monthly earnings.

[15] The second and third covariate sets were fixed when the children were 12 years old. This is the age at which children graduate or enter the final year of primary school. It is a critical juncture in which children are at a crossroads over continuing junior high school or starting work (Suharti, 2013; Suryahadi et al., 2005). Suryahadi et al. (2005) found an exponential jump in the probability of joining the labor market at around this age.

years old. Individuals who have yet to turn 18 by the time of the policy reform will be exposed to a higher degree of labor protection and education support while they are children. As such, they are the treatment group. The control group consists of individuals who have turned 18 by March 2003.[16] In other words, we will be comparing younger (treatment) and older (control) cohorts of children around the cutoff. "Around the cutoff" here can also be interpreted as around the time the children lived their adolescence.[17]

The RD approach utilizes nonparametric estimation represented by equation 1.

$$Y_i = \tau T_i + g(X_i) + \mu_i \tag{1}$$

$Y_i$ is the outcome variable. $T_i$ is the treatment status; hence, $\tau$ is the treatment effect. Note that given the variety of programs under the reform as described in Subsection 2.3, this will be the aggregate treatment effect of the reform, not the per-program effect. $g(X_i)$ is a polynomial function of the running variable and $\mu_i$ is the error term. All of the standard errors we report in this paper are robust and bias-corrected, as suggested by (Lee and Lemieux, 2010).

[16]Note that when we normalize the running variable so that the cutoff is zero, we also flipped the running variable such that the treatment units are on the right side of the cutoff. Otherwise, they will be on the left side of the cutoff by construction because their age by policy reform is smaller than 18.

[17]Fuzzy RDD is not appropriate in this setting. Being treated simply means the individual has not turned 18 by March 2003. Consequently, noncompliance entails individuals turning 18 when they are supposed to have not turned 18 by the time of policy reform. This condition implies a mismatch between reported and actual age. To verify this, we need data on actual age. However, if we had data on actual age, I would have been better off using the actual age as a running variable. Therefore, the key issue is really about running variable manipulation, which we discuss in detail in Subsection 5.1.

There are two approaches to RD estimation, continuity-based and randomization-based. The former approach conducts estimation by fitting a regression line around the cutoff. This requires continuity of outcome conditional on running variable near the cutoff.[18] In other words, if we plot the running variable on the x-axis and the outcome on the y-axis, we should not observe wild jumps between points. The distance between the points should be smaller, and the plot smoother as it approaches the cutoff. Otherwise, fitting the regression line around the cutoff would be problematic as the projection will be imprecise (Cattaneo et al., 2019a).

To have sufficient continuity of outcome near the cutoff, the running variable must have sufficient mass points; that is, the running variable must have sufficient unique values to allow a smoother plot of the outcome as it reaches the cutoff from both sides. Table A3 in Appendix A presents the sample size, mass points, and observations per mass point in this study across treatment and control. A total of 475 mass points would be considered as moderate (Cattaneo et al., 2023). We can still run the continuity-based approach, but we will verify the results with the second approach, which is more amenable to a limited number of mass points, the randomization-based approach.

Unlike the continuity-based approach, the randomization-based approach does not fit a regression line around the cutoff. It simply computes the difference in mean between treatment and control units around the cutoff to estimate the treatment effect. As it does not require smooth continuity of outcome near the cutoff,

[18]Continuity near, but not at the cutoff. Continuity of outcome at the cutoff implies no treatment effect.

randomization-based RD is preferred for cases of limited mass points (Cattaneo et al., 2023). Hence, all our estimations will report the results from these two approaches to check for consistency of results. Given moderately-sized mass points, we also cluster standard errors at the running variable as Cattaneo et al. (2023) advised.

As is standard in any RD approach, there are three specification choices to make. The first is the bandwidth determination, the second is the degree of the polynomial of the running variable in the estimation, and the last is the type of weights for observations, known as the kernel.

For the continuity-based approach, we use the standard data-driven bandwidth selection technique that optimizes the bias-variance trade-off by minimizing the mean-squared error (Imbens and Kalyanaraman, 2012; Skovron and Titiunik, 2015). As for the randomization-based RD, the standard procedure is finding a window where the covariates are balanced (Cattaneo et al., 2023). The choice of bandwidth is of high importance because it is critical to the RD framework's identifying assumption; that is, observations around the cutoff are similar in all respects except for treatment assignment; thus, the difference in outcome can be attributed to treatment effect rather than other confounding factors.

In a simple naive linear regression of child labor experience on treatment status, we would obtain biased treatment effect estimates. In this context, the greatest concern stems from omitted variable bias, as there might be myriad factors other than the policy reform that influence whether a child would participate in the labor force. The RD approach addresses this problem by conducting estimations not on the entire sample but only on the observations within some narrow bandwidth of the cutoff.[19] The expectation is that treatment and control units around the cutoff are close counterfactuals to each other. Indeed, the further away we are from the cutoff, the less likely this expectation will materialize. Subsection 5.2 empirically verifies whether this requirement has been fulfilled. We check for power in all estimations. Statistical power is crucial in the RD approach because we only conduct estimation on the sample within a narrow bandwidth (Cattaneo et al., 2019b).

Following Gelman and Imbens (2014), the continuity-based approach will use polynomial degree one (local linear regression) and two (local quadratic regression) of the running variable when fitting the regression line within the bandwidth.[20] This will allow us to examine if the result is consistent across different degrees of polynomials. As for the randomization-based approach, the polynomial is of degree zero by default since it is a simple difference in mean between the treatment and control group. Finally, all estimations will employ a triangular kernel that places heavier weight on observations closer to the cutoff.

### 3.3 Heterogeneity analysis

We observe heterogeneous impacts across gender, socioeconomic status, residential location, and enforcement regimes in the area. Yet, we implement a slightly

[19]Note that as RD estimations are conducted only within some narrow window, the appropriate interpretation for the treatment effect is Local Average Treatment Effect (LATE) (Lee and Lemieux, 2010).

[20]Gelman and Imbens (2014) also demonstrated how higher degree polynomials may inflate point estimates and thus indicate the presence of discontinuity where one actually does not exist.

adjusted strategy for this exercise. Conducting estimation on subsamples means a drop in the number of observations for each estimation. In this situation, there is a risk that the nonparametric RD estimator in model 1 leads to unreliable estimates, as a small sample size might lead to insufficient bunching of mass points around the cutoff (Cattaneo et al., 2023). The natural solution is randomization-based RD as it does not require bunching of mass points around the cutoff (Cattaneo et al., 2023). However, with a small sample, there is also a risk that the randomization-based RD procedure cannot find a window where the covariates are balanced because covariates are more likely to be imbalanced in a small sample. Even if the procedure managed to identify a window where the covariates are balanced, the number of observations in that window might be too small for proper estimation. As such, we're using parametric RD, which is a typical alternative for discrete running variable (Cattaneo et al., 2023). I model the estimator in equation 2 for polynomial degree one and 3 for polynomial degree two.

$$Y_i = \alpha_1 + \tau_1 T_i + \beta_{11} X_i + \beta_{12} T_i X_i + \beta_{13} C_i + \mu_{1i} \tag{2}$$

$$Y_i = \alpha_2 + \tau_2 T_i + \beta_{21} X_i + \beta_{22} T_i X_i + +\beta_{23} X_i^2 + \beta_{24} T_i X_i^2 + \beta_{25} C_i + \mu_{2i} \tag{3}$$

In the local linear model, the additional term compared to the nonparametric model is $T_i X_i$, which is just the interaction between the treatment status and running variable. Like the nonparametric model, the coefficient on the treatment status $\tau_1$ represents the treatment effect. The model structure is largely the same in the local quadratic equation, except I add the squared value of the running variable $X_i^2$ and its interaction with the treatment status $T_iX_i^2$. Again, parameter $\tau_2$ provides the treatment estimate. In both equations, $\alpha_1$ and $\alpha_2$ are just model intercepts, $\mu_{1i}$ and $\mu_{2i}$ are error terms, and $C_i$ is a vector of covariates. Bandwidth for the parametric estimation is taken from the mean squared error minimization procedure under the nonparametric estimation. The final difference with the nonparametric is the sample weighting. While the nonparametric RD employs a triangular kernel where the observations closer to the cutoff are weighted more heavily, parametric RD can only accommodate a uniform kernel where all observations are weighted equally.

# 4 Results

## 4.1 Main effects

Table 2 presents the main RD estimation results of the effect of child labor reform on child labor incidence and schooling outcomes. These results are based on observations within the bandwidth. Table A4 in Appendix A presents the age of observations within the bandwidth at the time of the reform and their corresponding birth date.[21] All the effective treatment units were in their secondary schooling years during the reform. Note that all estimations for all outcomes are sufficiently powered.

[21] The bandwidth in continuity-based RD (Panel A and B) is symmetric on both sides of the cutoff. Meanwhile, the randomization-based RD (Panel C) window is not symmetric because each mass point is tested as a possible window. This is the recommended procedure for when there are mass points in the running variable (Cattaneo et al., 2023).

Table 2: Effect on the main outcomes

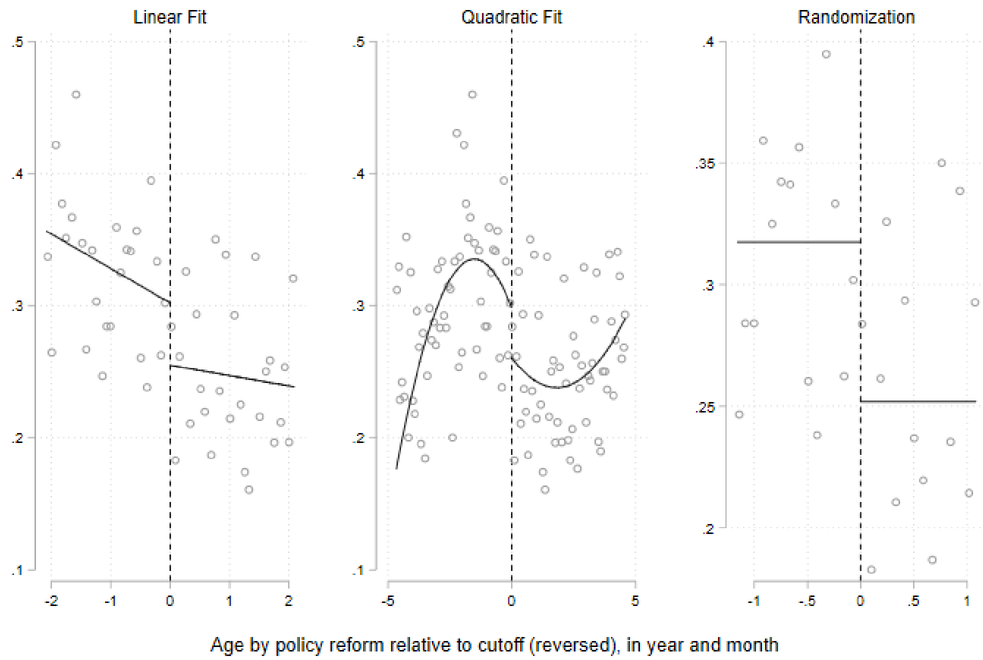
| | (1) Ever work before 18 | (2) Attended SHS | (3) Completed SHS |
|---|---|---|---|
| | Panel A: Linear | | |
| Treatment Effect | -0.0700** | 0.0250 | 0.0197 |
| | ( 0.0316) | ( 0.0218) | ( 0.0306) |
| Bandwidth | 2.1396 | 5.6167 | 2.9314 |
| Eff. Control | 1,814 | 4,963 | 2,607 |
| Eff. Treatment | 1,725 | 4,327 | 2,418 |
| Power | 1.0000 | 1.0000 | 1.0000 |
| | Panel B: Quadratic | | |
| Treatment Effect | -0.0611** | 0.0165 | 0.0186 |
| | ( 0.0310) | ( 0.0294) | ( 0.0360) |
| Bandwidth | 4.6785 | 5.2301 | 4.4055 |
| Eff. Control | 4,130 | 4,602 | 3,852 |
| Eff. Treatment | 3,623 | 4,039 | 3,440 |
| Power | 1.0000 | 1.0000 | 1.0000 |
| | Panel C: Mean Difference | | |
| Treatment Effect | -0.0616** | -0.0016 | 0.0064 |
| | ( 0.0269) | ( 0.0295) | ( -0.0297) |
| Left Window | 0.8247 | 0.8247 | 0.8247 |
| Right Window | 0.7616 | 0.7616 | 0.7616 |
| Eff. Control | 766 | 766 | 766 |
| Eff. Treatment | 840 | 840 | 840 |
| Power | 1.0000 | 1.0000 | 1.0000 |
| Control | 16,469 | 16,469 | 16,469 |
| Treatment | 8,368 | 8,368 | 8,368 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In the first column, we find robust evidence of impact on child labor incidence below 18 years old. Throughout all specifications, whether we use linear fit, quadratic fit, or randomization specification, child labor reform reduced the probability of working before 18 years old by around 6 to 7 percentage points.[22] All treatment effects are statistically significant at the 5% level. Figure 4 visualizes the discontinuity at the cutoff, indicating a statistically significant treatment effect.

Figure 4: Treatment effect on the probability of working before 18 years old




In the second and third columns, we do not find treatment effects on the probability of attending or completing senior high school in the full sample. However, when we perform separate estimations on the subsample split by gender in Table B1, we find that the reduction of child labor was concentrated among boys (columns 1 and 2). This is a similar story to Brazil, where the impact of the reform was only

[22] I discuss more about the treatment effect size and comparison with other studies in other countries in Section 6.

observed among boys (Piza and Souza, 2017) and in contrast to Mexico, where both boys and girls benefit from the reform (Kozhaya and Martinez Flores, 2022).

