Essays in Applied Economics

by

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Declaration and Inclusion of Material from a Prior Thesis

The thesis is submitted to the University of Warwick in support of my application for the degree of Doctor of Philosophy. It has been composed by myself and has not been submitted in any previous application for any degree.

The work presented (including data generated and data analysis) was carried out by the author, even though in the cases outlined below it was based on collaborative research:

Chapter 1	Data provided by the Research, Development and Statistics Directorate of the Home Office and analysis carried out entirely by the researcher.
Chapter 2	Data entirely collected by the researcher and analysis carried out in collaboration with Professor Lavy and Zizhong Yan.
Chapter 3	Data provided by the World Bank and the International Labour Organization, analysis carried out in collaboration with Dr. Julian Messina and Professor Luca Nunziata.

Summary

We live in a world where resources are limited and how we invest them has an impact on the citizens' wellbeing. The goal of this thesis is to provide, through the tools of economic analysis, some insights for the optimal allocation of our resources in three different areas: economics of crime, economics of education and economics of labour.

First, societies aim at lowering crime rates and this is why a great amount of resources is spent in punishing offenders. How effective is punishment in lowering crime rates is still unclear: what are the forms of custody that deter lawbreakers from resuming their life of crime? Through a fuzzy regression discontinuity design, we show that keeping young offenders separate from their older peers and far from an overcrowded environment is beneficial only when rehabilitation is offered.

Second, empowering women and enhancing children's early childhood development are two important objectives that are often pursued by independent policy initiatives in developing countries. Understanding the consequences of exploiting potentially beneficial complementarities in pursuing both aims together can be relevant. Through a quasi-natural experiment we evaluate a program implemented in Quito, Ecuador, that targets both. We find that women who are involved in the education of their children are empowered in different dimensions, as reflected in their higher likelihood to find full-time employment in the formal-sector and in their greater independence in intra-household decision-making. Children's drop-out rates decrease, while school grades and scores on cognitive tests increase, particularly for girls.

Finally, governments can introduce and raise minimum wage levels in order to protect their workers. We want to understand the implications of minimum wages on informal markets in developing countries. By exploiting relative variation in minimum wages across labour market groups within countries we show that a higher minimum wage is associated with a larger self-employment share.

Abbreviations

AVSI	The Association of Volunteers in International Service
CNA	Certified Normal Accommodation
I2D2	International Income Distribution Dataset
ILO	International Labour Organization
PelCa	*Preescolar en la Casa* – Home preschooling program implemented by AVSI
YOIs	Young Offender Institutions
MW	Minimum Wage
NGO	Non-Governmental Organization
RD	Regression Discontinuity
se	Standard Error
WB	World Bank

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Tough on young offenders: harmful or helpful?

ABSTRACT

How harshly should society punish young lawbreakers in order to prevent or reduce their criminal activity in the future? Through a fuzzy regression discontinuity design, we shed light on the question by exploiting two quasi-natural experiments stemming to compare outcomes from relatively harsh or rehabilitative criminal incarceration practices involving young offenders in 1980's in England and Wales. According to our local linear regression estimates, young offenders exposed to the harsher youth facilities are 20.7 percent more likely to recidivate in the nine years subsequent to their custody, and they commit on average 2.84 offences more than offenders who experienced prison. Moreover, they are more likely to commit violent offences, thefts, burglaries and robberies. On the contrary, offenders who were sent to the more rehabilitative youth facilities are less likely to reoffend in the future when compared to offenders sent to prison. We conclude that it is effective to keep young offenders separate from their older peers in prison, but only when they are held in institutions that are not solely focused on punishment.

JEL Codes: K42, Z13

1.1 Introduction

How tough societies should be on young criminal offenders has always been at the centre of a heated debate in history. Currently the answer is still unknown, and the evidence mixed.

On the one hand, tough policies and harsh sentences may have a *general deterrence effect* by discouraging people from embarking on criminal activity. Severe punishment could also have a *specific deterrence effect* by discouraging people who have already undertaken criminal activity from committing new crimes in the future (Galbiati et al. 2014). On the other hand, severe punishment may have instead a negative effect on offenders who are incarcerated, weakening their already fragile links with society, nourishing negative networks, and, as a result, increasing the likelihood of future criminal activity. Furthermore, keeping offenders in custody is expensive for society. In England and Wales for example, "the average annual overall cost of a prison place for 2013-14 was £36,237", but "45% of adults are reconvicted within one year of release" (Bromley Briefings, 2015). Hence, looking for ways in which taxpayers' resources can be spent effectively is important.

Because the subject is difficult to study, and quality evaluations are few, supporters and opponents of tough policies have often based their stances on differing views and personal opinions rather than on empirical, causal evidence.

In this paper we investigate the outcomes of two quasi-natural experiments in incarceration practices that occurred in the 1980's in England and Wales. At the beginning of the decade, offenders younger than 21 who were given a custodial sentence were sent to youth custody and detention centres. At the time, youth custody centres and detention centres in Britain were managed as more punitive facilities than previously had been the case, and, thus, young offenders held there experienced a tougher regime than had been usual. Towards the end of the decade, these tough regimes were abolished and turned into young offender institutions more oriented towards rehabilitation. This change allows us to evaluate the outcomes for young offenders under distinct scenarios, in which offenders experience incarceration in settings that are more punitive or more rehabilitative in nature.

To undertake the analysis, we first consider a sample of young offenders who appeared in court when 20/21 years old and were given a custodial sentence at the beginning of the decade, when these tough regimes were in place. Our first sample includes all the offenders in England and Wales who were born in three randomly sampled weeks in 1963. In total they are 558 young offenders. We observe their criminal records until they are 30 years old. Through a fuzzy regression discontinuity design, we exploit the fact that young offenders who appeared in court when below 21 years old were sent to youth custody centres and detention centres, while young offenders who were 21 or older were sent to prison. Everything else being equal, the only reason why offenders were sent to one of the two different types of custody was the age at court appearance. To capture the effects of the different custodial treatments we exploit the plausibly exogenous variation in the age at which offenders appeared in court which, in turn, determined the type of custody the offender was sentenced to. We compare the future offences of these two groups and find that young offenders who were exposed to the harsher youth facilities are 20.7 percent more likely to recidivate in the nine years subsequent to their custody, they commit on average 2.84 offences more than offenders who experienced prison, and they are also brought to court on average 1.39 times more. The crimes committed by young offenders who were exposed to a harsher regime also appear to be more serious, as suggested by the fact that in the future they are sentenced more often to prison (even though the effect is not significantly different from zero). Moreover, their felonies are not minor, but major crimes, such as violent offences, thefts, burglaries and robberies.

Second, we analyse a cohort of young offenders who appeared in court when 20/21 at the end of the decade. This sample is formed by all the offenders born in four randomly sampled weeks in 1968. In this second group there are 297 young offenders. However, we can observe their future offences only for 2.5 years after their release from custody. Through a second fuzzy regression discontinuity design, we exploit the fact that young offenders who appeared in court when younger than 21 were sent to the new young offender institutions, while young offenders who were 21 or older were sent to adults' prisons. Once again, the choice of sentencing offenders to one of the two types of custody depended only on their age at court appearance.

Thanks to the plausibly exogenous random variation in the age at court appearance we also compare the future outcomes of these two groups. We find that offenders who were sent to the more rehabilitative youth facilities are less likely to reoffend in the future when compared to offenders sent to prison, they commit fewer offences, and they are less likely to be brought to court over a 2.5-year time period, even though all of these effects are not significantly different from zero. Moreover, offenders experiencing the rehabilitative regime are sentenced to custody again 1.28 times less than offenders experiencing prison (significant at 5%), and they are significantly less likely to commit burglaries and robberies.

While prisons do not change much across the decade, the regimes in the youth custody facilities do. This setup allows us to compare the effects of experiencing a milder/harsher custody on recidivism. We conclude that keeping young offenders separate from their older peers in prison is beneficial only if they are not kept in a solely punitive regime.

Our strategy relies on the exogenous variation in the offenders' age at court appearance, which guarantees for the continuity of the conditional expectation of counterfactual outcomes. The ability of agents (offenders, judges, police force) to partially or completely manipulate the age at court appearance would invalidate our identification strategy. If this was the case, we would observe a discontinuity in the density function of the age at court appearance around the threshold. We perform a McCrary test and show that there is no evidence of a discontinuity in the running variable (age at court appearance) around the cut-off in neither of the two cohorts.

Our results are robust to a series of other checks: different estimation techniques (parametric and non-parametric); adding control variables in the estimation; adopting different bandwidths, samples and time windows; testing for discontinuities around the cut-off in pre-treatment variables; testing for discontinuities at points different from the cut-off in the running variable.

The remainder of the paper is organized as follows: in Section 2 we discuss the most relevant empirical literature related to the effects of detention on criminal re-offending. In Section 3 we outline the background of the quasi-natural experiment and the design. In Section

4 we describe the data. In Section 5 we present the empirical strategy and the results. In Section 6 we conduct some robustness checks and in Section 7 we conclude.

1.2 Literature Review

The empirical literature on the general and specific deterrent effects is still scarce (Galbiati and Drago, 2014). The main reason for this research gap is the difficulty in identifying a causal link between custody conditions and crime rates. In most cases, self-selection impedes establishing connections that are anything more meaningful than correlations: the most dangerous criminals are both more likely to be sentenced to harsher custody conditions and to reoffend in the future precisely because they are intrinsically more prone to criminal activity. Therefore, whether higher reoffending rates are driven by harsher custody conditions or by the offenders' higher propensity to recidivate cannot be distinguished.

The difficulty of identification is exacerbated by the challenges in gaining access to data on offenders at the micro level that are necessary to isolate a specific deterrence effect, and to determine the causal link between the harsh conditions of a custodial system and the offenders' propensity to be reconvicted. Moreover, the time span over which offenders are observed is usually short.

The findings from the literature so far are mixed. Among those who find evidence of deterrent effects, Katz, Levitt and Shustorovich (2003) use aggregate data on prison death rates (per state per year) as a proxy for prison conditions, providing evidence of a general deterrence effect; they find a negative relationship between death rates among prisoners and violent and property crime rates in the United States between 1950 and 1990. However, the effect they report is very small (they find elasticities smaller than 0.05.). Hjalmarsson (2009) finds evidence of a specific deterrence effect in examining juveniles (16 years old on average) sentenced to custody in juvenile residential facilities in the State of Washington. Exploiting the discontinuities in punishment in juvenile sentencing, he finds that after 1.5 years incarcerated offenders are 13 percent less likely to reoffend than offenders who were not incarcerated. However, the study only examined juvenile residential facilities in the State of

Washington, and, as a result, the author points out that "it is certainly feasible that incarceration has an exacerbating effect in states other than Washington, which have, for instance, worse prison conditions or educational programs" (Hjalmarsson, 2009). Lee and McCrary (2009) analyse arrests in Florida and take advantage of the more punitive sanctions for offenders who turn 18. They also find support for a specific deterrence effect, but a very small one: when offenders turn 18 and the punishment is harsher (as measured by a higher-than-expected sentence length), crime rates decline by 2 percent.

Another stream of researchers finds the opposite - showing no evidence of either general or specific deterrent effects stemming from harsh treatment - with some cases in literature concluding that harsh treatment increases the likelihood of recidivism, or that more rehabilitative facilities show deterrent effects. Aizer and Doyle (2015) look at a slightly younger population: juvenile offenders, ages 10 to 16. They use randomly assigned judges as an instrumental variable to show that offenders who have been incarcerated are *more likely* to recidivate over a 10-year period. Chen and Shapiro (2007) also find no deterrent effect of a harsher punishment. They observe 949 inmates for three years after release. Exploiting the discontinuities in the assignment rules of prisoners to security levels, they estimate that the offenders incarcerated in higher security prisons are no less prone to being rearrested than offenders in minimum security facilities. Drago and Galbiati (2011) employ the variation in the prison assignment to evaluate the impact of prison harshness (as measured by prison overcrowding and prisoner death rates), and the degree of isolation of a prison on the propensity to recidivate. They conclude that the harshness of Italian prisons actually increases the likelihood of re-offending in the seven months following release. Moreover, Mastrobuoni and Terlizzese (2014) estimate the effects of being exposed to a rehabilitative environment rather than to the usual prison conditions on recidivism over a three-year time window; they find that spending one more year in a rehabilitating prison (instead of one year in a regular prison) lowers the offenders' future likelihood of committing crimes by 10 percent, implying that the deterrence effect is given by the softer punishment rather than the harsher.