It is no surprise that in columns 3-6, we also observe that the effect on schooling outcomes was concentrated among boys. Throughout all specifications, the child labor reform increased the likelihood of boys attending senior high school by around 7.42 to 8.35 pp and completing high school by around 5.94 to 8.56 pp. The treatment estimates for boys are statistically significant and robust in all specifications.

In Table B2, we show heterogeneous effects by our socioeconomic status proxy: whether the main breadwinner during childhood worked in formal employment. We find that the child labor reduction effect was likely to be concentrated among children from households with lower socioeconomic status, those whose breadwinner did not work in formal employment during childhood. This suggests that the reform succeeded in shielding poor households from the potential economic shock of the ban. Unfortunately, we do not detect any effect on schooling outcomes in either subsample.

In Table B3, we find that the child labor reduction effect was concentrated in rural areas, where the child labor incidence is higher (Bessell, 2009). Splitting the sample along the urban-rural divide does not yield any impact on schooling outcomes.

In Table B4, we find robust evidence that the child labor reduction effect was concentrated in early enforcement regions. This supports the notion that regulation can be effective if it anticipates unintended consequences early on. This result is similar to the Brazilian child labor reform, except that Bellés-Obrero et al. (2022)

only found impact in the high enforcement areas and not in the main estimation. Some treatment estimates for schooling outcomes in early enforcement regions are also statistically significant, although not robust across specifications.

### 4.2 Effects on specific employment types and sectors

We examine the sectors driving the reform's impact following past studies (Kozhaya and Martinez Flores, 2022; Piza and Souza, 2017). However, past studies could only observe employment status at one point in time due to the use of national household survey data around the time of the reform. In this study, IFLS data enables us to observe the complete working history of every individual since their first employment. Consequently, while the results of other studies represent a static outcome, we are able to observe the dynamic effects between employment types and sectors after the reform, including whether there was any labor displacement.

Given that the child worker might have moved between jobs at any point in time before 18 years old (or exited the labor market only to return again), the outcome in this exercise is a dummy equal to one if the individual ever worked in the sector or employment status in question before 18 years old. Constructing the outcome this way allows us to analyze whether the reform made child labor incidence less likely in certain employment types/sectors but more likely in other employment types/sectors.

We construct five classifications of employment types. They are paid work, unpaid work[23], formal employment, informal employment, and hazardous work[24]. As for the sectors, we split them into six: agriculture, mining, manufacturing, services, construction, and retail.[25]

Child labor incidence in each specific employment status or sector will be lower than the overall indicator used as the main outcome. This is because the criterion for the outcome in this section is more restrictive than the main outcome. Formal and informal employment are subsets of paid work. Paid and unpaid work are subsets of child labor incidence as a whole. While an individual can work in any sector or any employment status during their childhood to have a dummy value equal to one in the main estimation, one must have work experience in a specific employment status or sector during childhood to qualify as a child laborer in this exercise.

To save space, we relegate all the result tables in this section to Appendix C. As in the main estimation, we also analyze heterogeneity on these outcomes and report the results in Appendix D and Appendix E for employment types and sectors, respectively.

#### 4.2.1 Employment type

Table C2 presents the estimation results of the reform's impact on various employment types. We do not detect a robust effect of the reform when we restrict

[23]Unpaid work usually refers to working on a farm and non-farm family business.

[24]We provide the complete list of hazardous occupations in Table C1 in Appendix C

[25]Agriculture includes forestry, fishing, and hunting. Services include electricity, gas, water, transportation, storage, communications, finance, insurance, real estate, business services, and social services. Retail includes trading, restaurants, and hotels.

the outcome to specific work types. However, when we conduct estimations of the subsample in heterogeneity analysis, we find some interesting findings.

As in the main result, we find robust evidence that the reform effectively reduced paid work, formal sector work, and hazardous work among boys (see Table D1, Table D2, and Table D3, respectively). We do not observe any impact on unpaid and informal sector work either in the boys' or girls' subsample. Our result is largely in line with Kozhaya and Martinez Flores (2022), who also found a stronger negative impact in paid work.

Consistent with the main result, we find robust evidence that the reform effectively reduced paid work in the rural areas in Table D7. We do not find a robust effect on unpaid work, formal or informal employment, and hazardous work in rural or urban subsamples (see Table D8 and D9).

We find some evidence of the displacement effect when we split the sample based on the breadwinner's employment. In Table D5, we find that the reform was effective in reducing formal work among children whose breadwinner worked in formal employment (column 1). However, we also find that the probability of working informal work is higher among the same group of children (column 3). Although the coefficient is not statistically significant in all specifications, this raises concern that some households redeploy their children from formal to informal work. Informal work might be less visible; hence, it is easier for the children and the employer to avoid detection by the authorities. Other than that, we do not find any discernible pattern in this subsample split. We do not find robust treatment estimates in paid, unpaid, or hazardous work in any subsample (see Table D4 and Table D6).

Like the main estimation, we show in Table D10 that the reform effectively curbed paid work in early enforcement areas (column 1) and even quite possibly unpaid work as well (column 3). However, we find evidence that unpaid work in late enforcement areas rose after the reform (column 4). This time, it is unlikely to be a displacement effect since paid work incidence did not fall in these regions (column 2). Considering schooling outcomes were not affected in these regions (see Table B2 columns 4 and 6), the more likely scenario is that households chose to utilize them in their family business as unpaid workers. We report the rest of the results in this subsample split in Table D11 and Table D12, where we mostly find null effects on the other regressions in this subsample split.

#### 4.2.2 Sector

The final angle of analysis in this section pertains to the effect on employment in a specific sector. We report the result of the full sample estimation in Table C3. As in the previous section, we do not find employment in any specific sector driving the impact in the full sample estimation. We detect statistically significant treatment estimates in agriculture and mining. Yet, they are not robust across different specifications.

The heterogeneity analysis shows evidence of a reduction in agriculture employment in the early enforcement regions (Table E4). Yet, the highlight would be that we find major reductions in manufacturing employment among boys, in urban areas, and surprisingly, in late enforcement regions (see Table E9, Table E11, and Table E12, respectively), even though the treatment estimates are not statistically significant in all specifications on the last one. This result is consistent with the government's effort because the National Action Plan placed offshore fishing (included in agriculture) and footwear (a subsector in manufacturing) as one of the top priorities.

Unfortunately, one sector saw a displacement effect. Table E24 The reform effectively reduced employment in trading and restaurants in the early enforcement regions. However, this gain is countered by the rise of employment in this sector in the late enforcement regions.

The result of this section is again largely in line with Kozhaya and Martinez Flores (2022), who found a more substantial sectoral impact in manufacturing. However, they found an effect in the services sector and no impact in agriculture. Instead, our study finds an effect on agriculture employment and none on services employment.

## 4.3 Long term effects

Now, we turn our attention to the long-run effect of child labor reform 11 years after its enactment. Table 3 presents the results for education and health outcomes, while Table 4 provides the labor market outcomes results during adulthood.

Table 3: Effect on higher education and health outcomes

| | (1)<br>Attended college | (2)<br>Poor health | (3)<br>Depressive symptoms |
|---|---|---|---|
| | | Panel A: Linear | |
| Treatment Effect | -0.0263 | -0.0580* | -0.0347 |
| | ( 0.0217) | ( 0.0344) | ( 0.0294) |
| Bandwidth | 3.9587 | 2.8125 | 3.3218 |
| Eff. Control | 3,526 | 2,429 | 2,941 |
| Eff. Treatment | 3,098 | 2,294 | 2,655 |
| Power | 1.0000 | 1.0000 | 1.0000 |
| | | Panel B: Quadratic | |
| Treatment Effect | -0.0294 | -0.0574 | -0.0408 |
| | ( 0.0300) | ( 0.0349) | ( 0.0335) |
| Bandwidth | 4.1079 | 5.7370 | 4.7774 |
| Eff. Control | 3,668 | 5,019 | 4,239 |
| Eff. Treatment | 3,228 | 4,395 | 3,744 |
| Power | 1.0000 | 1.0000 | 1.0000 |
| | | Panel C: Mean Difference | |
| Treatment Effect | -0.0279 | -0.0653** | -0.0256 |
| | ( 0.0221) | ( 0.0299) | ( 0.0259) |
| Left Window | 0.8247 | 0.8247 | 0.8247 |
| Right Window | 0.7616 | 0.7616 | 0.7616 |
| Eff. Control | 766 | 725 | 674 |
| Eff. Treatment | 840 | 795 | 758 |
| Power | 1.0000 | 1.0000 | 1.0000 |
| Control | 16,469 | 16,466 | 16,468 |
| Treatment | 8,368 | 8,363 | 8,368 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Despite increasing the likelihood of completing senior high school, the first column of Table 3 shows that the reform did not influence the likelihood of attending college. While the reform reduced child labor incidence, it did not translate to better mental health during adulthood. Column 3 of Table 3 shows that while the point estimates for exhibiting any depressive symptoms are negative, none are statistically significant. The reform's only significant impact on health was making it less likely for individuals to miss activities due to illness when they are adults by around 5.8-6.5 percentage points (column 2 of the same table). These point estimates are statistically significant in the first-degree polynomial (Panel A) and randomization-based RD (Panel C).

Table 4: Effect on adult labor market outcomes

| | (1) Employed past week | (2) Work formal jobs | (3) Work with a contract | (4) Earnings past year |
|---|---|---|---|---|
| | Panel A: Linear | | | |
| Treatment Effect | 0.0547* | 0.0127 | 0.0066 | -0.1076 |
| | ( 0.0314) | ( 0.0253) | ( 0.0212) | ( 0.1263) |
| Bandwidth | 2.8682 | 3.4877 | 4.1710 | 3.6618 |
| Eff. Control | 2,509 | 3,091 | 3,726 | 2,332 |
| Eff. Treatment | 2,348 | 2,788 | 3,228 | 1,916 |
| Power | 1.0000 | 1.0000 | 1.0000 | 0.9999 |
| | Panel B: Quadratic | | | |
| Treatment Effect | 0.0890** | 0.0175 | 0.0023 | -0.2519 |
| | ( 0.0405) | ( 0.0293) | ( 0.0264) | ( 0.1570) |
| Bandwidth | 3.6574 | 4.3758 | 4.6784 | 3.0802 |
| Eff. Control | 3,229 | 3,852 | 4,130 | 1,976 |
| Eff. Treatment | 2,891 | 3,440 | 3,623 | 1,645 |
| Power | 0.9995 | 1.0000 | 1.0000 | 0.9953 |
| | Panel C: Mean Difference | | | |
| Treatment Effect | 0.0637** | 0.0137 | -0.0033 | -0.1638 |
| | ( -0.0263) | ( -0.0304) | ( 0.0205) | ( 0.1002) |
| Left Window | 0.8247 | 0.8247 | 0.8247 | 0.8247 |
| Right Window | 0.7616 | 0.7616 | 0.7616 | 0.7616 |
| Eff. Control | 729 | 729 | 729 | 501 |
| Eff. Treatment | 799 | 799 | 799 | 560 |
| Power | 1.0000 | 1.0000 | 1.0000 | 0.9999 |
| Control | 16,469 | 16,469 | 16,469 | 12,336 |
| Treatment | 8,368 | 8,368 | 8,368 | 4,848 |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Likewise, Table 4 also demonstrates the minimum impact of the reform on adult labor market outcomes. The reform did make the younger cohort of children more likely to be employed in paid jobs when they are adults. The effect sizes are quite substantial, too, from 5.4 to 8.9 percentage points increase in the probability of being employed. All these treatment estimates are also statistically significant (column 1 Table 4). Given that the reform made the treatment group less likely to have poor health when they are adult, it is unsurprising that they also have better employability. However, that is about as far as the reform impact goes. I do not find evidence that it increased the prospect of working in better jobs as it has no impact on whether the individual worked in a formal job, worked with a contract, or had higher earnings (column 2-4 Table 4).[26]

# 5 Robustness checks

In this section, we demonstrate the validity of our identifying assumption by walking through the results of our robustness tests. The verifications that we have executed are running variable manipulation test, covariate balance test, placebo cut-off test, and testing for possible causal factors other than the reform. All the result tables are in Appendix F.

[26]The estimation on earnings (column 4 Table 4) only includes wage earners.

## 5.1 Running variable manipulation

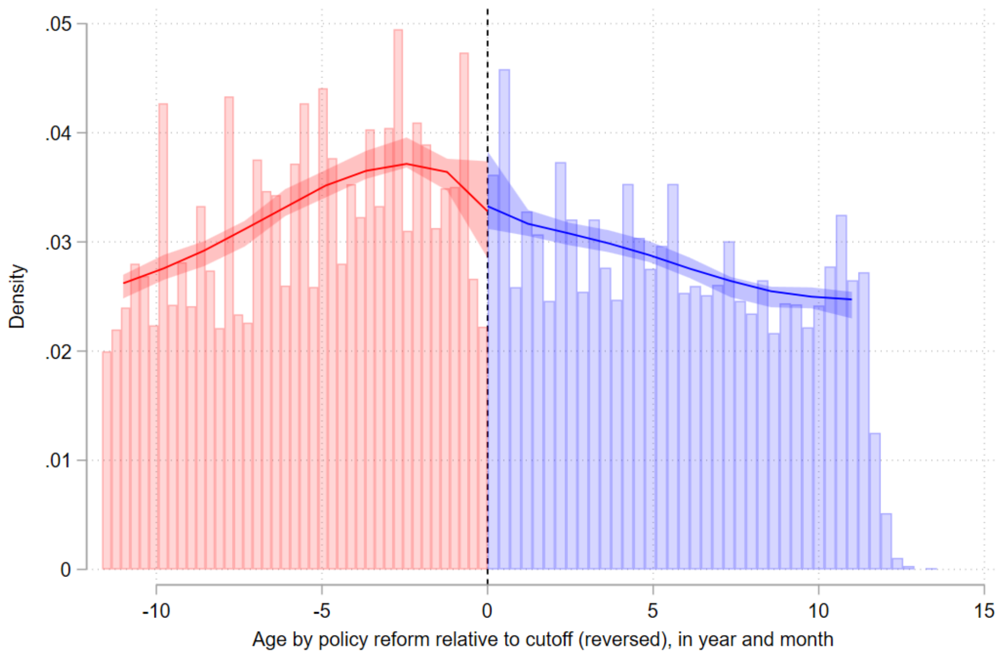
RDD identifying assumption requires that the cutoff is exogenous. This means units should not have the ability to choose or manipulate which sides of the cutoff they will be. If this is true, the density of the running variable should be continuous at the cutoff. In other words, there should be no stark jump or drop of running variable density at the cutoff. A wild jump or drop of mass point bunching at the cutoff suggests units might be able to manipulate the running variable to control at which sides of the cutoff they will fall.

We utilize the running variable manipulation test developed by Cattaneo et al. (2019a). This procedure constructs a running variable density function and tests whether the density function is continuous within some narrow bandwidth of the cutoff. The null hypothesis is that there is no discontinuity in the running variable density function at the cutoff. As is standard, the bandwidth is again selected using the MSE-minimization procedure.

Figure 5 plots the density function line and the corresponding 95% confidence interval band. The density function is continuous at the cutoff, and the CI band between the two sides of the cutoff overlaps. This suggests no statistically significant discontinuity of running variable density at the cutoff. Table F1 summarizes the test result. The t-statistic is far below the conventional t-critical value; hence, we do not reject the null hypothesis of no discontinuity.[27]

[27] Age is one of the most common running variables in research employing the RDD method. It has even been used in a context thought to be rife with age manipulations (see Carpenter and Dobkin (2009), who used age as a running variable to study the impact of alcohol consumption).

Figure 5: Running variable density




## 5.2 Covariate balance

If the cutoff is truly exogenous, then covariates or some placebo outcome that the treatment could not possibly affect should be balanced within the narrow bandwidth of the cutoff. In this study, the covariates we use are also not possible to be

The date of birth variable in Indonesian survey data has also been extensively used in the literature (see Duflo (2001); Lewis and Nguyen (2020); Shidiqi et al. (2023)). While there has been a popular belief that one can manipulate birth certificates in Indonesia, our data is based on survey questions, not administrative records. As such, while an individual might have the incentive to manipulate their date of birth for administrative purposes, there is less incentive to do so in the context of an anonymous survey where the research subject has built rapport with the researcher for many years. If anything, households in this context would prefer to manipulate their children's date of birth so that they are on the left of the cutoff instead of the right, so they can game the system and make their children work to fulfill pressing needs by making their children's age older than they should be.

affected by the treatment because they are all predetermined. When the cutoff is exogenous, we can confidently claim that the control group (older cohort of children) is a proper counterfactual for the treatment group (younger cohort of children). In continuity-based RD, covariate balance means covariates are continuous at the cutoff. In randomization-based RD, covariate balance simply means that there is no statistically significant difference in the covariate's mean between the treatment and control group.[28] This condition is automatically fulfilled under the randomization-based RD because the window selection procedure under the approach automatically searches for a window where the covariates are balanced. Otherwise, the procedure will not proceed with the computation.

To run the test in continuity-based RD, we simply run model 1 but place the covariates as the LHS variable. Table F2 and F3 summarizes the result. The fifth and sixth rows provide the bias-corrected point estimate and robust standard error, respectively. Almost all the point estimates here are no different than zero, suggesting that covariates are balanced within the narrow bandwidth of the cutoff.

## 5.3 Placebo cutoff

If the discontinuity of outcome at the cutoff is genuinely due to treatment, moving the cutoff would not yield a statistically significant point estimate. Otherwise, there is a concern that the running variable is inherently discontinuous. To test this, we

[28]Both of these conditions are also the conditions for null treatment effect in the respective approaches.

moved the cutoff to the left and the right by one point.

Table F4 displays the result. It shows that none of the treatment effects is statistically significant, which suggests that the running variable is not inherently discontinuous at any random points.

## 5.4 Other possible causal factors

The final concern over our results is that the effect might be driven by the fact that the younger cohort of children might simply have a stronger affinity for education or just better in terms of welfare. We provide three responses to this. First, in Table 2 we have shown no discontinuity of education attainment at the cutoff for the full sample. If the younger cohort simply likes education more, then there would be discontinuity, even at any point in the running variable. We only discover discontinuity in educational attainment at the cutoff when we perform separate estimations for boys. Second, in Subsection 5.2, we have demonstrated that there is no difference in socioeconomic status (as proxied by multiple covariates) between the treatment and control group. Again, if the younger cohort is just blessed with a wealthier family or better economic conditions in general, then there would be a discontinuity at the cutoff for the socioeconomic status covariates. Finally, the placebo cutoff test in Subsection 5.3 has also shown that the running variable is not inherently discontinuous at any point. If any other causal factors were at play alongside the reform affecting the younger cohort, then taking any arbitrary cutoff would result in a treatment effect. This is clearly not the case here.

To completely rule out the possibility of any unobservables associated with being younger driving the treatment effect, we add age as a covariate in the main estimation and report the results in columns 3 and 4 in Table F5. If being younger is all that matters, then age should completely absorb all of the variation in outcome explained by the treatment status. Table F5 clearly shows that it is not the case here, as the point estimates of treatment effect in any specifications barely change from the main estimation after adding age as a covariate.

Another possible concern is that raising the minimum working age is irrelevant because the enforcement program, the National Action Plan, which began a year before the law, is the one driving the effect. To cater to this criticism, we test the impact of the National Action Plan alone by redefining the cutoff to August 2002.

Columns 1 and 2 in Table F5 present the result. First, we are not able to run a randomization-based RD in this exercise because the window selection procedure failed to find a single window in which the covariate is balanced. This indicates that the cutoff at this point is not exogenous. None of the treatment estimates from the continuity-based RD approach is statistically significant. This suggests that while the law needs enforcement to succeed, enforcement programs without the formal support of the law will not deliver any effect either. We are only able to observe the impact of the reform after the law that supports the enforcement program is in place.

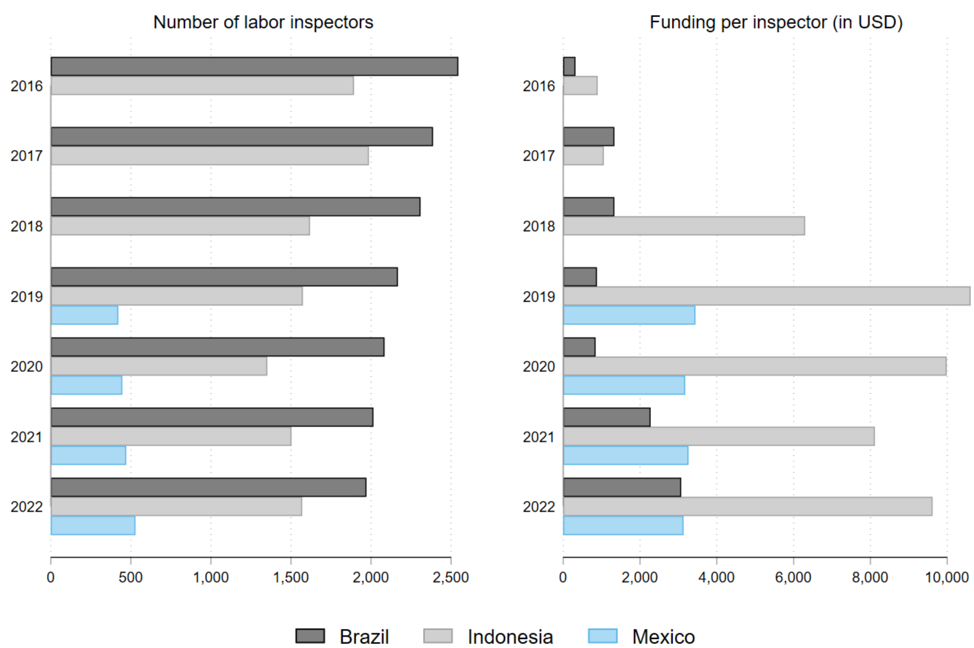
# 6 Discussion

The key learning from this study for future reforms is that aligning regulation with enforcement and support programs that anticipate unintended consequences is crucial to the success of a reform. We have shown that a child labor ban can still be an effective tool to combat child labor when supported by enforcement and support programs to minimize unintended harm. Conversely, a national program requires a strong mandate cemented by the law to deliver impact. A mismatch between the two undermines the effectiveness of the efforts as a whole. In our study context, we find no impact of the reform when only considering the national support program and ignoring the law.

The effect size of a higher minimum working age on child labor incidence in this study is nontrivial. Kozhaya and Martinez Flores (2022) and Bargain and Boutin (2021) only find treatment estimates around 1 pp, and the latter's estimate is statistically insignificant. Coefficient size by Piza and Souza (2017) comes closest to this study as they found a reduction in child labor by four percentage points, although only for boys. Our result stands in contrast to Boockmann (2010) and Edmonds and Shrestha (2012), which found a limited impact of child labor ban, and Bharadwaj et al. (2020), which found a negative impact of child labor ban. We conduct a simple qualitative cross-country analysis to better understand why this is the case.

The possible explanation might come down to the enforcement program underlying the regulation. Despite being far from ideal, Indonesia seems to be one of the few ILO ratifiers that invested in and installed comparatively more comprehensive enforcement programs (CEACR, 2016). Bharadwaj et al. (2020) discusses how labor inspectors in India are susceptible to bribery, which diminishes their effectiveness. Labor inspection in Indonesia is not flawless, but a number of studies have yet to report the same issue of corruption.[29]

Figure 6: Comparison of labor inspection capacity




Source: Compiled from Bureau of International Labor Affairs (2017a,b, 2019a,b, 2020, 2021a,b, 2022b,c,d).[30]

To provide some further illustration, Figure 6 depicts the labor inspection capacity comparison between the countries where the impact of child labor reform has been evaluated. One of the most frequently raised issues about the Indonesian labor

[29]See Amengual and Chirot (2016); Pujiastuti et al. (2023); Santoso (2018); Warnecke and De Ruyter (2012); Yo'el and Anshori (2019) for studies on Indonesian labor inspection system.

inspection system is the problem of understaffing (Santoso, 2018). Yet compared to Mexico, where Kozhaya and Martinez Flores (2022) has found a positive but moderately-sized impact[31], Indonesia has around at least thrice the number of labor inspectors. Indeed, the size of the labor inspector personnel in Indonesia is dwarfed by Brazil, where Bargain and Boutin (2021) found a null impact of the higher minimum working age policy despite its widely acclaimed labor inspection system (Abras et al., 2018). However, Indonesia also edges Brazil in terms of inspectorate funding by a substantial margin.

Throughout program implementation, Indonesia also benefited from generous support and funding from the international donor network (Ministry of Manpower and Transmigration, 2015; Sekretariat KAN-PBPTA, 2008). Studies have also shown how labor inspectors from the ILO Better Work program complement the tasks of the Indonesian labor inspectorate (Amengual and Chirot, 2016; Hardy et al., 2016).

To make the point more salient, we return to Figure 3 that visualizes the geographical distribution of the Action Plan local committee by the end of the first phase (2007). 54% of local governments in Indonesia had already established local committees on the matter of child labor in the first few years of program implementation. 67% of the observations in this study's data lived in these early enforcement regions during their childhood. This speaks to the breadth and the relative expediency of the enforcement program rollout. This study reinforces the view that in a decentralized government setting, the success of a national program highly depends

[31] In 2020, Bureau of International Labor Affairs (2020) praised Mexico for "significant advancement" in the reduction of worst forms of child labor.

on the local administration's effectiveness and prioritization.

Finally, to demonstrate the credibility of the enforcement program, Figure A1 depicts the Google Trends of various enforcement-related keywords. While it is not a perfect proxy, it is clear that interest in the information around enforcement soared and peaked shortly after the reform.

It is worth noting that, other than India, other countries, including Indonesia, implement child labor bans complemented by support programs for families who might lose out on economic opportunities from sending their children to work. As summarized in Bureau of International Labor Affairs (2022a,b,c,d), Brazil, Mexico, and Indonesia utilize a cash transfer program as one of the tools to suppress child labor. No similar scheme is listed for India.

Viewed through this lens, we have a little more understanding of why we only observe the negative impact of the child labor ban in India. As Basu and Van (1998) and Basu (2005) have theorized, a child labor ban may increase child labor incidence when households try to compensate for the lower child wages induced by the ban by supplying more child labor. In this situation, an economic support program such as a cash transfer can ameliorate the unintended damage of a child labor ban on poor households and curb their incentive to send their children to work.

There are plenty of future research avenues to embark upon. We highlight four possible ideas. First, while we've argued that this policy mitigates the unintended economic harm to households, it is worth reconfirming if it is really the case through a more formal empirical exercise. Our study setup limits us to attacking that question, as households with more than one child can be either on the left or right of the cutoff. Second, since our study focuses on child labor on the intensive margin (whether they work), future studies need to examine the extensive margin (how much they work). Such an evaluation will only be enabled by more detailed time-use data. Finally, our heterogeneity analysis has hinted at the possibility of a displacement effect in late enforcement areas and informal work. Future studies might want to look at whether a spatially heterogeneous enforcement regime leads to a fall in child labor incidence in some areas and a rise in others.