The evidence on the specific deterrence effect is mixed mainly due to the difference in punitive treatments, targeted populations and time windows in which offenders are observed: it is hard to draw conclusions from few and diverse studies.

The literature frequently distinguishes between the effects on offenders by their age, and whether they are classified as adults or juveniles. The former are more mature and less likely to change in response to the circumstances. The latter are more vulnerable to the surrounding environment. Malleability is not a desirable or undesirable trait per se: it implies that a young individual who lives in a negative environment is more likely to be negatively affected by it; at the same time, a young individual who lives in an edifying environment is more likely to positively change. How an individual is affected in the context of custody environments might push the individual in one of two directions: either he/she will be damaged and become more likely to reoffend in the future or he/she will not be willing to engage in crime anymore to avoid experiencing custody again.

How offenders respond to the environment when they are 20 or 21 is even more uncertain: individuals at those ages fall into a gap, in that they are not considered juveniles, and yet, at the same time, they are not as mature as adults. There is no study we are aware of that looks at how 20/21 years old offenders respond to harsh prison conditions.

1.3 Background and Design

We will compare the effect of sending a young offender to prison rather than to one of the two types of youth facilities: either the tougher youth custody and detention centres or the educational young offender institutions (YOIs). Because the regimen in prisons did not change much in these years, the comparisons also allow us to say something about the difference between keeping young offenders in establishments oriented towards punishment (youth custody/detention centres) or towards rehabilitation (young offender institutions).

1.3.1 Youth Custody and Detention Centres

The desire to keep young offenders separate from their older peers in the prison environment gained traction at the beginning of the 20th century in England. The idea of focusing on education rather than punishment led to the birth of a new type of youth detention centre: the borstal, an institution initially meant to guard and rehabilitate young offenders. Its name derived from the city where the first centre was opened in 1902: Borstal, Kent, England.

In 1952 detention centres were also opened to "provide a sanction for those who could not be taught to respect the law by such milder measures as fines, probation and attendance centres, but for whom long-term residential training was not yet necessary or desirable…" (Walker, 1965).

In the first decades borstals appeared to be successful. Despite their initial success, across the years, borstals did not adapt to the new, more criminally sophisticated generations, and 70 percent of the offenders released from borstals were reconvicted within two years (Warder and Wilson, 1973). More generally, crime rates, particularly among youths, rose in the 1970s, and the public attitude toward young offenders became more concerned with punishment (Pyle and Deadman, 1994).

Hence, in 1979 the conservative party pushed for the implementation of a "short, sharp shock" on young offenders in detention centres. "The theory was that if a young man who was convicted of a first crime was given a short period of intense regimented activity from morning till night, with everything done 'at the double', the experience would give him such a shock that he would give up any idea of a life of crime" (Coyle, 2005). The life in detention centres during the "short sharp shock" became tough, mainly as a result of the isolation it produced:

> "Two visits were permitted each month and new arrivals were entitled to a mere 30 minutes, increasing to 45 minutes and then to an hour […]. From this point (5:45 a.m.), prisoners were under a rule of silence, with commands shouted at them by prison officers. […] By 1pm the prisoners had changed their clothes three times, been inspected twice, marched everywhere and had remained in total silence. The routine continued throughout the day. At 8 p.m., following a lengthy period spent in isolation in their cells, prisoners were allowed 30 minutes' recreation. For five days each week

> prisoners were able to talk to each other for only 30 minutes daily. […] The rule of silence created an atmosphere of mental isolation. At weekends that mental isolation was consolidated by long periods of physical isolation. […] Lining the corridors, awaiting barked instructions, the sullen, pale-faced boys fixed their eyes on their jailers. It was a collective stare of silenced resentment." (Newburn, 2009)

In the same spirit, the 1982 Criminal Justice Act (CJA) abolished borstals and replaced them with youth custody centres. The name of the sentence was changed from "borstal training recommendation" to "youth custody order", reflecting "the view that containment is more appropriate than attempts to rehabilitate via 'training'". The 1982 CJA "for good or ill abandons the notions that young people are sent to penal establishments for treatment or rehabilitation" (Muncie 1984). The institution of the "short, sharp shock" and the replacement of borstals with youth custody centres represented a shift from a welfare policy system targeting rehabilitation towards a justice and retributive system focused on tighter control (Muncie, 2005; Smith, 2007). Anecdotal evidence highlights the suffering that both these centres imposed on young offenders (Muncie 1984; Taylor et al 1979); "(the centres) were, if anything, more brutal jungles than the adult prisons" (Smith, 1984). The young custody centres were not imposing the "short, sharp shock", but life in these institutions was also tough:

> "If the rule of silence, heavy discipline and limited recreation created conditions of mental and physical isolation in the Detention Centre, the endemic verbal harassment and physical violence in the Young Offenders' Institution *(Youth Custody Centres)* created a climate of fear and aggression. 'Doing time' in either regime was about negotiating and handling punitive conditions created formally (institutionally) and informally (cultural)." (Newburn, 2009)

Magistrates were given the power to choose whether an offender below 21 was to be sent to youth custody or detention centres. However, they were not convinced about the new "short, sharp shock" regime in detention centres (Pilcher and Wagg, 2005), and they preferred to sentence young offenders to youth custody. This led to an increase in the number of young offenders in youth custody centres, and to a lower staff to prisoners' ratio, making the general conditions even more unbearable (Scanlan and Emmins, 1988):

> "Staff were so stretched that inmates were now regularly locked up for 23 hours a day, and control problems were rapidly reaching crisis proportions. […] Since the centres were established the number of assaults on staff had more than doubled and there were now five times as many attacks by inmates against other inmates. Violence, bullying, drug-taking, and solvent abuse were becoming regular features of the system."
>
> Source: "Youth centres' reaching crisis point'", *The Guardian*, May 25, 1985.

In general during the "short, sharp shock" members of staff were often cited in the news for being violent against the offenders:

> "The incident[1] is the latest in a series of disturbing episodes concerning alleged staff mistreatment of youths since the Government introduced the short, sharp shock regime, with its emphasis on discipline, parades and physical activity, at all 18 detention centres in England and Wales last year. […] It seems that assaults on young people in end have become institutionalised and are viewed by some staff as an intrinsic part of the 'short sharp shock'. […] we should not go along the road of cruelty in our prisons and turn out youths who were more aggressive when they came out of custody than they were when they went in".
>
> Source: Ballantyne, Aileen. "Youth centre report criticises discipline", *The Guardian*, Nov 25, 1985.
>
> "Two dossiers containing fresh allegations of assaults by prison officers on youths at 'short sharp shock' detention centres are to be sent to Mr Douglas Hurd, the Home Secretary. They have been prepared by the National Association of Probation Officers and the Children's Legal Centre after several complaints from probation officers and social workers who have come into contact with boys who say they have been slapped and punched at Blantyre House in Kent and Haslar in Hampshire."
>
> Source: Ballantyne, Aileen. "Prison officers punched youth, Hurd told: The practice—and…;" *The Guardian*, Nov 24, 1986.
>
> "Boys (in custody) are alleged to have been punched for forgetting to say "sir", for not knowing their numbers before being given any, and for not running quickly enough. […] "We are talking about people being punched quite forcibly in the stomach, and being given quite hard slaps around the face. I have seen a boy whose lip had been split by a blow." […] The baton they were jumping over had been raised by the instructor just as they had estimated the height of it and had started the jump. They were clipped on the ankles,

[1] An officer who behaved violently towards a youth in custody.

> and the baton they were running under was deliberately lowered in the same way so that they were whacked on the back. […] As soon as he arrived, he said, he was subjected to racial abuse and slapped in the face with a ruler. A prison officer then punched him in the stomach and took off his belt and slapped him around his face with it."
>
> Source: Ballantyne Aileen. "Punching inquiry at sharp shock centre", *The Guardian*, Apr 26 1985.

It is in these years that our first quasi-natural experiment takes place. As the 1982 CJA stated, if an offender was to be punished with custody in England and Wales, he/she would have been sentenced to detention/youth custody centres if he/she was below 21 years old and to prison if he/she was 21 or older. Hence, the first comparison that we will make in this paper will be between being sentenced to a normal adults' prison, and being sentenced to a youth custody/detention centre, where the government had decided to be more punitive.

At the time, adults' prisons were just as tough as usual. Inmates in prison, like offenders in youth custody, could be locked up for 23 hours per day, and there were very few intermittent opportunities to work, and "little or no access to educational facilities, recreation or association" (Her Majesty's Chief Inspector of Prisons for England and Wales, 1993).

The main differences that young offenders experienced in prison rather than in youth custody/detention centres were a) the exposure to older peers (from 21 years old onwards) and b) overcrowded cells. As Table 1.1 shows, local prisons could hold up to 150 percent of the population that the facility was originally intended to allow.

1.3.2 Young Offender Institutions

Due to their failure[2], in 1988 the experiments under the "short, sharp shock" regime were abolished, and detention centres and youth custody centres were merged into young offender institutions (YOIs). The rules by which a young offender could be sentenced to a YOI rather than to a prison were the same in 1988 as in 1982: the offender needed to be below 21

[2] Crime rates did not decrease, nor the propensity to recidivate of the criminals who experienced the short, sharp shock.

when convicted of an imprisonable offence, and the court needed to be satisfied that he qualified for a custodial sentence (Scanlan and Emmins, 1988, p. 98).

These rules give us the opportunity for our second quasi-natural experiment.

It is relevant for the purpose of our study to highlight that the new institutions for young offenders were not meant to be tough anymore: at the end of the '80s there was a switch from a punitive system for young offenders towards a rehabilitative system (Coleman and Warren-Adamson, 1992; Muncie, 1990).

The first main differences between YOIs and prisons were that young offenders in prisons were exposed to a) older peers and to b) an overcrowded environment. As Table 1.2 shows, at the end of the '80s local prisons could be filled with 150 percent of the certified normal accommodation, as it used to happen at the beginning of the decade. A further dissimilarity between prisons and YOIs was c) the new educational and rehabilitative target of the latter: the aim of young offender institutions was now "to help offenders to prepare for their return to the outside community." (HC Deb 06 June 1989). The target was to be met by "providing a programme of activities, including education, training and work designed to assist offenders to acquire or develop personal responsibility, self-discipline, physical fitness, interests and skills and to obtain suitable employment after release; fostering links between the offender and the outside community; co-operating with the services responsible for the offender's supervision after release." (CJA 1988, rule 3). Encouraged to maintain their networks with the outside world, young offenders were entitled to send and to receive a letter once a week, and to receive a visit once in four weeks. Outside contacts with persons and agencies were also encouraged.

By contrast, the provision of educational or training opportunities was still low for inmates in prison:

> "[...] for many imprisonment results not only in a loss of liberty in stark conditions but also in the imposition of a regimented and unconstructive way of life. Meals are taken at close intervals during the day, opportunities for socialising can be few and far between, and evening activities and recreation, where they exist at all, are crammed into a few hours with nothing to occupy inmates after lock up. Employment, if it

> exists, can be soulless and unrelated to sentence and needs. In most cases very little is done to prepare prisoners for release and equip them for a life outside." (Her Majesty's Chief Inspector of Prisons for England and Wales, 1993)

Towards the end of the decade the time during which inmates were confined in their cells diminished, but to a much larger extent in YOIs than in prisons. Among all offenders in custody, inmates in open YOIs were forced to stay in their cells for the fewest hours (42 percent of weekend hours, 40 percent on weekdays), while inmates in male local prisons were locked up in their dormitories for approximately 60 percent of their time (with peaks in London of even 83 percent during weekends)[3] (Her Majesty's Chief Inspector of Prisons for England and Wales, 1993).

The number of monitored activities provided in the establishments also differed. For example, 23 out of 35 male and female YOIs in England and Wales offered inmates the option of undertaking agricultural and horticultural work in the open air (HC Deb 30 November 1989, HC Deb 07 November 1991), and, more generally, the largest range of activities (12–15) was provided by YOIs. Table 1.3 shows that the most popular activities were usually either equally likely to be available in both prisons and YOIs or more likely to be offered practiced in the latter[4].