# 7 Conclusion

The incentive of agents has always been the central theme in economics. It determines how subjects react to the policy, making it an essential element that will influence policy success. We study a reform in Indonesia that showcases this assertion.

This study exploits the discontinuity caused by the raising of the minimum working age in Indonesia's 2003 Manpower Act to identify the causal effects of child labor reform. When Indonesia raised the minimum working age to 18 in March 2003, those who had not turned 18 at that time were exposed to a higher degree of legal protection from child labor exploitation compared to the previous cohort of children.

Using the Regression Discontinuity Design and the age of individuals at the time of the policy reform as the running variable, we find that the reform leads children to better adulthood to a certain extent. Children who entered adulthood after the reform are less likely to have worked as children. Our heterogeneity analysis suggests that the reform increased boys' likelihood of attending or graduating from senior high school. Furthermore, we find that the reform makes the younger cohort of children less likely to miss daily activities due to illness when they are already adults. By this channel, the reform seems to have made them more likely to be employed in paid work when they reach adulthood. Yet, we do not find evidence that it helps them acquire better-quality jobs or higher earnings.

We find that the impact is concentrated in regions with early enforcement adoption, consistent with the notion that aligning regulation with enforcement and support programs is crucial for policy success.

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# Appendices

## A Summary statistics

Table A1: Mean value of outcomes

| | Control | Treatment |
|---|---|---|
| Ever worked before the age of 18 | 0.14 | 0.28 |
| Attended senior secondary | 0.41 | 0.60 |
| Graduated senior secondary | 0.39 | 0.53 |
| Attended college | 0.13 | 0.20 |
| Ever missed activities in the past month due to poor health | 0.40 | 0.42 |
| Exhibiting depressive symptoms | 0.21 | 0.28 |
| Working in the past week | 0.80 | 0.65 |
| Working in a formal employment | 0.37 | 0.40 |
| Working with a contract | 0.09 | 0.12 |
| Earnings in the past year | 16.23 | 15.93 |

Note: These are the mean values for the entire sample, not only the sample within the narrow bandwidth.

Table A2: Mean value of covariates

| | Control | Treatment |
|---|---|---|
| Is a male | 0.50 | 0.46 |
| Lived in urban area during childhood | 0.33 | 0.37 |
| Lived in early enforcement region during childhood | 0.68 | 0.65 |
| Main breadwinner during childhood was in formal work | 0.27 | 0.31 |
| House during childhood had access to electricity | 0.49 | 0.86 |
| House during childhood drank filtered water | 0.01 | 0.08 |
| House during childhood had their own toilet | 0.52 | 0.71 |
| Household size during childhood | 6.47 | 5.78 |

Note: These are the mean values for the entire sample, not only the sample within the narrow bandwidth.

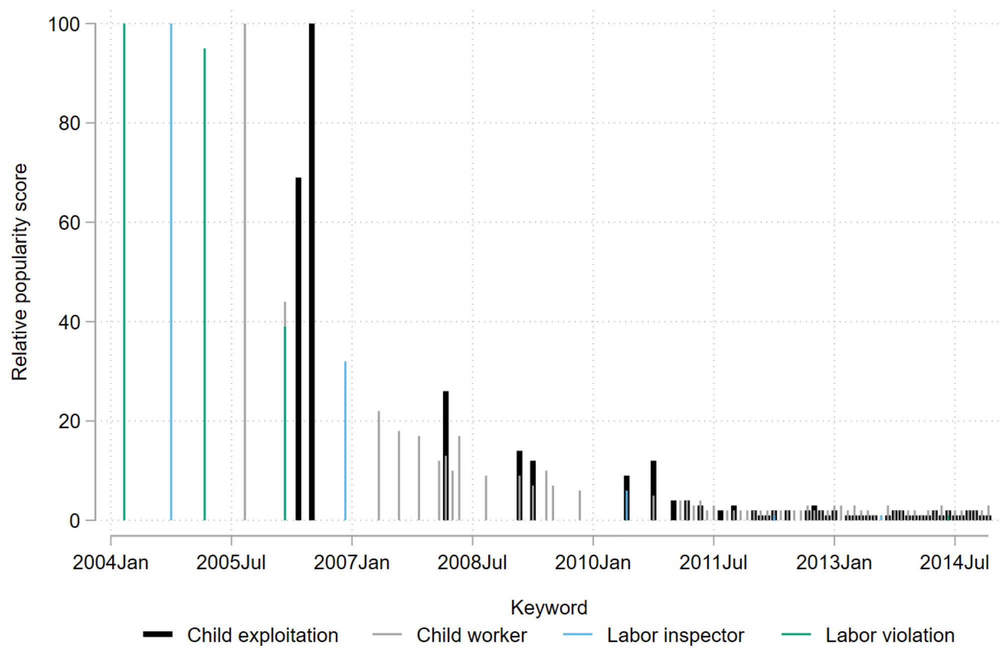
Table A3: Sample size and mass points

| | Control | Treatment |
|---|---|---|
| Number of observations | 18,636 | 9,420 |
| Mass points | 323 | 152 |
| Observation per mass point | 58 | 62 |

Table A4: Age of observations within the bandwidth during the reform

| | (1) Ever work before 18 | (2) Attended SHS | (3) Completed SHS |
|---|---|---|---|
| | Panel A: Linear | | |
| Oldest age of control | 20 | 24 | 21 |
| Youngest age of treated | 16 | 12 | 15 |
| Birth date of oldest control | 10/1982 | 5/1979 | 1/1979 |
| Birth date of youngest treated | 2/1987 | 7/1990 | 11/1990 |
| | Panel B: Quadratic | | |
| Oldest age of control | 23 | 23 | 22 |
| Youngest age of treated | 13 | 13 | 14 |
| Birth date of oldest control | 4/1980 | 9/1979 | 7/1979 |
| Birth date of youngest treated | 8/1989 | 3/1990 | 5/1990 |
| | Panel C: Mean Difference | | |
| Oldest age of control | 19 | 19 | 19 |
| Youngest age of treated | 17 | 17 | 17 |
| Birth date of oldest control | 2/1984 | 2/1984 | 2/1984 |
| Birth date of youngest treated | 9/1985 | 9/1985 | 9/1985 |

Figure A1: Google Trends of various keywords related to enforcement




Source: Google Trends

# B Heterogeneity analysis of main results

Table B1: Heterogeneous effect on main outcomes by gender

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Ever work before 18 | | Attended SHS | | Completed SHS | |
| | Male | Female | Male | Female | Male | Female |
| | | | Panel A: Linear | | | |
| Nonparametric | -0.1541*** | -0.0186 | 0.0742** | -0.0184 | 0.0856** | -0.0305 |
| | ( 0.0468) | ( 0.0327) | ( 0.0335) | ( 0.0304) | ( 0.0400) | ( 0.0374) |
| Parametric | -0.1395*** | -0.0374 | 0.0759*** | -0.0234 | 0.0594* | -0.0240 |
| | ( 0.0339) | ( 0.0297) | ( 0.0288) | ( 0.0262) | ( 0.0333) | ( 0.0299) |
| Bandwidth | 2.2641 | 2.8087 | 4.8289 | 4.8173 | 3.0089 | 3.5767 |
| Eff. Control | 872 | 1,337 | 1,981 | 2,310 | 1,215 | 1,770 |
| Eff. Treatment | 823 | 1,277 | 1,653 | 2,091 | 1,070 | 1,586 |
| Power | 0.9994 | 1.0000 | 1.0000 | 1.0000 | 1.0000 | 1.0000 |
| | | | Panel B: Quadratic | | | |
| Nonparametric | -0.1357*** | 0.0093 | 0.0783* | -0.0331 | 0.0809* | -0.0410 |
| | ( 0.0442) | ( 0.0350) | ( 0.0447) | ( 0.0365) | ( 0.0436) | ( 0.0478) |
| Parametric | -0.1413*** | -0.0038 | 0.0835** | -0.0291 | 0.0726* | -0.0448 |
| | ( 0.0353) | ( 0.0338) | ( 0.0372) | ( 0.0328) | ( 0.0374) | ( 0.0378) |
| Bandwidth | 4.4687 | 3.9971 | 5.2873 | 5.8452 | 4.9429 | 4.5166 |
| Eff. Control | 1,771 | 1,980 | 2,145 | 2,794 | 2,020 | 2,171 |
| Eff. Treatment | 1,551 | 1,736 | 1,810 | 2,499 | 1,710 | 1,996 |
| Power | 0.9997 | 1.0000 | 0.9999 | 1.0000 | 0.9999 | 0.9995 |
| Control | 7,846 | 8,623 | 7,846 | 8,623 | 7,846 | 8,623 |
| Treatment | 3,693 | 4,675 | 3,693 | 4,675 | 3,693 | 4,675 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B2: Heterogeneous effect on main outcomes by employment of breadwinner

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Ever work before 18 | | Attended SHS | | Completed SHS | |
| | Formal work | Not in formal work | Formal work | Not in formal work | Formal work | Not in formal work |
| | | | Panel A: Linear | | | |
| Nonparametric | -0.0620 | -0.0720* | 0.0560 | 0.0073 | 0.0494 | 0.0032 |
| | ( 0.0557) | ( 0.0412) | ( 0.0379) | ( 0.0249) | ( 0.0531) | ( 0.0304) |
| Parametric | -0.0592 | -0.0768** | 0.0398 | 0.0106 | 0.0419 | -0.0002 |
| | ( 0.0387) | ( 0.0326) | ( 0.0305) | ( 0.0224) | ( 0.0389) | ( 0.0255) |
| Bandwidth | 3.1654 | 1.9986 | 5.3084 | 4.8402 | 3.5795 | 2.9891 |
| Eff. Control | 799 | 1,228 | 1,318 | 3,106 | 906 | 1,859 |
| Eff. Treatment | 722 | 1,149 | 1,221 | 2,631 | 819 | 1,731 |
| Power | 0.9550 | 1.0000 | 1.0000 | 1.0000 | 0.9919 | 1.0000 |
| | | | Panel B: Quadratic | | | |
| Nonparametric | -0.0571 | -0.0612 | 0.0524 | -0.0033 | 0.0553 | 0.0023 |
| | ( 0.0652) | ( 0.0374) | ( 0.0616) | ( 0.0284) | ( 0.0598) | ( 0.0349) |
| Parametric | -0.0460 | -0.0909*** | 0.0722 | 0.0047 | 0.0658 | -0.0159 |
| | ( 0.0514) | ( 0.0315) | ( 0.0458) | ( 0.0263) | ( 0.0480) | ( 0.0298) |
| Bandwidth | 4.6452 | 4.6589 | 4.7887 | 5.7865 | 5.5552 | 4.3860 |
| Eff. Control | 1,143 | 2,903 | 1,197 | 3,663 | 1,383 | 2,759 |
| Eff. Treatment | 1,067 | 2,556 | 1,113 | 3,135 | 1,277 | 2,432 |
| Power | 0.8652 | 1.0000 | 0.9544 | 1.0000 | 0.9718 | 1.0000 |
| Control | 4,454 | 12,015 | 4,454 | 12,015 | 4,454 | 12,015 |
| Treatment | 2,632 | 5,736 | 2,632 | 5,736 | 2,632 | 5,736 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B3: Heterogeneous effect on main outcomes by area type

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Ever work before 18 | | Attended SHS | | Completed SHS | |
| | Urban | Rural | Urban | Rural | Urban | Rural |
| | | | Panel A: Linear | | | |
| Nonparametric | -0.0300 | -0.0955** | -0.0061 | 0.0360 | -0.0005 | 0.0257 |
| | ( 0.0469) | ( 0.0375) | ( 0.0306) | ( 0.0330) | ( 0.0299) | ( 0.0437) |
| Parametric | -0.0632 | -0.1036*** | -0.0058 | 0.0346 | 0.0154 | 0.0113 |
| | ( 0.0433) | ( 0.0307) | ( 0.0277) | ( 0.0258) | ( 0.0265) | ( 0.0321) |
| Bandwidth | 2.4482 | 2.2248 | 3.8821 | 4.9771 | 3.1520 | 3.1783 |
| Eff. Control | 709 | 1,240 | 1,151 | 2,954 | 941 | 1,906 |
| Eff. Treatment | 724 | 1,150 | 1,065 | 2,520 | 882 | 1,685 |
| Power | 0.9965 | 1.0000 | 1.0000 | 1.0000 | 1.0000 | 0.9999 |
| | | | Panel B: Quadratic | | | |
| Nonparametric | -0.0380 | -0.0683* | -0.0274 | 0.0334 | -0.0061 | 0.0224 |
| | ( 0.0500) | ( 0.0413) | ( 0.0352) | ( 0.0418) | ( 0.0348) | ( 0.0518) |
| Parametric | -0.0581 | -0.0604* | -0.0073 | 0.0277 | -0.0079 | 0.0240 |
| | ( 0.0447) | ( 0.0340) | ( 0.0346) | ( 0.0342) | ( 0.0303) | ( 0.0391) |
| Bandwidth | 5.0114 | 4.0521 | 4.8319 | 6.0175 | 4.5029 | 4.7743 |
| Eff. Control | 1,472 | 2,404 | 1,425 | 3,587 | 1,324 | 2,836 |
| Eff. Treatment | 1,357 | 2,054 | 1,293 | 2,995 | 1,217 | 2,451 |
| Power | 0.9879 | 0.9999 | 1.0000 | 1.0000 | 1.0000 | 0.9974 |
| Control | 5,278 | 11,191 | 5,278 | 11,191 | 5,278 | 11,191 |
| Treatment | 3,050 | 5,318 | 3,050 | 5,318 | 3,050 | 5,318 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B4: Heterogeneous effect on main outcomes by enforcement regime

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Ever work before 18 | | Attended SHS | | Completed SHS | |
| | Early enforcer | Late enforcer | Early enforcer | Late enforcer | Early enforcer | Late enforcer |
| | | | Panel A: Linear | | | |
| Nonparametric | -0.1283*** | 0.0792 | 0.0423 | -0.0512 | 0.0377 | -0.0487 |
| | ( 0.0293) | ( 0.0589) | ( 0.0258) | ( 0.0595) | ( 0.0274) | ( 0.0596) |
| Parametric | -0.1260*** | 0.0865* | 0.0420* | -0.0194 | 0.0241 | -0.0299 |
| | ( 0.0264) | ( 0.0502) | ( 0.0241) | ( 0.0467) | ( 0.0244) | ( 0.0420) |
| Bandwidth | 2.6963 | 1.8988 | 4.2262 | 2.8848 | 3.4379 | 2.4397 |
| Eff. Control | 1,686 | 440 | 2,704 | 693 | 2,243 | 585 |
| Eff. Treatment | 1,575 | 458 | 2,276 | 705 | 1,937 | 609 |
| Power | 1.0000 | 0.9648 | 1.0000 | 0.9876 | 1.0000 | 0.9872 |
| | | | Panel B: Quadratic | | | |
| Nonparametric | -0.1190*** | 0.0776 | 0.0477* | -0.0681 | 0.0600* | -0.0564 |
| | ( 0.0334) | ( 0.0573) | ( 0.0280) | ( 0.0681) | ( 0.0339) | ( 0.0592) |
| Parametric | -0.1154*** | 0.0056 | 0.0447* | -0.0683 | 0.0508* | -0.0507 |
| | ( 0.0293) | ( 0.0536) | ( 0.0266) | ( 0.0529) | ( 0.0303) | ( 0.0458) |
| Bandwidth | 4.3757 | 4.6714 | 6.1027 | 4.5156 | 3.9534 | 4.6591 |
| Eff. Control | 2,795 | 1,141 | 3,899 | 1,094 | 2,561 | 1,111 |
| Eff. Treatment | 2,383 | 1,107 | 3,257 | 1,092 | 2,150 | 1,107 |
| Power | 1.0000 | 0.9665 | 1.0000 | 0.9562 | 1.0000 | 0.9876 |
| Control | 11,987 | 4,482 | 11,987 | 4,482 | 11,987 | 4,482 |
| Treatment | 5,921 | 2,447 | 5,921 | 2,447 | 5,921 | 2,447 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

# C Effect on specific employment types and sectors

Table C1: List of hazardous occupations

| ISCO68 code | Occupation description |
|---|---|
| 03 | Surveyors, draftsmen, engineering assistants |
| 04 | Aircraft and ship's officer |
| 36 | Transport conductors |
| 58 | Protective service workers |
| 63 | Forestry workers |
| 7X | Production, category 7, but second digit could not be assigned |
| 70 | Production supervisors and general foremen |
| 71 | Miners, quarrymen, well drillers and related workers |
| 72 | Metal processers |
| 73 | Wood preparation workers and paper makers |
| 74 | Chemical processers and related workers |
| 78 | Tobacco preparers and tobacco product makers |
| 8X | Production, category 8, but second digit could not be assigned |
| 80 | Shoemakers and leather good makers |
| 81 | Cabinet makers and related wood makers |
| 82 | Stone cutters and carvers |
| 83 | Blacksmith, tool makers and machine tool operators |
| 84 | Machinery fitters, assemblers, repairers and precision instrument makers (except electrical) |
| 85 | Electrical fitters and related electrical and electronics workers |
| 86 | Broadcasting station, sound equipment operators and cinema projectionists |
| 87 | Plumbers, welders, sheet-metal and structural metal preparers and erectors |
| 88 | Jewelry and precious metal workers |
| 89 | Glass formers, potters and related workers |
| 94 | Production and related workers not elsewhere classified |
| 95 | Bricklayers, carpenters and other construction workers |
| 96 | Stationary engines and related equipment operators |
| 97 | Material handling and related equipment, operators dockers and freight handlers |
| 98 | Transport equipment operators |
| MM | Military |
| M1 | Military Unlabeled 1 |
| M2 | Military Unlabeled 2 |

Table C2: Effect on the probability of working before 18 in certain employment type

| | (1) Paid work | (2) Unpaid work | (3) Formal work | (4) Informal work | (5) Hazardous work |
|---|---|---|---|---|---|
| | | | Panel A: Linear | | |
| Treatment Effect | -0.0506 | -0.0221 | -0.0263 | -0.0114 | -0.0380 |
| | ( 0.0382) | ( 0.0228) | ( 0.0233) | ( 0.0147) | ( 0.0252) |
| Bandwidth | 2.7088 | 2.3493 | 2.5708 | 3.4275 | 2.9707 |
| Eff. Control | 2,339 | 2,014 | 2,158 | 3,091 | 2,607 |
| Eff. Treatment | 2,245 | 1,970 | 2,114 | 2,716 | 2,418 |
| Power | 0.9996 | 1.0000 | 1.0000 | 1.0000 | 1.0000 |
| | | | Panel B: Quadratic | | |
| Treatment Effect | -0.0618 | -0.0130 | -0.0356 | -0.0093 | -0.0098 |
| | ( 0.0378) | ( 0.0238) | ( 0.0238) | ( 0.0195) | ( 0.0294) |
| Bandwidth | 5.5524 | 4.4456 | 5.3736 | 3.6550 | 3.2148 |
| Eff. Control | 4,872 | 3,908 | 4,725 | 3,229 | 2,873 |
| Eff. Treatment | 4,276 | 3,507 | 4,168 | 2,891 | 2,591 |
| Power | 0.9994 | 1.0000 | 1.0000 | 0.9979 | 0.9991 |
| | | | Panel C: Mean Difference | | |
| Treatment Effect | -0.0351 | -0.0280 | -0.0247 | -0.0022 | -0.0186 |
| | ( 0.0227) | ( 0.0191) | ( 0.0171) | ( 0.0122) | ( 0.0182) |
| Left Window | 0.8247 | 0.8247 | 0.8247 | 0.8247 | 0.8247 |
| Right Window | 0.7616 | 0.7616 | 0.7616 | 0.7616 | 0.7616 |
| Eff. Control | 766 | 766 | 766 | 766 | 766 |
| Eff. Treatment | 840 | 840 | 840 | 840 | 840 |
| Power | 0.9996 | 1.0000 | 1.0000 | 1.0000 | 1.0000 |
| Control | 16,469 | 16,469 | 16,469 | 16,469 | 16,469 |
| Treatment | 8,368 | 8,368 | 8,368 | 8,368 | 8,368 |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table C3: Effect on the probability of working before 18 in certain sector

| | (1) Agriculture | (2) Mining | (3) Manufacturing | (4) Services | (5) Construction | (6) Retail |
|---|---|---|---|---|---|---|
| | | | Panel A: Linear | | | |
| Treatment Effect | -0.0317 | -0.0025* | -0.0203 | -0.0042 | -0.0032 | -0.0050 |
| | ( 0.0243) | ( 0.0014) | ( 0.0156) | ( 0.0152) | ( 0.0054) | ( 0.0138) |
| Bandwidth | 2.3434 | 5.8370 | 3.3171 | 2.6168 | 3.3196 | 2.2393 |
| Eff. Control | 2,014 | 5,197 | 2,941 | 2,239 | 2,941 | 1,881 |
| Eff. Treatment | 1,970 | 4,454 | 2,655 | 2,184 | 2,655 | 1,801 |
| Power | 1.0000 | 1.0000 | 1.0000 | 1.0000 | 1.0000 | 1.0000 |
| | | | Panel B: Quadratic | | | |
| Treatment Effect | -0.0203 | 0.0005 | -0.0253 | 0.0008 | -0.0033 | -0.0056 |
| | ( 0.0275) | ( 0.0019) | ( 0.0195) | ( 0.0170) | ( 0.0062) | ( 0.0139) |
| Bandwidth | 4.0611 | 4.5846 | 4.5822 | 4.2288 | 4.9887 | 4.6688 |
| Eff. Control | 3,596 | 4,046 | 4,046 | 3,726 | 4,407 | 4,130 |
| Eff. Treatment | 3,155 | 3,573 | 3,573 | 3,285 | 3,850 | 3,623 |
| Power | 0.9999 | 1.0000 | 0.9997 | 1.0000 | 1.0000 | 1.0000 |
| | | | Panel C: Mean Difference | | | |
| Treatment Effect | -0.0362** | -0.0010 | -0.0145 | -0.0060 | -0.0003 | 0.0003 |
| | ( 0.0183) | ( 0.0010) | ( 0.0120) | ( 0.0115) | ( 0.0049) | ( -0.0117) |
| Left Window | 0.8247 | 0.8247 | 0.8247 | 0.8247 | 0.8247 | 0.8247 |
| Right Window | 0.7616 | 0.7616 | 0.7616 | 0.7616 | 0.7616 | 0.7616 |
| Eff. Control | 766 | 766 | 766 | 766 | 766 | 766 |
| Eff. Treatment | 840 | 840 | 840 | 840 | 840 | 840 |
| Power | 1.0000 | 1.0000 | 1.0000 | 1.0000 | 1.0000 | 1.0000 |
| Control | 16,469 | 16,469 | 16,469 | 16,469 | 16,469 | 16,469 |
| Treatment | 8,368 | 8,368 | 8,368 | 8,368 | 8,368 | 8,368 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

# D Heterogeneity analysis on specific employment types

Table D1: Heterogeneous effect on paid vs. unpaid work by gender

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Did paid work before 18 | | Did unpaid work before 18 | |
| Gender | Male | Female | Male | Female |
| | | Panel A: Linear | | |
| Nonparametric | -0.1052** | -0.0204 | -0.0475 | -0.0033 |
| | ( 0.0498) | ( 0.0331) | ( 0.0424) | ( 0.0206) |
| Parametric | -0.1056*** | -0.0381 | -0.0386 | -0.0102 |
| | ( 0.0375) | ( 0.0268) | ( 0.0284) | ( 0.0192) |
| Bandwidth | 2.4010 | 4.0435 | 2.5819 | 2.4918 |
| Eff. Control | 895 | 1,980 | 1,013 | 1,149 |
| Eff. Treatment | 865 | 1,762 | 929 | 1,155 |
| Power | 0.9941 | 0.9999 | 0.9809 | 1.0000 |
| | | Panel B: Quadratic | | |
| Nonparametric | -0.1042** | -0.0067 | -0.0351 | 0.0083 |
| | ( 0.0517) | ( 0.0393) | ( 0.0430) | ( 0.0227) |
| Parametric | -0.1262*** | -0.0256 | -0.0268 | -0.0079 |
| | ( 0.0404) | ( 0.0336) | ( 0.0325) | ( 0.0218) |
| Bandwidth | 4.4787 | 5.4145 | 4.9525 | 4.1509 |
| Eff. Control | 1,771 | 2,586 | 2,020 | 2,020 |
| Eff. Treatment | 1,551 | 2,328 | 1,710 | 1,801 |
| Power | 0.9822 | 0.9969 | 0.9647 | 1.0000 |
| Control | 7,846 | 8,623 | 7,846 | 8,623 |
| Treatment | 3,693 | 4,675 | 3,693 | 4,675 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D2: Heterogeneous effect on formal vs. informal work by gender

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Did formal work before 18 | | Did informal work before 18 | |
| Gender | Male | Female | Male | Female |
| | | Panel A: Linear | | |
| Nonparametric | -0.0714*** | 0.0117 | -0.0213 | -0.0016 |
| | ( 0.0275) | ( 0.0336) | ( 0.0249) | ( 0.0132) |
| Parametric | -0.0822*** | -0.0043 | -0.0254 | 0.0013 |
| | ( 0.0215) | ( 0.0252) | ( 0.0190) | ( 0.0111) |
| Bandwidth | 3.3539 | 2.5023 | 2.7845 | 3.6353 |
| Eff. Control | 1,364 | 1,184 | 1,093 | 1,770 |
| Eff. Treatment | 1,202 | 1,155 | 1,018 | 1,621 |
| Power | 0.9999 | 0.9962 | 0.9968 | 1.0000 |
| | | Panel B: Quadratic | | |
| Nonparametric | -0.0731** | -0.0006 | -0.0167 | -0.0103 |
| | ( 0.0312) | ( 0.0325) | ( 0.0277) | ( 0.0198) |
| Parametric | -0.0829*** | -0.0105 | -0.0264 | -0.0108 |
| | ( 0.0263) | ( 0.0252) | ( 0.0222) | ( 0.0158) |
| Bandwidth | 4.8938 | 5.5437 | 4.4216 | 3.4847 |
| Eff. Control | 1,981 | 2,633 | 1,771 | 1,694 |
| Eff. Treatment | 1,676 | 2,389 | 1,525 | 1,561 |
| Power | 0.9992 | 0.9979 | 0.9542 | 0.9854 |
| Control | 7,846 | 8,623 | 7,846 | 8,623 |
| Treatment | 3,693 | 4,675 | 3,693 | 4,675 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D3: Heterogeneous effect on hazardous work by gender