1.4 Data

Data provided by the Research, Development and Statistics Directorate of the Home Office allow for examination of a wide range of variables: gender; ethnicity[5]; the type and number of offences for which the transgressors appeared in court; the length of the sentence they were given, the disposal; whether or not they pleaded guilty; the type of proceedings (e.g.

[3] The study from the Report of a Review of Regimes in Prison Service Establishments in England and Wales is based on 64 prison establishments in England and Wales in 1991/2.
[4] There were few exceptions, mainly related to activities whose availability depended on whether the establishment had the necessary ground to host them (like Farms Party) or to Prison Service Industries and Farms (PSIF) activities, that were not necessarily good quality workshops (Her Majesty's Chief Inspector of Prisons for England and Wales, 1993).
[5] Unfortunately the variable describing the ethnicity of the offenders of the 1963 cohort has a high percentage of missing values.

summoned by police, committed to Crown Court for trial, beach of probation order, etc.); and the date of birth (day, month and year).

We are able to access the offenders' crime records of the first (second) cohort since their birth year until 1993, which means until they are 30 (25) years old. We measure the age at which they commit their first offence to have an indication of their initial propensity to commit a crime.

We construct several outcome variables: the likelihood of being brought to court at least once in the future; the number of offences for which an individual is sent to court; the number of times the offender appears in court again[6]; and the number of sentences to prison. These outcomes refer to different time spans depending on the cohort considered. For the cohort born in 1963, the future time window in which offenders are observed is nine years (or four years after release)[7]. Due to data constraints, we can observe the crime records of the offenders born in 1968 for a shorter time period. In order to maximize the time span after release in which we can observe the offenders born in 1968, we only consider the offenders who are sentenced to custody for one year or less, and we restrict the sample to offenders who turned 20/21 before June 1990[8]. This way we broaden the time window in which we observe the future offences of the second cohort to 2.5 years after release.

[6] Please note that the number of offences for which an individual is brought to court is different from the number of times the individual is brought to court: an offender could be brought to court once for having committed multiple offences. For example, an individual who stole a car and, when escaping, broke a shop window will go to court once but he/she will be sentenced for two different offences.

[7] We reduce the time window in which we analyse the future criminal records of the offenders to nine years (instead of 10) so that the outcomes of the two groups of offenders are comparable: we could observe for 10 years the offenders in our sample who have been sentenced at age 20, but we cannot do the same with the offenders who are sentenced when 21. This is why we choose a time window of nine years to construct our outcome variables. Therefore, we measure the future offences of the offenders who are sentenced when 20 (looking at their outcomes when they are 21 to 29), and we compare them with the future offences of the offenders who are sentenced at 21 (looking at their outcomes when they are 22 to 30). Let us point out that in the nine-year time window we are also considering offenders with a sentence longer than one year, i.e. offenders who are still in custody in this period. However, as we will see later, the sentence length is balanced between offenders assigned to youth custody/detention centres, and offenders assigned to prison, meaning that the time spent in custody by offenders from the two groups is not significantly different, and consequently, should not affect the estimates. As a robustness check we will re-conduct the analysis by looking at the offences committed only in a time window where we can observe all the offenders after release. This time window will necessarily be shorter: four years. As expected, results are perfectly in line with what is found over the nine-year period.

[8] We do this because we can observe offenders until December 1993, and if we limit our sample to offenders who turn 20/21 before June 1990, we can observe them for a longer time period. Otherwise,

We can also observe the type of offences committed in the future: whether they are thefts, violent offences, sexual offences, burglaries/robberies, frauds, criminal damages, drug offences, minor offences or other offences. This way we can have a measure of both the quantity and quality of future crimes.

Our first (second) sample consists of all the offenders who were born in three (four) randomly sampled weeks[9] of 1963 (1968), and who were sent to either youth custody/detention centres (young offender institutions) or adults' prisons in England and Wales when they were 20/21 years old.

The Criminal Justice Act 1982 that established the rules for youth custody and detention centres was implemented on the 24th of May 1983. We therefore include in our first sample only offenders who were 20/21 years old after that date. In total there are 558 offenders[10]. Of them 315 offenders were sent to adults' prisons (our treatment group), and 243 offenders were sent to youth custody/detention centres (our control group). The Criminal Justice Act 1988, which abolished youth custody/detention centres and established YOIs, was implemented on the 1st of October 1988. Following the same reasoning, we include in our sample offenders who appeared in court when 20/21 after that date. In total there are 297 offenders. Of them, 132 were sentenced to adults' prisons (our treatment group), and 165 were sentenced to YOIs (our control group).

Summary statistics of the observable characteristics of offenders from both cohorts are reported in Table 1.4. Most of the offenders born in 1963 (Panel A) are male (93.2 percent), and they appeared in court for the first time when they were almost 17 years old on average. Around 90 percent of them pleaded guilty when 20/21, and they were given a sentence of

we would also observe offenders who turned 20/21 between July and December 1990, but we would examine their post-release behaviour for two years only.

[9] Dates for the 1963 cohort: 3rd-9th March, 28th September-4th October, 17th-23rd December. Dates for the 1968 cohort: 3rd-9th March, 28th September-4th October, 17th-23rd December and 19th-25th June for the 1968 cohort.

[10] We exclude from the 1963 cohort offenders who committed their first crime when they were younger than 14 years old. This way we get rid of the most dangerous criminals, who are more numerous in our control group and consequently might bias our results. In a robustness check, we will re-conduct the analysis in the full sample, including offenders who committed their first crime when younger than 14 years old.

approximately 9.5 months on average. The offences were: burglaries (36.7 percent), violent offences (17 percent), and thefts of different kinds (30.5 percent). Most of the offenders born in 1968[11] (Panel B) are male (97.3 percent) and of White European ethnicity (58.1%). The offences committed by the 1968 cohort are also mainly burglaries (30.7 percent), violent offences (22.6 percent) and thefts (26.4 percent).

1.4.1 Treatment-Control Comparisons: Balancing Tests

We rely on a standard regression discontinuity design assumption, specifically in this case that the assignment to treatment is not correlated to individuals' characteristics other than age. Therefore, we provide visual evidence of whether other covariates exhibit a jump around the threshold. As shown in Appendix Figure A1.1, this is not the case for any of the available observable characteristics: gender, ethnicity, birth year (the members of the groups we compare are all born in the same year), month of birth (March, June[12], September/October, December), whether they pleaded guilty, the type of offence, the age at which they committed their first offence and the proceedings types. The absence of a jump in observable characteristics around the cut-off further supports our analysis.

1.5 Empirical Strategy and Results

1.5.1 Empirical Strategy

The 1963 and 1968 cohorts are analysed separately through a fuzzy regression discontinuity (RD) design. It is a fuzzy RD because not all the offenders who should be sentenced to either prison or separate youth establishments are effectively sentenced to them. That is, 230 (160) offenders out of the 243 (164) who appeared in court when age 20 from the 1963 (1968) cohort were sent to youth custody/detention centres (young offender institutions),

[11] We limit our sample of offenders born in 1968 to offenders who were given a custodial sentence of one year maximum, which makes summary statistics of the 1968 cohort slightly different compared to the 1963 cohort.
[12] June is available only for the 1968 cohort.

and 297 (128) young offenders out of the 315 (132) who appeared in court when age 21 were sent to adults' prisons. This gives us the possibility of estimating the local average treatment effects (LATE) by two-stage least squares (2SLS). The following model illustrates how.

First stage equation:

$$D_i = \alpha + f_1(\tilde{x}_i) + \rho T_i + \eta_i \qquad (1)$$

Second-stage equation:

$$Y_i = \alpha + f_2(\tilde{x}_i) + \gamma D_i + e_i \qquad (2)$$

Where:

Y_i = the outcome for individual *i*, i.e. the likelihood to re-offend in the future, the number of crimes committed, the number of court appearances, the number of sentences to prison, the number of specific types of crime committed;

D_i = the treatment variable, equal to 1 if individual *i* is sentenced to an adults' prison, and 0 otherwise;

T_i = 1 if individual *i* is 21 years old or older, and 0 otherwise; it is used as instrument for D_i.

X_i = age of individual *i* when sentenced, centred so that it is 0 when the individual turns 21 years old, positive if the individual is sentenced when 21 years old or older, and negative when the individual is younger than 21 years old[13].

The functional forms f_1 and f_2 need to be correctly specified.

Our main specification is estimated through a non-parametric approach, implementing a local linear regression constructed with a triangular kernel regression[14]. As Lee and Lemieux

[13] The centred running variable is equal to 1 the day after the offender turned 21 and -1 the day before his 21st birthday.

[14] A triangular kernel is ideal for estimating effects at the boundary (Fan and Gijbels 1996, Lee and Lemieux 2014). Moreover, results (available upon request) are robust to using different kernels, like the uniform or Epanechnikov.

(2010) suggest, it is better not to rely on one method only, so we will also estimate equations (1) and (2) through a parametric approach. To allow for non-linearities, we use polynomials, but up to the second order only. We do not control for higher polynomials (third, fourth, etc.) of the forcing variable because it could lead to misleading results (see Gelman and Imbens (2014)). We also allow the treatment to have a different impact before and after the cut-off by including an interaction of the centred variable and the treatment variable. Finally, for a further robustness check, we also include in our parametric approach estimations control variables such as gender, month of birth, ethnicity, age at which the offender committed the first offence, sentence length, plea, proceedings and type of offence when the offender was sentenced to youth custody/detention centres/young offender institutions or adults' prisons.

1.5.2 Results

For both our cohorts, the first stages are strong: the estimated coefficients in equation (1) are 0.761 for the 1963 cohort and 0.891 for the 1968 cohort (Table 1.5), very precisely estimated. We can visualize the strength of our first stages in Figure 1.1.

1.5.2.1 Prison vs. harsher youth punishment

Let us begin our treatment effects analysis by looking at the future offences of the 1963 cohort through the local linear regression (Table 1.6). In the first column we report the estimated treatment effect when the bandwidth is one year on both sides[15]. In column (2) we present the estimates with the bandwidth suggested by Ludwig and Miller (2007), in column (3) we restrict the bandwidth to ¾ of a year and in column (4) to half a year.

We find that young offenders who experienced custody in prison are 20.7 percent less likely to re-offend than those who were exposed to a harsher treatment over a nine-year time span. The effect is significant and does not change even when we reduce the bandwidth around

[15] By this, we mean that we include in our sample young offenders who appear in court from the date of their 20th birthday up to young offenders who are sentenced the in their 22nd birthday, i.e. +/- 1 year from the threshold of 21.

the cut-off from one year to the optimal bandwidth suggested by Ludwig and Miller (2007) or to ¾ of a year. The effect is no longer significantly different from zero at conventional significance levels only if we reduce the bandwidth to half a year. Hence, young offenders exposed to a harsher punishment are more likely to reoffend, and this is also reflected in the number of future offences they commit over the nine-year period: on average 2.84 offences more than their peers who were subject to less severe incarceration conditions. This is true across all different bandwidths. Not only young offenders who experienced the harsher treatment are more likely to be sentenced for more offences in the future, but they are also brought to court on average 1.39 times more. The two outcomes differ in magnitude because an offender can go to court once and be sentenced for more than one offence at the same court appearance.

We then investigate on the seriousness of the crimes committed in the nine subsequent years. Using the number of future sentences to prison as a proxy for severe crimes, we find that offenders who experience the tougher regime are more likely to be sentenced to prison in the future, but not significantly so. In Table 1.7, we examine the types of crimes committed, and we show that the overall effects we find are not driven by minor offences, but mainly by violent offences, thefts, burglaries and robberies. These differences between the two groups of young offenders are significant even when we restrict the bandwidth as previously detailed[16]. We find no significant differences in the number of future violent crimes (such as sexual offences), or in the number of various other crimes (drug offences, minor offences, motoring offences, frauds). There seems to be an effect on criminal damage too, but it vanishes when we restrict the bandwidth around the threshold.