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Male | | Female | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0958*** | -0.0484 | 0.0060 | 0.0164 |
| | ( 0.0344) | ( 0.0390) | ( 0.0237) | ( 0.0301) |
| Parametric | -0.1131*** | -0.0808** | 0.0030 | 0.0105 |
| | ( 0.0267) | ( 0.0403) | ( 0.0168) | ( 0.0246) |
| Bandwidth | 2.9337 | 3.0678 | 3.5549 | 4.0267 |
| Eff. Control | 1,168 | 1,215 | 1,728 | 1,980 |
| Eff. Treatment | 1,070 | 1,088 | 1,586 | 1,762 |
| Power | 0.9997 | 0.9975 | 0.9892 | 0.8961 |
| Control | 7,846 | 7,846 | 8,623 | 8,623 |
| Treatment | 3,693 | 3,693 | 4,675 | 4,675 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D4: Heterogeneous effect on paid vs. unpaid work by breadwinner's employment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Did paid work before 18 | | Did unpaid work before 18 | |
| Breadwinner's work | Formal work | Not in formal work | Formal work | Not in formal work |
| | | Panel A: Linear | | |
| Nonparametric | -0.0521 | -0.0587 | -0.0203 | -0.0240 |
| | ( 0.0481) | ( 0.0442) | ( 0.0254) | ( 0.0273) |
| Parametric | -0.0552* | -0.0812** | -0.0144 | -0.0234 |
| | ( 0.0329) | ( 0.0333) | ( 0.0187) | ( 0.0216) |
| Bandwidth | 3.6636 | 2.9684 | 3.1610 | 2.2873 |
| Eff. Control | 935 | 1,859 | 799 | 1,366 |
| Eff. Treatment | 833 | 1,731 | 722 | 1,349 |
| Power | 0.9723 | 0.9981 | 0.9934 | 1.0000 |
| | | Panel B: Quadratic | | |
| Nonparametric | -0.0457 | -0.0486 | -0.0207 | -0.0121 |
| | ( 0.0596) | ( 0.0501) | ( 0.0317) | ( 0.0279) |
| Parametric | -0.0579 | -0.0811** | -0.0227 | -0.0270 |
| | ( 0.0437) | ( 0.0399) | ( 0.0256) | ( 0.0239) |
| Bandwidth | 5.2963 | 4.8557 | 4.2600 | 4.5607 |
| Eff. Control | 1,318 | 3,106 | 1,073 | 2,843 |
| Eff. Treatment | 1,221 | 2,660 | 961 | 2,524 |
| Power | 0.8606 | 0.9865 | 0.9316 | 1.0000 |
| Control | 4,454 | 12,015 | 4,454 | 12,015 |
| Treatment | 2,632 | 5,736 | 2,632 | 5,736 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D5: Heterogeneous effect on formal vs. informal work by breadwinner's employment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Did formal work before 18 | | Did informal work before 18 | |
| Breadwinner's work | Formal work | Not in formal work | Formal work | Not in formal work |
| | | Panel A: Linear | | |
| Nonparametric | -0.0687* | 0.0013 | 0.0265** | -0.0252 |
| | ( 0.0407) | ( 0.0303) | ( 0.0116) | ( 0.0184) |
| Parametric | -0.0659** | -0.0149 | 0.0170 | -0.0253* |
| | ( 0.0281) | ( 0.0252) | ( 0.0120) | ( 0.0143) |
| Bandwidth | 3.2177 | 2.3324 | 2.5488 | 3.7697 |
| Eff. Control | 799 | 1,422 | 625 | 2,442 |
| Eff. Treatment | 743 | 1,349 | 595 | 2,126 |
| Power | 0.9596 | 0.9998 | 1.0000 | 0.9999 |
| | | Panel B: Quadratic | | |
| Nonparametric | -0.0843 | -0.0136 | 0.0347*** | -0.0285 |
| | ( 0.0533) | ( 0.0302) | ( 0.0126) | ( 0.0254) |
| Parametric | -0.0784* | -0.0413 | 0.0354*** | -0.0263 |
| | ( 0.0406) | ( 0.0253) | ( 0.0134) | ( 0.0216) |
| Bandwidth | 4.2659 | 5.1607 | 3.9688 | 3.7072 |
| Eff. Control | 1,073 | 3,305 | 991 | 2,367 |
| Eff. Treatment | 983 | 2,795 | 898 | 2,092 |
| Power | 0.8089 | 0.9998 | 0.9987 | 0.9859 |
| Control | 4,454 | 12,015 | 4,454 | 12,015 |
| Treatment | 2,632 | 5,736 | 2,632 | 5,736 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D6: Heterogeneous effect on hazardous work by breadwinner's employment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Formal work | | Not in formal work | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0585 | 0.0032 | -0.0344 | -0.0225 |
| | ( 0.0400) | ( 0.0501) | ( 0.0280) | ( 0.0319) |
| Parametric | -0.0879*** | -0.0139 | -0.0336 | -0.0285 |
| | ( 0.0295) | ( 0.0447) | ( 0.0213) | ( 0.0293) |
| Bandwidth | 3.3821 | 3.2090 | 3.2047 | 3.7820 |
| Eff. Control | 838 | 799 | 2,074 | 2,442 |
| Eff. Treatment | 782 | 743 | 1,848 | 2,126 |
| Power | 0.9676 | 0.8739 | 0.9996 | 0.9941 |
| Control | 4,454 | 4,454 | 12,015 | 12,015 |
| Treatment | 2,632 | 2,632 | 5,736 | 5,736 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D7: Heterogeneous effect on paid vs. unpaid work by area type

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Did paid work before 18 | | Did unpaid work before 18 | |
| Location | Urban | Rural | Urban | Rural |
| | | Panel A: Linear | | |
| Nonparametric | 0.0067 | -0.0892*** | -0.0285 | -0.0220 |
| | ( 0.0538) | ( 0.0302) | ( 0.0244) | ( 0.0309) |
| Parametric | -0.0074 | -0.0913*** | -0.0199 | -0.0203 |
| | ( 0.0442) | ( 0.0229) | ( 0.0193) | ( 0.0240) |
| Bandwidth | 2.2731 | 5.0060 | 3.4228 | 2.2026 |
| Eff. Control | 667 | 3,009 | 1,036 | 1,240 |
| Eff. Treatment | 673 | 2,520 | 952 | 1,150 |
| Power | 0.9715 | 1.0000 | 0.9992 | 1.0000 |
| | | Panel B: Quadratic | | |
| Nonparametric | -0.0173 | -0.0766* | -0.0129 | -0.0038 |
| | ( 0.0533) | ( 0.0395) | ( 0.0247) | ( 0.0337) |
| Parametric | -0.0604 | -0.0669** | -0.0011 | 0.0019 |
| | ( 0.0461) | ( 0.0318) | ( 0.0213) | ( 0.0267) |
| Bandwidth | 5.1300 | 6.0145 | 6.2496 | 3.9829 |
| Eff. Control | 1,495 | 3,587 | 1,810 | 2,356 |
| Eff. Treatment | 1,384 | 2,995 | 1,652 | 2,015 |
| Power | 0.9467 | 0.9990 | 0.9970 | 0.9996 |
| Control | 5,278 | 11,191 | 5,278 | 11,191 |
| Treatment | 3,050 | 5,318 | 3,050 | 5,318 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D8: Heterogeneous effect on formal vs. informal work by area type

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Did formal work before 18 | | Did informal work before 18 | |
| Location | Urban | Rural | Urban | Rural |
| | | Panel A: Linear | | |
| Nonparametric | -0.0172 | -0.0358 | 0.0159 | -0.0255 |
| | ( 0.0393) | ( 0.0266) | ( 0.0182) | ( 0.0181) |
| Parametric | -0.0215 | -0.0534** | 0.0109 | -0.0265* |
| | ( 0.0299) | ( 0.0212) | ( 0.0142) | ( 0.0141) |
| Bandwidth | 2.6541 | 3.1158 | 3.5342 | 3.5917 |
| Eff. Control | 769 | 1,858 | 1,055 | 2,151 |
| Eff. Treatment | 766 | 1,646 | 992 | 1,848 |
| Power | 0.9827 | 1.0000 | 0.9905 | 0.9999 |
| | | Panel B: Quadratic | | |
| Nonparametric | -0.0112 | -0.0409 | 0.0279 | -0.0297 |
| | ( 0.0430) | ( 0.0282) | ( 0.0223) | ( 0.0245) |
| Parametric | -0.0295 | -0.0407* | 0.0226 | -0.0259 |
| | ( 0.0353) | ( 0.0228) | ( 0.0189) | ( 0.0212) |
| Bandwidth | 4.6346 | 5.7049 | 4.2386 | 3.6843 |
| Eff. Control | 1,340 | 3,373 | 1,233 | 2,200 |
| Eff. Treatment | 1,255 | 2,859 | 1,151 | 1,917 |
| Power | 0.9404 | 0.9999 | 0.9033 | 0.9888 |
| Control | 5,278 | 11,191 | 5,278 | 11,191 |
| Treatment | 3,050 | 5,318 | 3,050 | 5,318 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D9: Heterogeneous effect on hazardous work by area type

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Urban | | Rural | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0307 | 0.0413 | -0.0476* | -0.0384 |
| | ( 0.0335) | ( 0.0420) | ( 0.0256) | ( 0.0292) |
| Parametric | -0.0482* | -0.0005 | -0.0532** | -0.0413 |
| | ( 0.0250) | ( 0.0389) | ( 0.0208) | ( 0.0267) |
| Bandwidth | 3.6668 | 3.2115 | 3.1196 | 3.6656 |
| Eff. Control | 1,102 | 967 | 1,858 | 2,200 |
| Eff. Treatment | 1,009 | 906 | 1,646 | 1,882 |
| Power | 0.9798 | 0.9229 | 1.0000 | 0.9990 |
| Control | 5,278 | 5,278 | 11,191 | 11,191 |
| Treatment | 3,050 | 3,050 | 5,318 | 5,318 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D10: Heterogeneous effect on paid vs. unpaid work by enforcement regime

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Did paid work before 18 | | Did unpaid work before 18 | |
| Enforcement regime | Early enforcer | Late enforcer | Early enforcer | Late enforcer |
| | Panel A: Linear | | | |
| Nonparametric | -0.0846*** | 0.0054 | -0.0519* | 0.0554* |
| | ( 0.0282) | ( 0.0806) | ( 0.0280) | ( 0.0304) |
| Parametric | -0.0860*** | -0.0109 | -0.0521** | 0.0466* |
| | ( 0.0222) | ( 0.0617) | ( 0.0221) | ( 0.0273) |
| Bandwidth | 4.9151 | 2.2042 | 2.5982 | 2.6385 |
| Eff. Control | 3,178 | 537 | 1,607 | 632 |
| Eff. Treatment | 2,633 | 537 | 1,531 | 653 |
| Power | 1.0000 | 0.7065 | 1.0000 | 0.9968 |
| | Panel B: Quadratic | | | |
| Nonparametric | -0.0771** | 0.0231 | -0.0445 | 0.0655* |
| | ( 0.0346) | ( 0.0838) | ( 0.0292) | ( 0.0350) |
| Parametric | -0.0773*** | -0.0173 | -0.0435* | 0.0569* |
| | ( 0.0299) | ( 0.0679) | ( 0.0234) | ( 0.0304) |
| Bandwidth | 5.6418 | 4.3741 | 4.8789 | 4.0865 |
| Eff. Control | 3,587 | 1,057 | 3,124 | 1,003 |
| Eff. Treatment | 3,006 | 1,057 | 2,633 | 964 |
| Power | 0.9999 | 0.6080 | 0.9999 | 0.9793 |
| Control | 11,987 | 4,482 | 11,987 | 4,482 |
| Treatment | 5,921 | 2,447 | 5,921 | 2,447 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D11: Heterogeneous effect on formal vs. informal work by enforcement regime

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Did formal work before 18 | | Did informal work before 18 | |
| Enforcement regime | Early enforcer | Late enforcer | Early enforcer | Late enforcer |
| | Panel A: Linear | | | |
| Nonparametric | -0.0418 | -0.0015 | -0.0111 | -0.0131 |
| | ( 0.0259) | ( 0.0382) | ( 0.0164) | ( 0.0329) |
| Parametric | -0.0511** | -0.0070 | -0.0128 | -0.0154 |
| | ( 0.0210) | ( 0.0334) | ( 0.0130) | ( 0.0222) |
| Bandwidth | 3.2068 | 2.3310 | 3.6614 | 3.6835 |
| Eff. Control | 2,083 | 569 | 2,392 | 910 |
| Eff. Treatment | 1,805 | 559 | 2,006 | 895 |
| Power | 1.0000 | 0.9827 | 0.9998 | 0.8559 |
| | Panel B: Quadratic | | | |
| Nonparametric | -0.0426 | -0.0124 | -0.0144 | -0.0061 |
| | ( 0.0285) | ( 0.0381) | ( 0.0220) | ( 0.0417) |
| Parametric | -0.0579** | -0.0334 | -0.0044 | -0.0128 |
| | ( 0.0238) | ( 0.0331) | ( 0.0186) | ( 0.0316) |
| Bandwidth | 5.1961 | 5.1661 | 3.6268 | 4.8467 |
| Eff. Control | 3,324 | 1,278 | 2,341 | 1,202 |
| Eff. Treatment | 2,805 | 1,211 | 2,006 | 1,147 |
| Power | 0.9999 | 0.9802 | 0.9847 | 0.5906 |
| Control | 11,987 | 4,482 | 11,987 | 4,482 |
| Treatment | 5,921 | 2,447 | 5,921 | 2,447 |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table D12: Heterogeneous effect on hazardous work by enforcement regime

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Early enforcer | | Late enforcer | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0327 | -0.0049 | -0.0474 | -0.0417 |
| | ( 0.0259) | ( 0.0303) | ( 0.0491) | ( 0.0592) |
| Parametric | -0.0428** | -0.0178 | -0.0455 | -0.0538 |
| | ( 0.0201) | ( 0.0284) | ( 0.0348) | ( 0.0526) |
| Bandwidth | 2.9140 | 3.3471 | 3.9885 | 4.2779 |
| Eff. Control | 1,887 | 2,197 | 965 | 1,037 |
| Eff. Treatment | 1,643 | 1,887 | 948 | 1,031 |
| Power | 1.0000 | 0.9990 | 0.7270 | 0.5488 |
| Control | 11,987 | 11,987 | 4,482 | 4,482 |
| Treatment | 5,921 | 5,921 | 2,447 | 2,447 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

# E Heterogeneity analysis on specific sectoral employment

Table E1: Heterogeneous effect on agriculture by gender

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Male | | Female | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0386 | -0.0261 | -0.0287 | -0.0168 |
| | ( 0.0329) | ( 0.0303) | ( 0.0205) | ( 0.0299) |
| Parametric | -0.0172 | -0.0316 | -0.0424*** | -0.0158 |
| | ( 0.0249) | ( 0.0247) | ( 0.0158) | ( 0.0231) |
| Bandwidth | 2.3558 | 4.8599 | 3.3242 | 3.7366 |
| Eff. Control | 895 | 1,981 | 1,619 | 1,817 |
| Eff. Treatment | 865 | 1,676 | 1,485 | 1,651 |
| Power | 0.9998 | 0.9999 | 1.0000 | 0.9964 |
| Control | 7,846 | 7,846 | 8,623 | 8,623 |
| Treatment | 3,693 | 3,693 | 4,675 | 4,675 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E2: Heterogeneous effect on agriculture by breadwinner's employment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Formal work | | Not in formal work | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0282 | -0.0339 | -0.0311 | -0.0124 |
| | ( 0.0272) | ( 0.0412) | ( 0.0322) | ( 0.0347) |
| Parametric | -0.0266 | -0.0208 | -0.0306 | -0.0072 |
| | ( 0.0186) | ( 0.0300) | ( 0.0222) | ( 0.0257) |
| Bandwidth | 4.2872 | 4.6396 | 2.2368 | 4.1002 |
| Eff. Control | 1,073 | 1,143 | 1,328 | 2,621 |
| Eff. Treatment | 983 | 1,067 | 1,289 | 2,289 |
| Power | 0.9540 | 0.6456 | 0.9999 | 0.9989 |
| Control | 4,454 | 4,454 | 12,015 | 12,015 |
| Treatment | 2,632 | 2,632 | 5,736 | 5,736 |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table E3: Heterogeneous effect on agriculture by area type

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Urban | | Rural | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0023 | 0.0005 | -0.0476 | -0.0310 |
| | ( 0.0188) | ( 0.0215) | ( 0.0382) | ( 0.0431) |
| Parametric | 0.0007 | 0.0041 | -0.0324 | -0.0250 |
| | ( 0.0138) | ( 0.0174) | ( 0.0263) | ( 0.0314) |
| Bandwidth | 3.5253 | 5.2639 | 2.3798 | 4.0843 |
| Eff. Control | 1,055 | 1,534 | 1,326 | 2,454 |
| Eff. Treatment | 992 | 1,428 | 1,272 | 2,054 |
| Power | 0.9990 | 0.9936 | 0.9985 | 0.9854 |
| Control | 5,278 | 5,278 | 11,191 | 11,191 |
| Treatment | 3,050 | 3,050 | 5,318 | 5,318 |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table E4: Heterogeneous effect on agriculture by enforcement regime

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Early enforcer | | Late enforcer | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0543** | -0.0472* | 0.0370 | 0.0452 |
| | ( 0.0267) | ( 0.0286) | ( 0.0301) | ( 0.0380) |
| Parametric | -0.0473** | -0.0578*** | 0.0332 | 0.0378 |
| | ( 0.0189) | ( 0.0216) | ( 0.0265) | ( 0.0305) |
| Bandwidth | 2.5623 | 4.5552 | 2.5473 | 3.7230 |
| Eff. Control | 1,547 | 2,877 | 611 | 910 |
| Eff. Treatment | 1,485 | 2,481 | 629 | 895 |
| Power | 1.0000 | 0.9998 | 0.9987 | 0.9679 |
| Control | 11,987 | 11,987 | 4,482 | 4,482 |
| Treatment | 5,921 | 5,921 | 2,447 | 2,447 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E5: Heterogeneous effect on mining by gender

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Male | | Female | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0060* | 0.0007 | 0.0004 | 0.0006 |
| | ( 0.0033) | ( 0.0044) | ( 0.0005) | ( 0.0007) |
| Parametric | -0.0073** | 0.0009 | 0.0003 | 0.0006 |
| | ( 0.0031) | ( 0.0041) | ( 0.0003) | ( 0.0006) |
| Bandwidth | 6.1097 | 4.6276 | 4.0747 | 5.9236 |
| Eff. Control | 2,501 | 1,830 | 1,980 | 2,835 |
| Eff. Treatment | 2,045 | 1,595 | 1,762 | 2,521 |
| Power | 1.0000 | 1.0000 | 1.0000 | 1.0000 |
| Control | 7,846 | 7,846 | 8,623 | 8,623 |
| Treatment | 3,693 | 3,693 | 4,675 | 4,675 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E6: Heterogeneous effect on mining by breadwinner's employment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Formal work | | Not in formal work | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | 0.0019 | 0.0017 | -0.0027 | -0.0008 |
| | ( 0.0018) | ( 0.0022) | ( 0.0020) | ( 0.0024) |
| Parametric | 0.0001 | 0.0052 | -0.0041** | -0.0004 |
| | ( 0.0012) | ( 0.0037) | ( 0.0019) | ( 0.0023) |
| Bandwidth | 3.1547 | 3.0830 | 5.5024 | 5.4805 |
| Eff. Control | 790 | 790 | 3,489 | 3,432 |
| Eff. Treatment | 722 | 702 | 2,962 | 2,962 |
| Power | 1.0000 | 1.0000 | 1.0000 | 1.0000 |
| Control | 4,454 | 4,454 | 12,015 | 12,015 |
| Treatment | 2,632 | 2,632 | 5,736 | 5,736 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E7: Heterogeneous effect on mining by area type

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Urban | | Rural | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0020 | 0.0019 | -0.0019 | 0.0001 |
| | ( 0.0013) | ( 0.0020) | ( 0.0021) | ( 0.0030) |
| Parametric | -0.0018 | 0.0005 | -0.0033 | 0.0025 |
| | ( 0.0015) | ( 0.0007) | ( 0.0020) | ( 0.0029) |
| Bandwidth | 6.3636 | 3.6749 | 4.9388 | 4.3724 |
| Eff. Control | 1,833 | 1,102 | 2,954 | 2,575 |
| Eff. Treatment | 1,690 | 1,009 | 2,520 | 2,240 |
| Power | 1.0000 | 1.0000 | 1.0000 | 1.0000 |
| Control | 5,278 | 5,278 | 11,191 | 11,191 |
| Treatment | 3,050 | 3,050 | 5,318 | 5,318 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E8: Heterogeneous effect on mining by enforcement regime

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Early enforcer | | Late enforcer | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | 0.0012 | 0.0022 | -0.0044 | -0.0046 |
| | ( 0.0010) | ( 0.0014) | ( 0.0049) | ( 0.0058) |
| Parametric | 0.0015 | 0.0032** | -0.0040 | -0.0030 |
| | ( 0.0013) | ( 0.0014) | ( 0.0044) | ( 0.0054) |
| Bandwidth | 2.5855 | 3.9545 | 4.1690 | 4.9832 |
| Eff. Control | 1,607 | 2,561 | 1,022 | 1,229 |
| Eff. Treatment | 1,485 | 2,150 | 990 | 1,176 |
| Power | 1.0000 | 1.0000 | 1.0000 | 0.9976 |
| Control | 11,987 | 11,987 | 4,482 | 4,482 |
| Treatment | 5,921 | 5,921 | 2,447 | 2,447 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E9: Heterogeneous effect on manufacturing by gender

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Male | | Female | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0365** | -0.0419** | -0.0044 | -0.0111 |
| | ( 0.0166) | ( 0.0188) | ( 0.0192) | ( 0.0259) |
| Parametric | -0.0373*** | -0.0357** | 0.0016 | -0.0054 |
| | ( 0.0139) | ( 0.0163) | ( 0.0146) | ( 0.0205) |
| Bandwidth | 3.4205 | 4.9493 | 3.9899 | 4.6558 |
| Eff. Control | 1,397 | 2,020 | 1,938 | 2,216 |
| Eff. Treatment | 1,202 | 1,710 | 1,736 | 2,028 |
| Power | 0.9996 | 0.9973 | 1.0000 | 0.9959 |
| Control | 7,846 | 7,846 | 8,623 | 8,623 |
| Treatment | 3,693 | 3,693 | 4,675 | 4,675 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E10: Heterogeneous effect on manufacturing by breadwinner's employment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Formal work | | Not in formal work | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0285 | -0.0311 | -0.0154 | -0.0226 |
| | ( 0.0234) | ( 0.0334) | ( 0.0165) | ( 0.0199) |
| Parametric | -0.0258 | -0.0362 | -0.0129 | -0.0189 |
| | ( 0.0164) | ( 0.0261) | ( 0.0131) | ( 0.0171) |
| Bandwidth | 4.4414 | 4.9114 | 3.2719 | 4.5227 |
| Eff. Control | 1,107 | 1,220 | 2,126 | 2,843 |
| Eff. Treatment | 1,028 | 1,132 | 1,893 | 2,524 |
| Power | 0.9846 | 0.8351 | 1.0000 | 0.9999 |
| Control | 4,454 | 4,454 | 12,015 | 12,015 |
| Treatment | 2,632 | 2,632 | 5,736 | 5,736 |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table E11: Heterogeneous effect on manufacturing by area type

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Urban | | Rural | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0573** | -0.0597* | 0.0015 | -0.0079 |
| | ( 0.0265) | ( 0.0321) | ( 0.0134) | ( 0.0174) |
| Parametric | -0.0592*** | -0.0651*** | 0.0087 | -0.0095 |
| | ( 0.0190) | ( 0.0241) | ( 0.0113) | ( 0.0151) |
| Bandwidth | 3.4870 | 5.2247 | 3.8709 | 4.0005 |
| Eff. Control | 1,036 | 1,512 | 2,306 | 2,404 |
| Eff. Treatment | 973 | 1,401 | 1,977 | 2,015 |
| Power | 0.9836 | 0.8981 | 1.0000 | 1.0000 |
| Control | 5,278 | 5,278 | 11,191 | 11,191 |
| Treatment | 3,050 | 3,050 | 5,318 | 5,318 |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table E12: Heterogeneous effect on manufacturing by enforcement regime

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Early enforcer | | Late enforcer | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0108 | -0.0161 | -0.0399* | -0.0463 |
| | ( 0.0171) | ( 0.0206) | ( 0.0226) | ( 0.0338) |
| Parametric | -0.0088 | -0.0106 | -0.0296* | -0.0542** |
| | ( 0.0132) | ( 0.0171) | ( 0.0163) | ( 0.0249) |
| Bandwidth | 3.6234 | 4.7820 | 4.1333 | 4.7487 |
| Eff. Control | 2,341 | 3,071 | 1,003 | 1,168 |
| Eff. Treatment | 2,006 | 2,597 | 990 | 1,130 |
| Power | 1.0000 | 0.9997 | 0.9874 | 0.8245 |
| Control | 11,987 | 11,987 | 4,482 | 4,482 |
| Treatment | 5,921 | 5,921 | 2,447 | 2,447 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E13: Heterogeneous effect on services by gender

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Male | | Female | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0207 | -0.0213 | 0.0078 | 0.0144 |
| | ( 0.0250) | ( 0.0281) | ( 0.0129) | ( 0.0141) |
| Parametric | -0.0262 | -0.0259 | 0.0010 | 0.0108 |
| | ( 0.0194) | ( 0.0228) | ( 0.0131) | ( 0.0139) |
| Bandwidth | 3.3151 | 5.3113 | 2.8985 | 4.1373 |
| Eff. Control | 1,322 | 2,145 | 1,384 | 2,020 |
| Eff. Treatment | 1,170 | 1,810 | 1,306 | 1,801 |
| Power | 0.9930 | 0.9529 | 1.0000 | 1.0000 |
| Control | 7,846 | 7,846 | 8,623 | 8,623 |
| Treatment | 3,693 | 3,693 | 4,675 | 4,675 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E14: Heterogeneous effect on services by breadwinner's employment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Formal work | | Not in formal work | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | 0.0118 | 0.0174 | -0.0114 | -0.0061 |
| | ( 0.0198) | ( 0.0223) | ( 0.0196) | ( 0.0232) |
| Parametric | 0.0132 | 0.0171 | -0.0141 | -0.0086 |
| | ( 0.0156) | ( 0.0176) | ( 0.0144) | ( 0.0180) |
| Bandwidth | 2.5941 | 4.4298 | 3.0474 | 4.5001 |
| Eff. Control | 657 | 1,107 | 1,939 | 2,843 |
| Eff. Treatment | 595 | 1,008 | 1,763 | 2,479 |
| Power | 0.9979 | 0.9781 | 0.9999 | 0.9963 |
| Control | 4,454 | 4,454 | 12,015 | 12,015 |
| Treatment | 2,632 | 2,632 | 5,736 | 5,736 |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table E15: Heterogeneous effect on services by area type