In summary, on the one side there are overcrowded prisons where offenders are exposed to older peers; on the other side there is a tougher than usual regime, with the main purpose to punish and shock offenders. The overall effects of the latter are more detrimental:

[16] While in the first column we report the estimated treatment effect when the bandwidth is one year on both sides, in column (2) we present the estimates with the bandwidth suggested by Ludwig and Miller (2007), in column (3) we restrict the bandwidth to ¾ of a year and in column (4) to half a year.

offenders who are sentenced to youth custody/detention centres are more likely to re-offend in the future, to commit a greater number of offences and to commit offences that are more dangerous for society. Through this analysis we are not able yet to disentangle the mechanisms that are driving the results.

1.5.2.2 Prison vs. softer youth punishment

We now analyse the future offences of the 1968 cohort, comparing the young individuals who were sent to the usual adults' prisons to the ones assigned to YOIs. As we previously explained, we examine this cohort over a shorter period: 2.5 years after release. We will then re-conduct our analysis for the 1963 cohort limiting the time window to 2.5 years, and limiting the sample to offenders sentenced for one year or less. This way we can compare the results we obtain by analysing the 1963 and 1968 cohorts.

In Table 1.8, Panel B we can see a higher incidence of the number of future felonies, the number of subsequent court appearances, and the likelihood of reoffending among those sentenced to prison compared to those sent to other institutions. In each instance, the magnitude is greater, but not significant. The number of times that former prisoners are sentenced again to custody is positive and significantly different from zero, suggesting that the future offences they commit represent a greater danger for society. If we then consider the types of offences that they commit, we see that young offenders who experienced prison are more likely to commit burglaries and robberies. Let us keep in mind that these results are the opposite of what we found when the treatment for younger offenders was harsher, i.e. for the 1963 cohort, where it is the young offenders kept in youth custody and detention centres who become more dangerous instead. In order to make the comparison more adequate, we now repeat the analysis for the 1963 cohort restricting the sample to offenders sentenced for one year or less and limiting the time window in which we observe their offences to 2.5 years after release. Now that the time window is shorter, the number of future offences considered will necessarily be smaller, but we find that results go in the same direction as over the nine-year period. As shown in Table 1.8, Panel A, young offenders born in 1963 who were sentenced to prison rather than youth custody/detention centres, are 31 percent less likely to reoffend in the 2.5

years following release, they commit on average 1.03 fewer offences, and they appear in court 0.57 times fewer. Hence, it seems that even in the short term, young offenders who experience the harsher treatment become more dangerous for society. All these estimates are significantly different from zero and, as we highlighted before, they go in the opposite direction of what we find once the harsh treatment for young offenders is abolished.

Moreover, similarly to what we found over the nine-year time window, this shorter time window still shows that violent offences and thefts constitute the types of crimes more often committed more often by offenders who experienced youth custody and detention centres (Table 1.9).

In summary, being exposed to (harsher) youth custody/detention centres makes offenders more dangerous than being exposed to prisons; while being exposed to (less harsh) YOIs makes offenders less dangerous than being exposed to prisons. Given that prisons did not experience major changes over the '80s, and given that the differences in the age of peers and in overcrowding rates between prisons and establishments for youth did not change significantly over time, our findings seem to suggest that it is wise to keep young offenders away from prisons, but only if they are kept in institutions with a rehabilitative purpose. If instead, young offenders are kept separate from their older peers and far from an overcrowded environment, but with the aim of punishing them, their likelihood of reoffending in the future is exacerbated.

1.6 Robustness Checks

We now verify whether our local treatment effects are robust to a series of checks. First, we consider whether results are stable across alternative estimation methods: we find that they hold also when the analysis is carried out through a parametric approach up to a second-order polynomial (Table 1.10). Second, in the even columns of Table 1.10 we also add control variables as a further check: gender, sentence length, ethnicity, plea, proceedings, month of birth, type of offence and age at which the offender committed the first offence.

Estimated coefficients tend to appear slightly smaller in size when control variables are included, but they are not significantly different from the coefficients estimated without control variables. In Table 1.11 we show the different treatment effects by offence type, estimated through a parametric approach: effects go in the same direction as through the non-parametric.

One could worry if there were a discontinuity in the distribution of the forcing variable (the age at which offenders go to court) at the threshold (21 years). This would suggest that people (judges, police, the offenders themselves) can manipulate the forcing variable around the threshold. For example, young offenders, knowing ex-ante the harsh conditions of youth custody and detention centres, could wait to commit their crimes until they turn 21 years old. Reassuringly, the McCrary test shows no manipulation of the assignment variable for either cohort (Figure 1.2).

Let us remember that in the analysis of the 1963 cohort we excluded offenders who committed their first offence when younger than 14. We proceeded this way because the age at which offenders committed their first offence was the only unbalanced covariate between treatment and control groups: young offenders who went to youth custody/detention centres were more likely to have committed their first offence when they were younger than their counterparts. Because this difference may bias the results, we re-conduct the analysis for the 1963 cohort with the full sample of offenders, including those who committed their first crime before turning 14 years old. The full sample includes 708 offenders in total. As we might have expected, the magnitude of the treatment effects in the full sample is slightly greater than in our main analysis (Tables 1.13-1.14): young offenders who experienced a tougher punishment commit on average 3.46 offences more (2.84 in our original sample); they are brought to court 1.65 times more (1.39 in our original sample); they are sentenced to prison 1.52 times more (0.92 in our original sample); and they are 18.8 percent more likely to re-offend in the future (20.7 percent in our original sample). All of the treatment effects found are significantly different from zero and remain so even when the bandwidth around the threshold is reduced. Even when we analyse the type of offence committed (Table 1.14), we realize that

young offenders who went to youth custody/detention centres are significantly more likely to commit thefts, violent offences, burglaries and robberies, as we found in our original sample.

In Section 5 we analysed the future felonies of the 1963 cohort over the next nine years, even though over this time some offenders are not free from confinement, but kept in custody. If the sentence length for offenders in youth custody/detention centres and offenders in prisons were different, the main results we presented would be biased, as the number of free people facing the choice of committing (or not) new offences would be disproportionate. However, we have already seen that the sentence length is balanced, meaning that the time spent in custody by offenders from the two groups is not significantly different, and consequently, will not affect the estimates. As a robustness check we re-conduct the analysis by looking at the offences committed only in a time window where we can observe all the offenders outside of custody. The time window that enables us to conduct this analysis is four years[17]. As we can see in Table 1.15, results are perfectly in line with what is found over the nine-year and 2.5-year periods: offenders who have been exposed to prisons rather than to youth custody/detention centres on average commit 1.8 fewer offences in the five years following release (-1.03 in 2.5 years following release, -2.84 in nine years); they are 35.7 percent less likely to commit offences (-31.1 percent in 2.5 years, -20.7 percent in nine years); and they appear in court almost once less (-0. 57 time in 2.5 years, -1.39 in nine years). If we then dig into the type of offences committed, we can see that they are mostly violent offences, thefts and, in this case, also criminal damage.

We also need to bear in mind that the number of offences captured in the analysis underestimates the true level of re-offending because crimes are only partially detected, sanctioned and recorded. Our estimated effects would be biased if there were a difference in

[17] The time window is four years because once we exclude two offenders who have been given a sentence of 60 months, the longest sentence we have in the sample is 48 months, i.e. four years. This means that offenders born at the latest in our sample (i.e. in December 1963) and who are sentenced to prison until they are still 21 (i.e. at the latest December 1985, some days before their 22nd birthday) for the maximum time (i.e. four years from December 1985) will be out of custody in December 1989. As we can observe offenders until December 1993, our time window is four years maximum.

how easy it is to detect, sanction and record the offences of the two groups. However, we do not have any reason to believe there was.

Our first stage is very strong, but as a placebo test we also check if there are other jumps in the forcing variable. Following Imbens and Lemieux (2008) we only look at one side of the discontinuity, take the median of the forcing variable in that side and test for discontinuity. Reassuringly, we find none.

1.7 Conclusion

We use a fuzzy regression discontinuity design to analyse two quasi-natural experiments in criminal sentence of 20- and 21-year-old offenders to compare the effects of incarceration practices that are harsher or more rehabilitative in nature. The work contributes to the literature and current public debate on the most effective type of punishment to reduce crime among young offenders and to protect the citizens' wellbeing.

We find evidence that keeping young offenders separate from their older fellows is efficient when we aim to reduce their future criminal activity. However, this is true only if the young offenders are housed in institutions that provide for their rehabilitation. Keeping young offenders in institutions with a sole punitive purpose proves to be counterproductive instead.

During the '80s, prisons in England and Wales do not experience major changes, while institutions where offenders younger than 21 are held separately from their older peers do: initially these institutions are meant to punish young offenders severely, but in 1988, they adopted a more rehabilitative orientation. We find that young offenders exposed to the temporarily tougher regime are 20.7 percent more likely to re-offend in the subsequent nine years; they commit on average 2.84 offences more; and they are brought to court 1.39 times more often than their counterpart in prison. The crimes that young offenders exposed to a harsher regime commit also appear to be more serious, as suggested by the fact that in the future they are sentenced more often to prison, even though the effect is not significantly different from zero. Moreover, their felonies are not minor, but major crimes, such as violent

offences, thefts, burglaries and robberies. By the end of the decade punitive institutions for young offenders are abolished and substituted with more rehabilitative ones, which enables us to compare young offenders sentenced to the usual prison with young offenders sentenced to the separate educational institutions. In the 2.5 years after release, offenders held in the new educational facilities are sentenced to custody 1.28 times less than offenders kept in ordinary prisons; they are also significantly less likely to commit burglaries and robberies, suggesting that they become less of a threat for their society. They are also less likely to re-offend and they commit fewer crimes in the future, but the estimates of these effects are not significant.

Adults' prisons do not experience major changes over the decade. Moreover, the different exposure to overcrowding and to peers between prisons and establishments for younger offenders stay the same. The only difference between the two types of custody that varies over time is the change of target in institutions for young offenders, from a punitive one to a rehabilitative one. Hence, our results imply that being kept separately from more adult criminals is positive only if the purpose of the offender's custody is rehabilitative. If it is punitive, the lawbreaker becomes even more likely to reoffend in the future.

Our estimates hold to different robustness checks.

These results suggest that the experience of being held in punitive incarceration facilities can have negative long-term consequences on young offenders, and therefore on the entire society. The evidence is significant, with the caveat that it relates to a specific group of offenders: law breakers who are sentenced to custody when 20/21 years old. While being an interesting result per se, it cannot be generalized to juvenile or adult offenders, even though our results are in line with the literature that does not find evidence in favour of a specific deterrence effect for juveniles (Aizer and Doyle 2015) and adult offenders (Chen and Shapiro 2007, Drago and Galbiati 2011, Mastrobuoni and Terlizzese 2014).

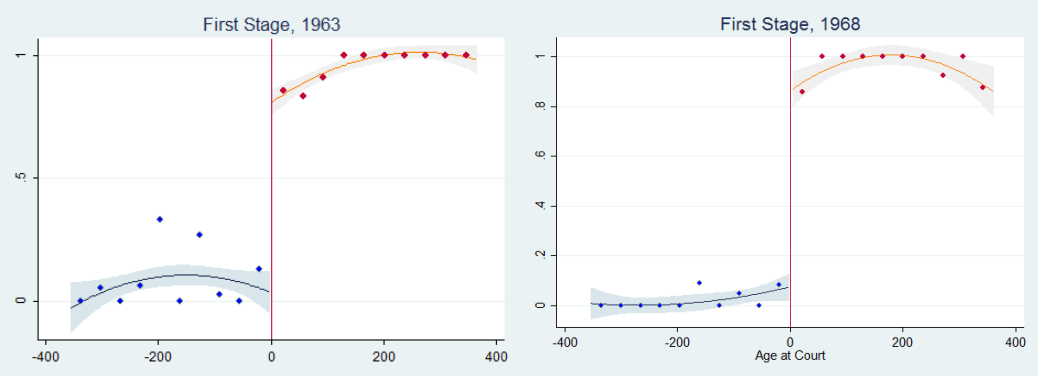
Other two caveats need to be kept in mind for policy implications. First, we cannot infer anything about unreported crimes, which we know exist, but which we cannot measure by definition. If the number of unreported crimes was different between the groups we compare, our results would be biased, but we do not have any reason to believe so. Moreover,

the aim of our paper is to test for the presence of a specific deterrence effect, but we cannot draw any conclusion on the general deterrence effect: we do not know how other individuals who did not experience youth custody, detention centres, young offender institutions or adults' prisons when 20/21 respond to the existence of these institutions.