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Urban | | Rural | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | 0.0269 | 0.0383* | -0.0166 | -0.0146 |
| | ( 0.0174) | ( 0.0202) | ( 0.0195) | ( 0.0232) |
| Parametric | 0.0220 | 0.0328* | -0.0170 | -0.0156 |
| | ( 0.0151) | ( 0.0182) | ( 0.0147) | ( 0.0185) |
| Bandwidth | 2.2593 | 3.6821 | 3.2364 | 4.7266 |
| Eff. Control | 667 | 1,102 | 1,906 | 2,761 |
| Eff. Treatment | 673 | 1,029 | 1,685 | 2,410 |
| Power | 1.0000 | 0.9981 | 0.9998 | 0.9940 |
| Control | 5,278 | 5,278 | 11,191 | 11,191 |
| Treatment | 3,050 | 3,050 | 5,318 | 5,318 |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table E16: Heterogeneous effect on services by enforcement regime

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Early enforcer | | Late enforcer | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0067 | -0.0025 | -0.0025 | 0.0022 |
| | ( 0.0162) | ( 0.0175) | ( 0.0258) | ( 0.0323) |
| Parametric | -0.0098 | -0.0065 | -0.0079 | -0.0098 |
| | ( 0.0126) | ( 0.0149) | ( 0.0186) | ( 0.0264) |
| Bandwidth | 2.5767 | 4.3511 | 3.9801 | 4.9125 |
| Eff. Control | 1,607 | 2,795 | 965 | 1,229 |
| Eff. Treatment | 1,485 | 2,383 | 948 | 1,159 |
| Power | 1.0000 | 1.0000 | 0.9704 | 0.8851 |
| Control | 11,987 | 11,987 | 4,482 | 4,482 |
| Treatment | 5,921 | 5,921 | 2,447 | 2,447 |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table E17: Heterogeneous effect on construction by gender

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Male | | Female | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0066 | -0.0080 | -0.0003 | -0.0000 |
| | ( 0.0119) | ( 0.0135) | ( 0.0012) | ( 0.0019) |
| Parametric | -0.0022 | -0.0092 | -0.0002 | -0.0005 |
| | ( 0.0090) | ( 0.0112) | ( 0.0014) | ( 0.0008) |
| Bandwidth | 3.3293 | 5.4393 | 5.3632 | 3.2392 |
| Eff. Control | 1,364 | 2,205 | 2,554 | 1,587 |
| Eff. Treatment | 1,170 | 1,868 | 2,328 | 1,446 |
| Power | 0.9986 | 0.9900 | 1.0000 | 1.0000 |
| Control | 7,846 | 7,846 | 8,623 | 8,623 |
| Treatment | 3,693 | 3,693 | 4,675 | 4,675 |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table E18: Heterogeneous effect on construction by breadwinner's employment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Formal work | | Not in formal work | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0129 | -0.0160 | 0.0011 | 0.0015 |
| | ( 0.0120) | ( 0.0136) | ( 0.0066) | ( 0.0072) |
| Parametric | -0.0094 | -0.0133 | 0.0018 | 0.0024 |
| | ( 0.0086) | ( 0.0117) | ( 0.0052) | ( 0.0061) |
| Bandwidth | 3.4764 | 4.5530 | 3.6349 | 6.0349 |
| Eff. Control | 867 | 1,128 | 2,323 | 3,831 |
| Eff. Treatment | 804 | 1,049 | 2,058 | 3,247 |
| Power | 0.9608 | 0.8217 | 0.9999 | 0.9997 |
| Control | 4,454 | 4,454 | 12,015 | 12,015 |
| Treatment | 2,632 | 2,632 | 5,736 | 5,736 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E19: Heterogeneous effect on construction by area type

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Urban | | Rural | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | 0.0032 | 0.0045 | -0.0066 | -0.0082* |
| | ( 0.0115) | ( 0.0117) | ( 0.0043) | ( 0.0049) |
| Parametric | 0.0033 | 0.0076 | -0.0035 | -0.0065 |
| | ( 0.0083) | ( 0.0097) | ( 0.0041) | ( 0.0047) |
| Bandwidth | 3.3356 | 5.9037 | 3.2802 | 4.3853 |
| Eff. Control | 1,016 | 1,701 | 1,949 | 2,575 |
| Eff. Treatment | 935 | 1,575 | 1,720 | 2,240 |
| Power | 0.9947 | 0.9705 | 1.0000 | 1.0000 |
| Control | 5,278 | 5,278 | 11,191 | 11,191 |
| Treatment | 3,050 | 3,050 | 5,318 | 5,318 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E20: Heterogeneous effect on construction by enforcement regime

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Early enforcer | | Late enforcer | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0079 | -0.0092 | 0.0066 | 0.0091 |
| | ( 0.0072) | ( 0.0086) | ( 0.0110) | ( 0.0123) |
| Parametric | -0.0045 | -0.0077 | 0.0020 | 0.0052 |
| | ( 0.0055) | ( 0.0068) | ( 0.0082) | ( 0.0102) |
| Bandwidth | 3.2066 | 4.6139 | 3.9155 | 5.7394 |
| Eff. Control | 2,083 | 2,935 | 965 | 1,405 |
| Eff. Treatment | 1,805 | 2,516 | 932 | 1,335 |
| Power | 0.9999 | 0.9947 | 0.9665 | 0.8717 |
| Control | 11,987 | 11,987 | 4,482 | 4,482 |
| Treatment | 5,921 | 5,921 | 2,447 | 2,447 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E21: Heterogeneous effect on trading and restaurant by gender

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Male | | Female | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0294 | -0.0280 | 0.0122 | 0.0086 |
| | ( 0.0229) | ( 0.0303) | ( 0.0179) | ( 0.0188) |
| Parametric | -0.0313** | -0.0307 | -0.0037 | -0.0022 |
| | ( 0.0147) | ( 0.0209) | ( 0.0188) | ( 0.0178) |
| Bandwidth | 3.0518 | 4.3748 | 2.2311 | 4.9186 |
| Eff. Control | 1,215 | 1,737 | 1,038 | 2,387 |
| Eff. Treatment | 1,088 | 1,525 | 1,013 | 2,116 |
| Power | 0.9937 | 0.8933 | 1.0000 | 1.0000 |
| Control | 7,846 | 7,846 | 8,623 | 8,623 |
| Treatment | 3,693 | 3,693 | 4,675 | 4,675 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E22: Heterogeneous effect on trading and restaurant by breadwinner's employment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Formal work | | Not in formal work | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0189 | -0.0204 | 0.0004 | -0.0060 |
| | ( 0.0176) | ( 0.0211) | ( 0.0184) | ( 0.0181) |
| Parametric | -0.0121 | -0.0104 | -0.0114 | -0.0163 |
| | ( 0.0158) | ( 0.0182) | ( 0.0179) | ( 0.0166) |
| Bandwidth | 3.2667 | 4.2592 | 2.1434 | 4.9536 |
| Eff. Control | 815 | 1,073 | 1,287 | 3,166 |
| Eff. Treatment | 762 | 961 | 1,235 | 2,701 |
| Power | 1.0000 | 0.9985 | 1.0000 | 1.0000 |
| Control | 4,454 | 4,454 | 12,015 | 12,015 |
| Treatment | 2,632 | 2,632 | 5,736 | 5,736 |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table E23: Heterogeneous effect on trading and restaurant by area type

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Urban | | Rural | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | 0.0023 | 0.0085 | -0.0104 | -0.0157 |
| | ( 0.0233) | ( 0.0268) | ( 0.0160) | ( 0.0163) |
| Parametric | -0.0022 | 0.0127 | -0.0211 | -0.0274* |
| | ( 0.0184) | ( 0.0223) | ( 0.0153) | ( 0.0150) |
| Bandwidth | 3.0314 | 4.3986 | 2.2030 | 4.8726 |
| Eff. Control | 915 | 1,277 | 1,240 | 2,901 |
| Eff. Treatment | 863 | 1,200 | 1,150 | 2,483 |
| Power | 0.9968 | 0.9820 | 1.0000 | 1.0000 |
| Control | 5,278 | 5,278 | 11,191 | 11,191 |
| Treatment | 3,050 | 3,050 | 5,318 | 5,318 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E24: Heterogeneous effect on trading and restaurant by enforcement regime

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Early enforcer | | Late enforcer | |
| | Linear | Quadratic | Linear | Quadratic |
| Nonparametric | -0.0292* | -0.0297 | 0.0663*** | 0.0848*** |
| | ( 0.0171) | ( 0.0213) | ( 0.0239) | ( 0.0272) |
| Parametric | -0.0282** | -0.0319* | 0.0608** | 0.0825*** |
| | ( 0.0134) | ( 0.0172) | ( 0.0290) | ( 0.0256) |
| Bandwidth | 3.2635 | 4.1287 | 1.7176 | 3.0807 |
| Eff. Control | 2,136 | 2,665 | 393 | 767 |
| Eff. Treatment | 1,847 | 2,238 | 428 | 742 |
| Power | 1.0000 | 0.9995 | 0.9931 | 0.9715 |
| Control | 11,987 | 11,987 | 4,482 | 4,482 |
| Treatment | 5,921 | 5,921 | 2,447 | 2,447 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

# F Robustness tests

Table F1: Running variable density test

| | Age by reform |
|---|---|
| t-statistic | 0.6156 |
| p-value | 0.5382 |
| Left Bandwidth | 3.8787 |
| Right Bandwidth | 4.6348 |
| Eff. Control | 3,936 |
| Eff. Treatment | 4,122 |
| Control | 18,636 |
| Treatment | 9,420 |

Table F2: Covariate balance within the narrow bandwidth, local linear regression

| | (1) Male | (2) Urban | (3) Early enforcement | (4) Breadwinner formal work | (5) Electricity access | (6) Filtered water | (7) Own toilet | (8) Household size |
|---|---|---|---|---|---|---|---|---|
| Conventional | 0.00468 | -0.00365 | 0.0125 | -0.00844 | -0.0252 | 0.00577 | -0.0275 | -0.0113 |
| | (0.0183) | (0.0208) | (0.0209) | (0.0197) | (0.0186) | (0.00596) | (0.0257) | (0.181) |
| Bias-corrected | 0.00513 | -0.00789 | 0.0211 | -0.00901 | -0.0256 | 0.00826 | -0.0333 | -0.0202 |
| | (0.0183) | (0.0208) | (0.0209) | (0.0197) | (0.0186) | (0.00596) | (0.0257) | (0.181) |
| Robust | 0.00513 | -0.00789 | 0.0211 | -0.00901 | -0.0256 | 0.00826 | -0.0333 | -0.0202 |
| | (0.0220) | (0.0241) | (0.0242) | (0.0240) | (0.0220) | (0.00720) | (0.0307) | (0.222) |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table F3: Covariate balance within the narrow bandwidth, local quadratic regression

| | (1) Male | (2) Urban | (3) Early enforcement | (4) Breadwinner formal work | (5) Electricity access | (6) Filtered water | (7) Own toilet | (8) Household size |
|---|---|---|---|---|---|---|---|---|
| Conventional | 0.00916 | -0.0104 | 0.0117 | 0.0414 | -0.0282 | 0.00807 | -0.0232 | -0.0932 |
| | (0.0268) | (0.0234) | (0.0281) | (0.0317) | (0.0235) | (0.00778) | (0.0356) | (0.249) |
| Bias-corrected | 0.0139 | -0.0107 | 0.00751 | 0.0522* | -0.0310 | 0.00945 | -0.0167 | -0.164 |
| | (0.0268) | (0.0234) | (0.0281) | (0.0317) | (0.0235) | (0.00778) | (0.0356) | (0.249) |
| Robust | 0.0139 | -0.0107 | 0.00751 | 0.0522 | -0.0310 | 0.00945 | -0.0167 | -0.164 |
| | (0.0305) | (0.0261) | (0.0321) | (0.0354) | (0.0266) | (0.00924) | (0.0402) | (0.285) |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table F4: Placebo cutoff test

| | (1) Cutoff: 1 | (2) Cutoff: -1 |
|---|---|---|
| | Panel A: Linear | |
| Treatment Effect | -0.0220 | 0.0091 |
| | ( 0.0449) | ( 0.0341) |
| Bandwidth | 2.5017 | 2.3847 |
| Eff. Control | 2,090 | 2,177 |
| Eff. Treatment | 1,931 | 2,050 |
| Power | 0.9990 | 1.0000 |
| | Panel B: Quadratic | |
| Treatment Effect | -0.0038 | 0.0123 |
| | ( 0.0476) | ( 0.0350) |
| Bandwidth | 4.5578 | 4.6414 |
| Eff. Control | 4,009 | 4,118 |
| Eff. Treatment | 3,419 | 3,736 |
| Power | 0.9979 | 1.0000 |
| | Panel C: Mean Difference | |
| Treatment Effect | -0.0547 | 0.0393 |
| | ( 0.0415) | ( -0.0511) |
| Left Window | 0.5945 | 1.2411 |
| Right Window | 1.3425 | -0.8247 |
| Eff. Control | 349 | 227 |
| Eff. Treatment | 382 | 271 |
| Power | 0.9990 | 1.0000 |
| Control | 17,326 | 15,624 |
| Treatment | 7,511 | 9,213 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F5: Sensitivity analysis

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Cutoff in enforcement program | | Adding age as a covariate | |
| | Linear | Quadratic | Linear | Quadratic |
| Treatment Effect | 0.0145 | 0.0013 | -0.0699** | -0.0613** |
| | ( 0.0294) | ( 0.0285) | ( 0.0315) | ( 0.0310) |
| Bandwidth | 2.0206 | 4.6231 | 2.1391 | 4.6869 |
| Eff. Control | 1,801 | 4,164 | 1,814 | 4,130 |
| Eff. Treatment | 1,643 | 3,536 | 1,725 | 3,684 |
| Power | 1.0000 | 1.0000 | 1.0000 | 1.0000 |
| Control | 16,031 | 16,031 | 16,469 | 16,469 |
| Treatment | 8,806 | 8,806 | 8,368 | 8,368 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$