Finally, more research on the mechanisms behind these effects would be beneficial for a better understanding of what are the drivers of the offenders' behaviour and tailor appropriate policy responses.

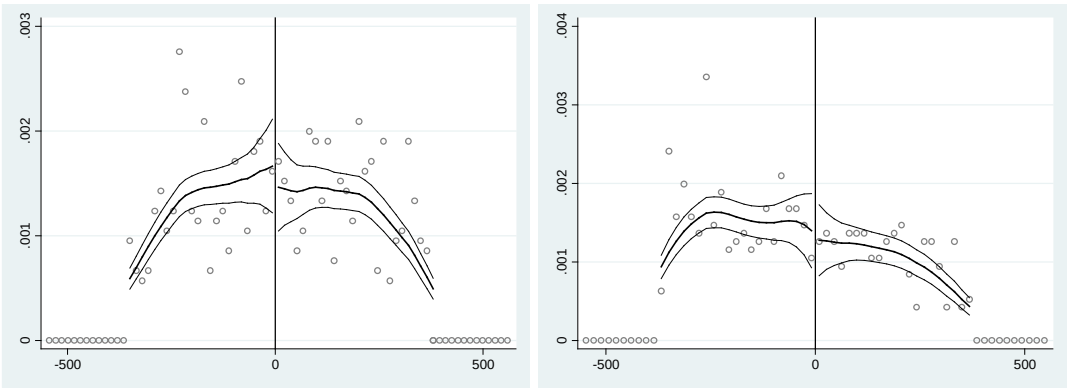
Figures and Tables

Figure 1.1. First Stage (20 bins)



Notes: The figure above reports the first stages, i.e. how much of being sentenced to an adults' prison depends on actually being 21. The left (right) hand side refers to the 1963 (1968) cohort of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes all the offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance. The 1968 sample includes all the offenders who were sentenced to young offender institutions or adults' prisons when being age 20/21 at the date of court appearance. On the x axis lies our running variable, age at court appearance, centred at 0 when age at court appearance is 21. Age at court appearance is positive (negative) when young offenders are older (younger) than 21. On the y axis the treatment dummy (equal to 1 when the offender is sentenced to prison) is plotted. The coloured areas represent the 90% confidence intervals around the separate lines of quadratic best fit plotted on the left and right hand side of the cut-off.

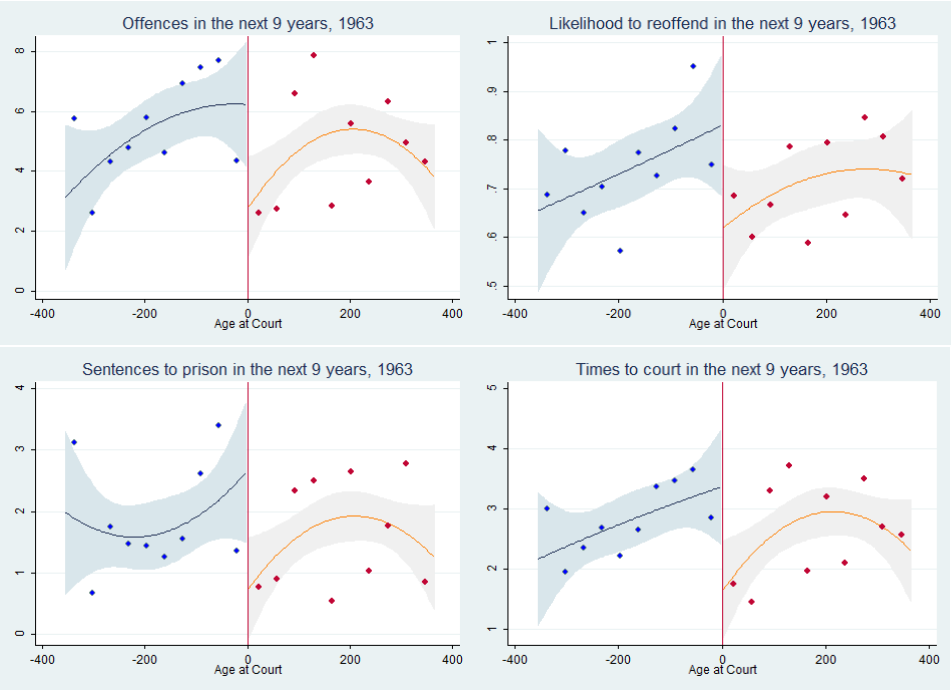
Figure 1.2. McCrary Test



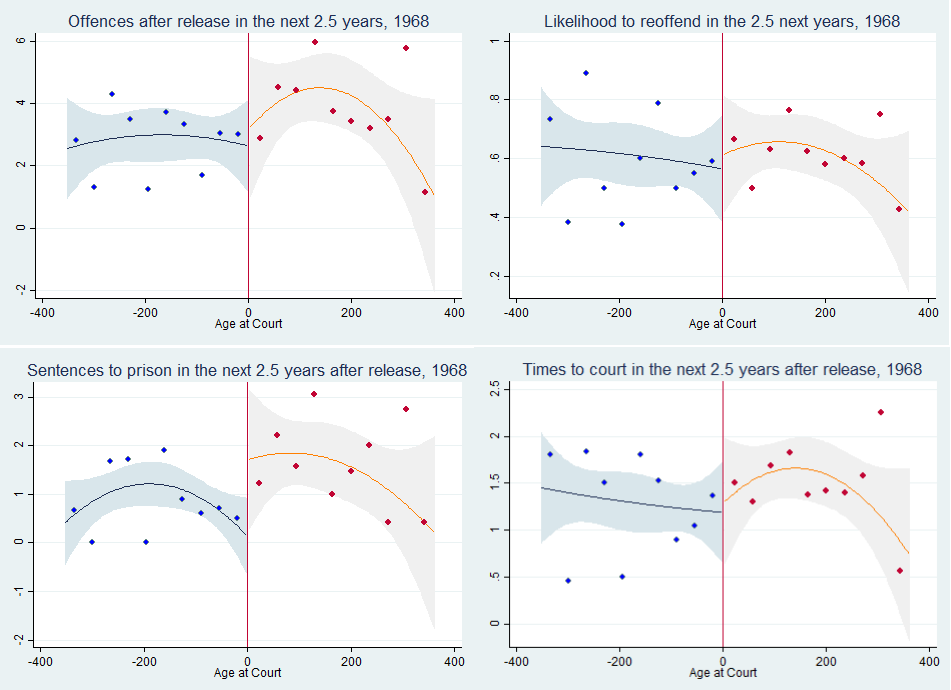
Notes: The figure above refers to the 1963 (Panel A) and 1968 (Panel B) cohorts of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes all the offenders who were sentenced to either youth custody/detention centres or adults' prisons when 20/21. The 1968 sample includes all the offenders who were sentenced to young offender institutions or adults' prisons when being age 20/21 at the date of court appearance. The McCrary test is "a test of manipulation related to the continuity of the running variable density function" (McCrary, 2008). On the x axis lies our running variable, age at court appearance, centred at 0 when the age at court appearance is 21. Age at court appearance is positive (negative) when young offenders are older (younger) than 21. On the y axis the density function of the running variable is plotted.

Figure 1.3. Second Stage (20 bins)

Panel A – 1963 Cohort



Panel B –1968 Cohort



Notes: The figure above refers to the two samples from the 1963 (Panel A) and 1968 (Panel B) cohorts of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14. The 1968 sample includes offenders who were sentenced to young offender institutions or adults' prisons when being age 20/21 at the date of court appearance, whose sentence was equal or shorter than one year and who committed an offence before June 1990. On the x axis lies the variable age at court appearance, centred at 0 when age at court appearance is 21. Age at court appearance is positive (negative) when young offenders are older (younger) than 21. On the y axis the outcomes measured after release are represented: the number of future offences, the likelihood to reoffend, the number of sentences to prison and the times the offenders go to court again. The coloured areas represent the 90% confidence intervals around the quadratic of best fit. The time span over which outcomes are observed is nine (2.5) years after release for offenders born in 1963 (1968).

Table 1.1. Annual Average Population in Prison Department Establishments & Certified Normal Accommodation (CNA) on 30 June by Type of Establishment in England & Wales, 1983-1985

Type Of Establishment	1983		1984		1985	
	Average Pop.	CNA	Average Pop.	CNA	Average Pop.	CNA
Local Prisons	15,801	10,864	15,219	10,934	16,512	10,949
Open Prisons	3,104	3,246	2,971	3,281	3,194	3,406
Closed Training Prisons	12,368	11,690	12,096	11,821	13,050	12,669
Open Youth Custody Centres	1,425	1,557	1,390	1,613	1,351	1,496
Closed Youth Custody Centres	5,066	5,280	5,244	5,297	5,488	5,375
Senior Detention Centres	1,144	1,550	943	1,459	968	1,341

Notes: The table reports the annual average population in the prison department establishments relevant to our paper and their certified normal accommodation (CNA) on 30th of June in England and Wales in 1983-1985.

Source: Home Office Statistical bulletin, The Prison Population in 1986.

Table 1.2. Annual Average Population in Prison Department Establishments & Certified Normal Accommodation (CNA) on 30 June by Type of Establishment in England & Wales, 1988-1990

Type Of Establishment	1988		1989		1990	
	Average Pop.	CNA	Average Pop.	CNA	Average Pop.	CNA
Local Prisons	17,298	11,237	17,354	12,347	15,551	11,460
Open Prisons	3,141	3,312	3,252	3,700	3,187	3,496
Closed Training Prisons	15,525	16,090	16,543	17,086	16,651	17,073
Juvenile Young Offender Institutions	293	502	330	409	285	398
Short Sentence Young Offender Institutions	438	694	340	570	296	448
Other Open Young Offender Institutions	1,174	1,472	976	1,456	877	1,312
Other Closed Young Offender Institutions	5,102	5,361	4,863	5,191	4,232	4,711

Notes: The table reports the annual average population in the prison department establishments relevant to our paper and their certified normal accommodation (CNA) on 30th of June in England and Wales in 1988-1990. Young offender institutions were established in October 1988, hence their CNA in 1988 is measured on the 30th of December.

Source: Home Office Statistical bulletin, The Prison Population in 1992.

Table 1.3. Monitored Activities Offered by Functional Groups of Establishments, % of Group Offering Each Activity in 1991/2

	Male Local (no London)	Male Dispersal	Male B Trainer (no London)	Male C Trainer	Male D Trainer	Female Local/Remand	Female Trainer	Closed YOI	Open YOI	Male Remand (no London)	London
Daytime Education	100	100	100	85	100	100	100	92	100	100	100
VT Courses	6	75	80	70	57	25	67	75	75	-	-
CIT Courses	18	50	80	70	71	25	-	75	100	-	-
Works Party	94	75	80	100	100	75	67	75	100	10	83
PSIF Workshops	88	100	80	92	86	75	33	58	25	10	67
Farms Party	12	-	40	54	71	25	33	33	50	-	-
Gardens Party	82	100	80	77	86	75	67	75	100	10	67
Kitchens	94	100	100	92	100	75	67	75	100	20	67
Other Domestic	100	100	100	100	100	100	100	100	100	100	100
Induction	29	75	100	77	86	25	67	75	100	30	-
Other (Specify)	88	50	100	92	86	75	100	83	75	70	33
All Other	88	100	100	92	100	75	67	92	100	30	83
PE	100	100	100	92	100	100	100	100	100	100	100
Evening Education	94	100	100	100	86	100	100	83	100	30	100
Chaplaincy	100	100	100	92	100	100	100	92	100	90	100

Notes: The table reports the percentage of functional groups of establishments offering each set of **monitored activities in 1991/2.** *VT* and *CIT courses* are generally courses of bricklaying, plumbing, electrical installation, painting and decorating, motor mechanics, etc. *Work parties* are groups that help the establishments to operate. *Prison Service Industries and Farms (PSIF)* are workshops ranging from sewing mailbags to highly technical (engineering/construction) work. *Gardens Party* and *Kitchens* "have a dual function in most establishments in that they serve both the institution and the inmate by offering training within the networking environment" (Her Majesty's Chief Inspector of Prisons for England and Wales, 1993). *Other domestic* activities indicate other work activities such as cleaning. *Induction* is "the process by which inmates are introduced to the establishment's routines, rules and, in most cases, opportunities" (Her Majesty's Chief Inspector of Prisons for England and Wales, 1993). *Other (specify)* activities are generally "parties, groups or individuals who are trusted to help prison staff run various parts of the establishment" (Her Majesty's Chief Inspector of Prisons for England and Wales, 1993). *All Other* occupations are pre-release courses. *PE* is physical education.

Source: Her Majesty's Chief Inspector of Prisons for England and Wales (1993), *Doing Time or Using Time, Report of a Review of Regimes in Prison Service Establishments in England and Wales*, London HMSO.

Table 1.4. Summary Statistics

	Mean (1)	Sd (2)	Min (3)	Max (4)
Panel A. 1963 Cohort				
Male	0.932	0.252	0	1
White European	0.237	0.425	0	1
Afro-Caribbean	0.027	0.162	0	1
Oriental	0.002	0.042	0	1
Arab	0.002	0.042	0	1
Born in March	0.513	0.500	0	1
Born in Sept/Oct	0.247	0.432	0	1
Born in December	0.240	0.428	0	1
Age at first court appearance	16.783	2.274	14	21
ii. Offence Characteristics				
Sentence length				
Sentence length (months)	9.528	9.793	0.467	60
Plea				
Plea: guilty	0.896	0.305	0	1
Proceedings				
Apprehension	0.294	0.456	0	1
Committed to High/Crown Court for trial	0.572	0.495	0	1
Offence				
Burglaries/Robberies	0.367	0.483	0	1
Thefts	0.305	0.461	0	1
Frauds	0.048	0.215	0	1
Violent Offences	0.170	0.376	0	1
Sexual Offences	0.011	0.103	0	1
Criminal Damage	0.011	0.103	0	1
Drug Offences	0.018	0.133	0	1
Motoring Offences	0.014	0.119	0	1
Minor Offences	0.029	0.167	0	1
Observations	558			

Table 1.4 (continued): Summary statistics

	Mean (1)	Sd (2)	Min (3)	Max (4)
Panel B. 1968 Cohort				
Male	0.973	0.162	0	1
White European	0.582	0.494	0	1
Dark European	0.000	0.000	0	0
Afro-Caribbean	0.024	0.152	0	1
Asian	0.010	0.100	0	1
Born in March	0.209	0.407	0	1
Born in June	0.263	0.441	0	1
Born in Sept/Oct	0.242	0.429	0	1
Born in December	0.286	0.453	0	1
Age at first court appearance	15.391	2.983	10	21
ii. Offence Characteristics				
Sentence length				
Sentence length (months)	5.932	3.579	0	12
Plea				
Plea: guilty	0.778	0.416	0	1
Proceedings				
Apprehension	0.286	0.453	0	1
Committed to High/Crown Court for trial	0.535	0.500	0	1
Offence				
Burglaries/Robberies	0.306	0.462	0	1
Thefts	0.259	0.439	0	1
Frauds	0.030	0.172	0	1
Violent Offences	0.229	0.421	0	1
Sexual Offences	0.007	0.082	0	1
Criminal Damage	0.007	0.082	0	1
Drug Offences	0.020	0.141	0	1
Motoring Offences	0.020	0.141	0	1
Minor Offences	0.067	0.251	0	1
Observations	297			

Notes: This table reports the summary statistics of the two samples from the 1963 and 1968 cohorts of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14. The 1968 sample includes offenders who were sentenced to young offender institutions or adults' prisons when being age 20/21 at the date of court appearance, whose sentence was equal or shorter than one year and who committed an offence before June 1990. In Panel A (B) the means, standard deviations, minima and maxima of the 1963 (1968) cohort of offenders' observable characteristics are reported, measured at the time the offenders were sentenced to either youth custody/detention centres (young offender institutions) or adults' prisons. If the offender was sentenced for multiple offences at the court appearance, the characteristics of the offence for which the sentence was longer are reported.

Table 1.5. First Stage - Parametric Approach

Independent Variable: Dummy=1 if Offender is 21 at Court Appearance

	1963 cohort		1968 cohort	
	(1)	(2)	(3)	(4)
Sentence to Adults' Prison	0.761***	0.748***	0.891***	0.862***
	(0.039)	(0.039)	(0.021)	(0.043)
Age at Court	0.000***	0.000***	0.000**	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Male		-0.038		-0.051
		(0.053)		(0.074)
Sentence Length		0.002		0.003
		(0.002)		(0.004)
Other Controls		X		X
Centered R^2	0.793	0.806	0.882	0.893
Uncentered R^2	0.910	0.916	0.935	0.941
Observations	558		297	

Notes: The table reports the first stages, i.e. how much of being sentenced to an adults' prison depends on actually being 21. Columns (1)-(2) refer to the sample from the 1963 cohort, which includes offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14; Columns (3)-(4) refer to the sample form the 1968 cohort, which includes offenders who were sentenced to young offender institutions or adults' prisons when being age 20/21 at the date of court appearance, whose sentence was equal or shorter than one year and who committed an offence before June 1990. Robust standard errors are reported in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. In Columns (2)-(4) control variables are included: gender, sentence length and other controls (ethnicity, plea, proceedings, month of birth, type of offence and age at which the offender committed the first offence).

Table 1.6. Effects of Adults' Prison vs. Youth Custody/Detention Centres (in the Next Nine Years)

Independent Variable: Adults' Prison	365 days	Ludwig and Miller (2007)	274 days	183 days
	(1)	(2)	(3)	(5)
Likelihood to reoffend	-0.207**	-0.208**	-0.186*	-0.126
	(0.095)	(0.096)	(0.109)	(0.148)
Mean in Control Group	0.737			
Offences	-2.838***	-2.856***	-2.713**	-2.273*
	(1.021)	(1.028)	(1.081)	(1.339)
Mean in Control Group	5.243			
Times to court	-1.385***	-1.404***	-1.426**	-1.320*
	(0.521)	(0.527)	(0.573)	(0.739)
Mean in Control Group	2.749			
Sentences to prison	-0.920	-0.947	-0.962	-0.691
	(0.613)	(0.618)	(0.648)	(0.729)
Mean in Control Group	1.848			
Observations	558	542	457	288

Notes: The table reports the effects of experiencing prison rather than youth custody/detention centres for the 1963 cohort of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14. The time window over which the outcome variables are observed is nine years. Each set of rows corresponds to a different outcome variable: the likelihood to reoffend (a dummy equal to 1 if the offender commits at least 1 offence in the future time window), the number of offences the offender commits, the times he/she is brought to court and the times he/she is sentenced to prison again. The estimation is conducted through a local linear regression constructed with a triangular kernel regression. Each Column corresponds to a different bandwidth selection: in Column (1) the bandwidth is 365 days; in Column (2) the bandwidth is the one suggested by Ludwig and Miller (2007); in Column (3) it is 274 days; in Column (4) it is 183 days. Standard errors are reported in parentheses:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.7. Effects of Adults' Prison vs. Youth Custody/Detention Centres by Type of Offence (in the Next Nine Years)

Independent Variable: Adults' Prison	365 days	Ludwig and Miller (2007)	274 days	183 days
	(1)	(2)	(3)	(5)
Thefts	-0.906**	-0.803	-0.967**	-0.805
	(0.456)	(0.502)	(0.445)	(0.501)
Mean in Control Group	1.835			
Violent offences	-0.695**	-0.698**	-0.707**	-0.843*
	(0.299)	(0.305)	(0.348)	(0.464)
Mean in Control Group	0.613			
Sexual offences	-0.021	-0.022	-0.016	-0.008
	(0.037)	(0.036)	(0.032)	(0.009)
Mean in Control Group	0.041			
Burglaries/robberies	-0.431*	-0.442*	-0.430	-0.234
	(0.248)	(0.250)	(0.264)	(0.332)
Mean in Control Group	0.716			
Minor offences	-0.318	-0.314	-0.267	-0.265
	(0.292)	(0.298)	(0.338)	(0.460)
Mean in Control Group	0.663			
Frauds	-0.146	-0.015	0.001	0.220
	(0.207)	(0.212)	(0.213)	(0.214)
Mean in Control Group	0.514			
Criminal Damage	-0.166**	-0.161**	-0.119	-0.075
	(0.074)	(0.075)	(0.081)	(0.100)
Mean in Control Group	0.144			
Drug offences	0.127	0.125	0.119	0.128
	(0.095)	(0.096)	(0.103)	(0.121)
Mean in Control Group	0.165			
Motoring Offences	-0.039	-0.041	-0.073	-0.119**
	(0.085)	(0.085)	(0.076)	(0.051)
Mean in Control Group	0.082			
Other offences† †	-0.323**	-0.320**	-0.329**	-0.334*
	(0.156)	(0.156)	(0.157)	(0.178)
Mean in Control Group	0.453			
Observations	558	542	457	288

Notes: The table reports the effects of experiencing prison rather than youth custody/detention centres for the 1963 cohort of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14. The time window over which the outcome variables are observed is nine years. Each set of rows corresponds to a different type of offence. The estimation is conducted through a local linear regression constructed with a triangular kernel regression. Each Column corresponds to a different bandwidth selection: in Column (1) the bandwidth is 365 days; in Column (2) the bandwidth is the one suggested by Ludwig and Miller (2007); in Column (3) it is 274 days; in Column (4) it is 183 days. Standard errors are reported in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. † Other offences include mainly: failing to surrender to bail (65.63%), going equipped for stealing (20.79%) and other offences against the state or public order (6.55%).

Table 1.8. Effects of Adults' Prison vs. Youth Custody/Detention Centres & vs. Young Offender Institutions (in the 2.5 Years Following Release)

Independent Variable: Adults' Prison	365 days	Ludwig and Miller (2007)	274 days	183 days
	(1)	(2)	(3)	(5)
Panel A. 1963 Cohort				
Likelihood to reoffend	-0.311***	-0.314***	-0.317**	-0.238
	(0.110)	(0.112)	(0.125)	(0.163)
Mean in Control Group	0.709			
Offences	-1.029*	-1.020*	-0.893	-0.602
	(0.603)	(0.612)	(0.679)	(0.869)
Mean in Control Group	3.452			
Times to court	-0.567*	-0.567*	-0.541	-0.358
	(0.292)	(0.297)	(0.336)	(0.445)
Mean in Control Group	1.927			
Sentences to prison	-0.388	-0.393	-0.377	-0.284
	(0.367)	(0.372)	(0.413)	(0.528)
Mean in Control Group	1.194			
Observations	445	435	364	228
Panel B. 1968 Cohort				
Likelihood to reoffend	0.115	0.114	0.113	0.130
	(0.124)	(0.126)	(0.140)	(0.169)
Mean in Control Group	0.606			
Offences	1.050	1.009	0.841	0.139
	(0.992)	(1.007)	(1.104)	(1.360)
Mean in Control Group	2.856			
Times to court	0.351	0.340	0.365	0.253
	(0.366)	(0.372)	(0.409)	(0.485)
Mean in Control Group	1.303			
Sentences to prison	1.281**	1.286**	1.311*	1.171
	(0.618)	(0.625)	(0.670)	(0.846)
Mean in Control Group	0.879			
Observations	297	291	254	182

Notes: The table reports the effects of experiencing prison rather than youth custody/detention centres for the 1963 cohort (Panel A) and the effects of experiencing prison rather than young offender institutions for the 1968 cohort (Panel B) of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes offenders who were sentenced to either youth custody/ detention centres or adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14. The 1968 sample includes offenders who were sentenced to young offender institutions or adults' prisons when being age 20/21 at the date of court appearance, whose sentence was equal or shorter than one year and who committed an offence before June 1990. The time window over which the outcome variables are observed is 2.5 years following release from custody. Each set of rows corresponds to a different outcome variable: the likelihood to reoffend (a dummy equal to 1 if the offender commits at least 1 offence in the future time window), the number of offences the offender commits, the times he/she is brought to court and the times he/she is sentenced to prison again. The estimation is conducted through a local linear regression constructed with a triangular kernel regression. Each Column corresponds to a different bandwidth selection: in Column (1) the bandwidth is 365 days; in Column (2) the bandwidth is the one suggested by Ludwig and Miller (2007); in Column (3) it is 274 days; in Column (4) it is 183 days. Standard errors are reported in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.9. Effects of Adults' Prison vs. Youth Custody/Detention Centres & vs. Young Offender Institutions by Type of Offence (in the 2.5 Years Following Release)

Independent Variable: Adults' Prison	365 days	Ludwig and Miller (2007)	274 days	183 days
	(1)	(2)	(3)	(5)
Panel A. 1963 Cohort				
Burglaries and Robberies	0.113	0.188	0.199	0.365
	(0.197)	(0.222)	(0.227)	(0.298)
Mean in Control Group	0.485			
Thefts	-0.567**	-0.595**	-0.603**	-0.487*
	(0.236)	(0.250)	(0.253)	(0.288)
Mean in Control Group	1.282			
Violent Offences	-0.477**	-0.477**	-0.459*	-0.518
	(0.223)	(0.228)	(0.261)	(0.357)
Mean in Control Group	0.432			
Panel B. 1968 Cohort				
Burglaries and Robberies	0.684*	0.679*	0.616	0.566
	(0.386)	(0.394)	(0.444)	(0.550)
Mean in Control Group	0.467			
Thefts	0.200	-0.018	-0.002	-0.491
	(0.387)	(0.414)	(0.411)	(0.533)
Mean in Control Group	1.055			
Violent Offences	-0.051	-0.051	-0.033	0.016
	(0.188)	(0.193)	(0.221)	(0.278)
Mean in Control Group	0.206			

Notes: The table reports the effects of experiencing prison rather than youth custody/detention centres for the 1963 cohort (Panel A) and the effects of experiencing prison rather than young offender institutions for the 1968 cohort (Panel B) of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes offenders who were sentenced to either youth custody/ detention centres or adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14. The 1968 sample includes offenders who were sentenced to young offender institutions or adults' prisons when being age 20/21 at the date of court appearance, whose sentence was equal or shorter than one year and who committed an offence before June 1990. The time window over which the outcome variables are observed is 2.5 years following release from custody. Each set of rows corresponds to a different type of offence. The estimation is conducted through a local linear regression constructed with a triangular kernel regression. Each Column corresponds to a different bandwidth selection: in Column (1) the bandwidth is 365 days; in Column (2) the bandwidth is the one suggested by Ludwig and Miller (2007); in Column (3) it is 274 days; in Column (4) it is 183 days. Standard errors are reported in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.10. Effects of Adults' Prison vs. Youth Custody/Detention Centres (in the Next Nine Years) - Parametric Approach

Independent Variable: Adults' Prison	(1)	(2)	(3)	(4)	(5)	(6)
Likelihood to reoffend	-0.244**	-0.164*	-0.248**	-0.173*	-0.265*	-0.226*
	(0.101)	(0.094)	(0.099)	(0.091)	(0.146)	(0.127)
Offences	-3.142**	-2.209*	-3.289**	-2.430*	-3.096*	-2.201
	(1.311)	(1.306)	(1.332)	(1.317)	(1.689)	(1.650)
Times to court	-1.460**	-1.097*	-1.481**	-1.169*	-1.754**	-1.476*
	(0.625)	(0.614)	(0.631)	(0.614)	(0.889)	(0.830)
Sentences to prison	-0.757	-0.273	-0.783	-0.303	-1.630*	-1.342
	(0.724)	(0.707)	(0.750)	(0.715)	(0.973)	(0.964)
Age at Court	X	X	X	X	X	X
Age*prison			X	X	X	X
Age^2*prison					X	X
Age at $Court^2$					X	X
Controls		X		X		X
Observations	558	557	558	557	558	557

Notes: The table reports the effects of experiencing prison rather than youth custody/detention centres for the 1963 cohort of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14. The time window over which the outcome variables are observed is nine years. Each set of rows corresponds to a different outcome variable: the likelihood to reoffend (a dummy equal to 1 if the offender commits at least 1 offence in the future time window), the number of offences the offender commits, the times he/she is brought to court and the times he/she is sentenced to prison again. The estimation is conducted through a parametric approach using a polynomial up to the second order. We also allow the treatment to have a different impact before and after the cut-off by including an interaction of the centred variable and the treatment variable (age at court*prison). Robust Standard errors are reported in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The control variables in the even Columns include gender, sentence length, ethnicity, plea, proceedings, month of birth, type of offence, age at which the offender committed the first offence.

Table 1.11. Effects of Adults' Prison vs. Youth Custody/Detention Centres (in the Next Nine Years) by Offence Type - Parametric Approach

Independent Variable: Adults' Prison	(1)	(2)	(3)	(4)	(5)	(6)
Thefts	-0.778	-0.124	-0.840	-0.204	-0.944	-0.367
	(0.626)	(0.628)	(0.635)	(0.632)	(0.692)	(0.688)
Violent offences	-0.818***	-0.892***	-0.843***	-0.927***	-0.918	-1.080*
	(0.304)	(0.322)	(0.323)	(0.335)	(0.561)	(0.563)
Sexual offences	-0.014	-0.014	-0.016	-0.010	-0.029	-0.010
	(0.050)	(0.052)	(0.053)	(0.053)	(0.054)	(0.059)
Burglary/robbery	-0.372	-0.219	-0.414	-0.279	-0.609	-0.480
	(0.340)	(0.348)	(0.341)	(0.346)	(0.379)	(0.372)
Minor offences	-0.385	-0.406	-0.453	-0.471	-0.419	-0.558
	(0.298)	(0.306)	(0.312)	(0.316)	(0.513)	(0.484)
Fraud	-0.385	-0.301	-0.383	-0.312	0.236	0.495
	(0.273)	(0.280)	(0.261)	(0.266)	(0.276)	(0.304)
Criminal damage	-0.249***	-0.255***	-0.259***	-0.257***	-0.103	-0.118
	(0.086)	(0.089)	(0.089)	(0.091)	(0.124)	(0.134)
Drug offences	0.182	0.218*	0.208*	0.229*	0.082	0.146
	(0.131)	(0.128)	(0.124)	(0.122)	(0.147)	(0.136)
Motoring offences	-0.001	-0.013	-0.017	-0.026	-0.067	-0.101
	(0.117)	(0.113)	(0.116)	(0.111)	(0.111)	(0.113)
Other offences†	-0.459**	-0.353	-0.411*	-0.322	-0.336	-0.141
	(0.226)	(0.254)	(0.222)	(0.249)	(0.260)	(0.286)
Age at Court	X	X	X	X	X	X
Age*prison			X	X	X	X
Age^2*prison					X	X
Age at Court^2					X	X
Controls		X		X		X
Observations	558	557	558	557	558	557

Notes: The table reports the effects of experiencing prison rather than youth custody/detention centres for the 1963 cohort of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14. The time window over which the outcome variables are observed is nine years. Each set of rows corresponds to a different type of offence. The estimation is conducted through a parametric approach using a polynomial up to the second order. We also allow the treatment to have a different impact before and after the cut-off by including an interaction of the centred variable and the treatment variable (age at court*prison). Robust Standard errors are reported in parentheses: * p < 0.1, ** p < 0.05, *** p < 0.01. The control variables in the even Columns include gender, sentence length, ethnicity, plea, proceedings, month of birth, type of offence, age at which the offender committed the first offence. † Other offences include mainly: failing to surrender to bail (65.63%), going equipped for stealing (20.79%) and other offences against the state or public order (6.55%).

Table 1.12. Effects of Adults' Prison vs. Youth Custody/Detention Centres & vs. Young Offender Institutions by Type of Offence (in the 2.5 Years Following Release) - Parametric Approach

Independent Variable: Adults' Prison	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 1963 cohort						
Likelihood to reoffend	-0.346***	-0.354***	-0.356***	-0.363***	-0.521**	-0.522***
	(0.127)	(0.126)	(0.126)	(0.124)	(0.214)	(0.198)
Offences	-1.272*	-1.224*	-1.302**	-1.250*	-1.334	-1.113
	(0.676)	(0.708)	(0.658)	(0.699)	(1.004)	(1.011)
Times to court	-0.663**	-0.664**	-0.672**	-0.671**	-0.817	-0.824*
	(0.316)	(0.318)	(0.313)	(0.317)	(0.519)	(0.492)
Sentences to prison	-0.298	-0.404	-0.363	-0.457	-0.809	-0.697
	(0.427)	(0.457)	(0.415)	(0.448)	(0.623)	(0.642)
Age at Court	X	X	X	X	X	X
Age*prison			X	X	X	X
Age^2*prison					X	X
Age at Court^2					X	X
Controls		X		X		X
Observations	445	445	445	445	445	445
Panel B: 1968 cohort						
Likelihood to reoffend	0.147	0.169	0.154	0.157	0.130	0.207
	(0.118)	(0.115)	(0.120)	(0.116)	(0.205)	(0.205)
Offences	1.596	0.698	1.722	0.699	0.835	0.838
	(1.072)	(1.077)	(1.107)	(1.093)	(1.726)	(1.927)
Times to court	0.537	0.481	0.540	0.455	0.288	0.423
	(0.358)	(0.362)	(0.366)	(0.369)	(0.602)	(0.648)
Sentences to prison	1.399**	0.990	1.463**	1.080*	1.894*	1.998*
	(0.630)	(0.607)	(0.683)	(0.645)	(1.026)	(1.164)
Age at Court	X	X	X	X	X	X
Age*prison			X	X	X	X
Age^2*prison					X	X
Age at Court^2					X	X
Controls		X		X		X
Observations	297	297	297	297	297	297

Notes: The table reports the effects of experiencing prison rather than youth custody/detention centres for the 1963 cohort (Panel A) and the effects of experiencing prison rather than young offender institutions for the 1968 (Panel B) cohort of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14. The 1968 sample includes offenders who were sentenced to young offender institutions or adults' prisons when being age 20/21 at the date of court appearance, whose sentence was equal or shorter than one year and who committed an offence before June 1990. The time window over which the outcome variables are observed is 2.5 years following release from custody. Each set of rows corresponds to a different outcome variable: the likelihood to reoffend (a dummy equal to 1 if the offender commits at least 1 offence in the future time window), the number of offences the offender commits, the times he/she is brought to court and the times he/she is sentenced to prison again. The estimation is conducted through a parametric approach using a polynomial up to the second order. We also allow the treatment to have a different impact before and after the cut-off by including an interaction of the centred variable and the treatment variable (age at court*prison). Each Column corresponds to a different bandwidth selection: in Column (1) the bandwidth is 365 days; in Column (2) the bandwidth is the one suggested by Ludwig and Miller (2007); in Column (3) it is 274 days; in Column (4) it is 183 days. Robust Standard errors are reported in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The control variables in the even Columns include gender, sentence length, ethnicity, plea, proceedings, month of birth, type of offence, age at which the offender committed the first offence.

Table 1.13. Effects of Adults' Prison vs. Youth Custody/Detention Centres (in the Next Nine Years) - Full Sample

Independent Variable: Adults' Prison	365 days	Ludwig and Miller (2007)	274 days	183 days
	(1)	(2)	(3)	(5)
Likelihood to reoffend	-0.188**	-0.189**	-0.183**	-0.143
	(0.080)	(0.082)	(0.092)	(0.117)
Mean in Control Group	0.779			
Offences	-3.462***	-3.480***	-3.377***	-3.849***
	(0.994)	(1.002)	(1.067)	(1.382)
Mean in Control Group	6.000			
Times to court	-1.645***	-1.665***	-1.742***	-2.082***
	(0.489)	(0.495)	(0.537)	(0.685)
Mean in Control Group	3.061			
Sentences to prison	-1.524***	-1.541***	-1.492***	-1.467**
	(0.537)	(0.541)	(0.568)	(0.680)
Mean in Control Group	2.285			
Observations	706	690	578	382

Notes: The table reports the effects of experiencing prison rather than youth custody/detention centres for the 1963 cohort of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance and as a robustness check we also include offenders who committed their first offence when younger than 14. The time window over which the outcome variables are observed is nine years. Each set of rows corresponds to a different outcome variable: the likelihood to reoffend (a dummy equal to 1 if the offender commits at least 1 offence in the future time window), the number of offences the offender commits, the times he/she is brought to court and the times he/she is sentenced to prison again. The estimation is conducted through a local linear regression constructed with a triangular kernel regression. Each Column corresponds to a different bandwidth selection: in Column (1) the bandwidth is 365 days; in Column (2) the bandwidth is the one suggested by Ludwig and Miller (2007); in Column (3) it is 274 days; in Column (4) it is 183 days. Standard errors are reported in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.14. Effects of Adults' Prison vs. Youth Custody/Detention Centres (in the Next Nine Years) by Offence Type - Full Sample

	365 days	Ludwig and Miller	274 days	183 days
	(1)	(2)	(3)	(5)
Thefts	-0.867*	-1.459**	-1.042**	-1.375**
	(0.461)	(0.678)	(0.484)	(0.617)
	2.043			
Violent offences	-0.883***	-0.874***	-0.795***	-0.872**
	(0.247)	(0.251)	(0.281)	(0.365)
	0.745			
Sexual offences	-0.017	-0.019	-0.027	-0.041*
	(0.030)	(0.029)	(0.026)	(0.023)
	0.034			
Burglaries/robberies	-0.754***	-0.760***	-0.740**	-0.664*
	(0.277)	(0.279)	(0.297)	(0.358)
	0.862			
Minor offences	-0.338	-0.328	-0.229	-0.253
	(0.226)	(0.229)	(0.251)	(0.317)
	0.779			
Frauds	-0.232	-0.224	-0.154	-0.103
	(0.197)	(0.199)	(0.213)	(0.259)
	0.607			
Criminal Damage	-0.120**	-0.115*	-0.057	-0.034
	(0.060)	(0.060)	(0.063)	(0.072)
	0.169			
Drug offences	0.127	0.124	0.104	0.086
	(0.080)	(0.080)	(0.087)	(0.107)
	0.175			
Motoring Offences	-0.046	-0.048	-0.084	-0.177**
	(0.077)	(0.077)	(0.074)	(0.078)
	0.092			
Other offences †	-0.384***	-0.386***	-0.407***	-0.452***
	(0.122)	(0.122)	(0.122)	(0.146)
	0.463			
Observations	706	690	578	382

Notes: The table reports the effects of experiencing prison rather than youth custody/detention centres for the 1963 cohort of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance and as a robustness check we also include offenders who committed their first offence when younger than 14. The time window over which the outcome variables are observed is nine years. Each set of rows corresponds to a different type of offence. The estimation is conducted through a local linear regression constructed with a triangular kernel regression. Each Column corresponds to a different bandwidth selection: in Column (1) the bandwidth is 365 days; in Column (2) the bandwidth is the one suggested by Ludwig and Miller (2007); in Column (3) it is 274 days; in Column (4) it is 183 days. Standard errors are reported in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. † Other offences include mainly: failing to surrender to bail (65.63%), going equipped for stealing (20.79%) and other offences against the state or public order (6.55%).

Table 1.15. Effects of Adults' Prison vs. Youth Custody/Detention Centres (in the Four Years Following Release)

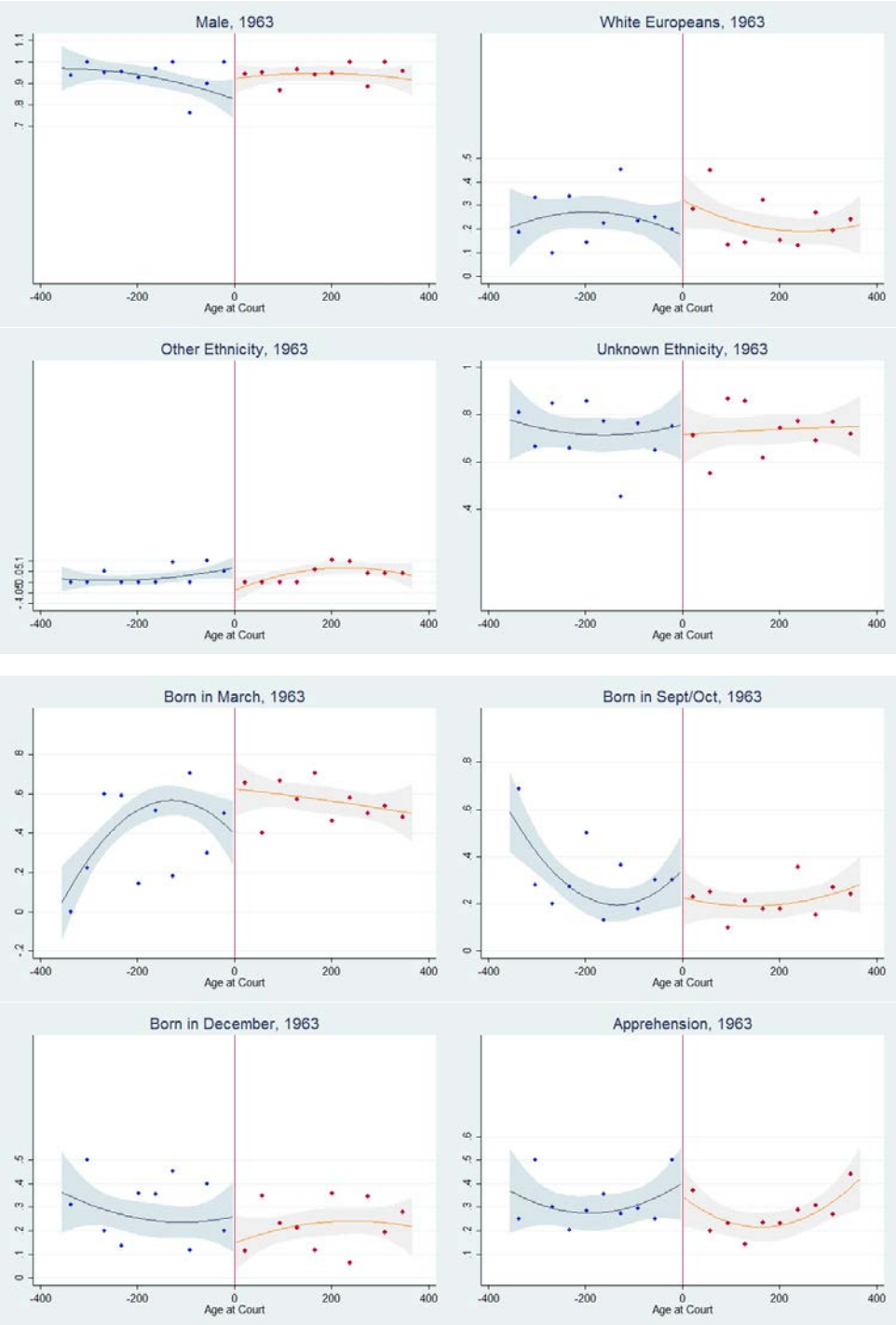
Independent Variable: Adults' Prison	365 days	Ludwig and Miller (2007)	274 days	183 days
	(1)	(2)	(3)	(5)
Likelihood to reoffend	-0.357***	-0.362***	-0.391***	-0.387***
	(0.093)	(0.094)	(0.106)	(0.140)
Mean in Control Group	0.672			
Offences	-1.804**	-1.807**	-1.785**	-1.529
	(0.749)	(0.758)	(0.820)	(1.038)
Mean in Control Group	3.021			
Times to court	-0.961***	-0.966***	-0.981***	-0.841*
	(0.331)	(0.336)	(0.375)	(0.501)
Mean in Control Group	1.656			
Sentences to prison	-0.506	-0.514	-0.489	-0.217
	(0.496)	(0.501)	(0.530)	(0.620)
Mean in Control Group	1.104			
Observations	555	539	454	286

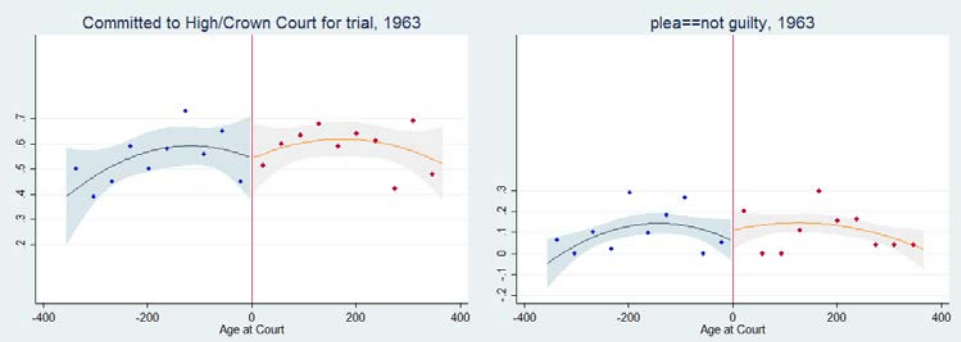
Notes: The table reports the effects of experiencing prison rather than youth custody/detention centres for the 1963 cohort of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14. The time window over which the outcome variables are observed is four years after release. Each set of rows corresponds to a different outcome variable: the likelihood to reoffend (a dummy equal to 1 if the offender commits at least 1 offence in the future time window), the number of offences the offender commits, the times he/she is brought to court and the times he/she is sentenced to prison again. The estimation is conducted through a local linear regression constructed with a triangular kernel regression. Each Column corresponds to a different bandwidth selection: in Column (1) the bandwidth is 365 days; in Column (2) the bandwidth is the one suggested by Ludwig and Miller (2007); in Column (3) it is 274 days; in Column (4) it is 183 days. Standard errors are reported in parentheses: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

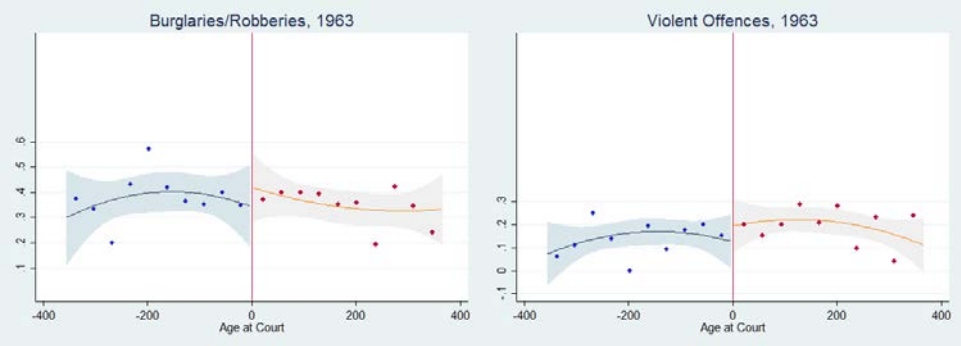
SUPPLEMENTAL ONLINE APPENDIX

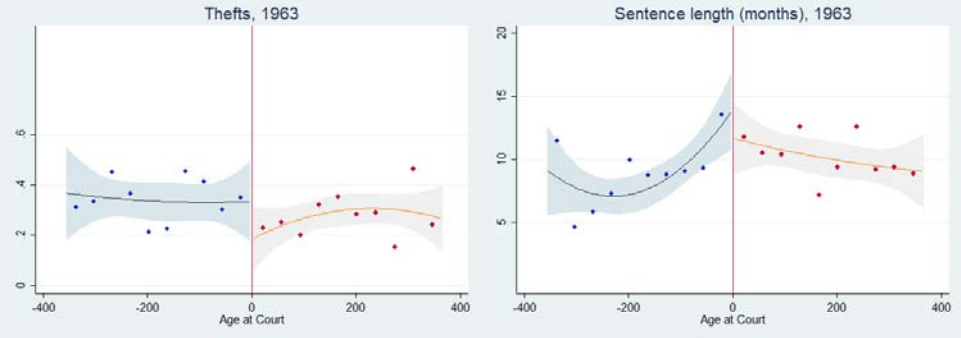
Appendix Figure A1. 1. Pre-Treatment Variables (20 bins)

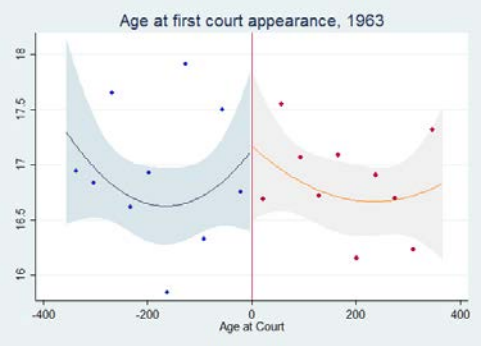
Panel A – 1963 cohort



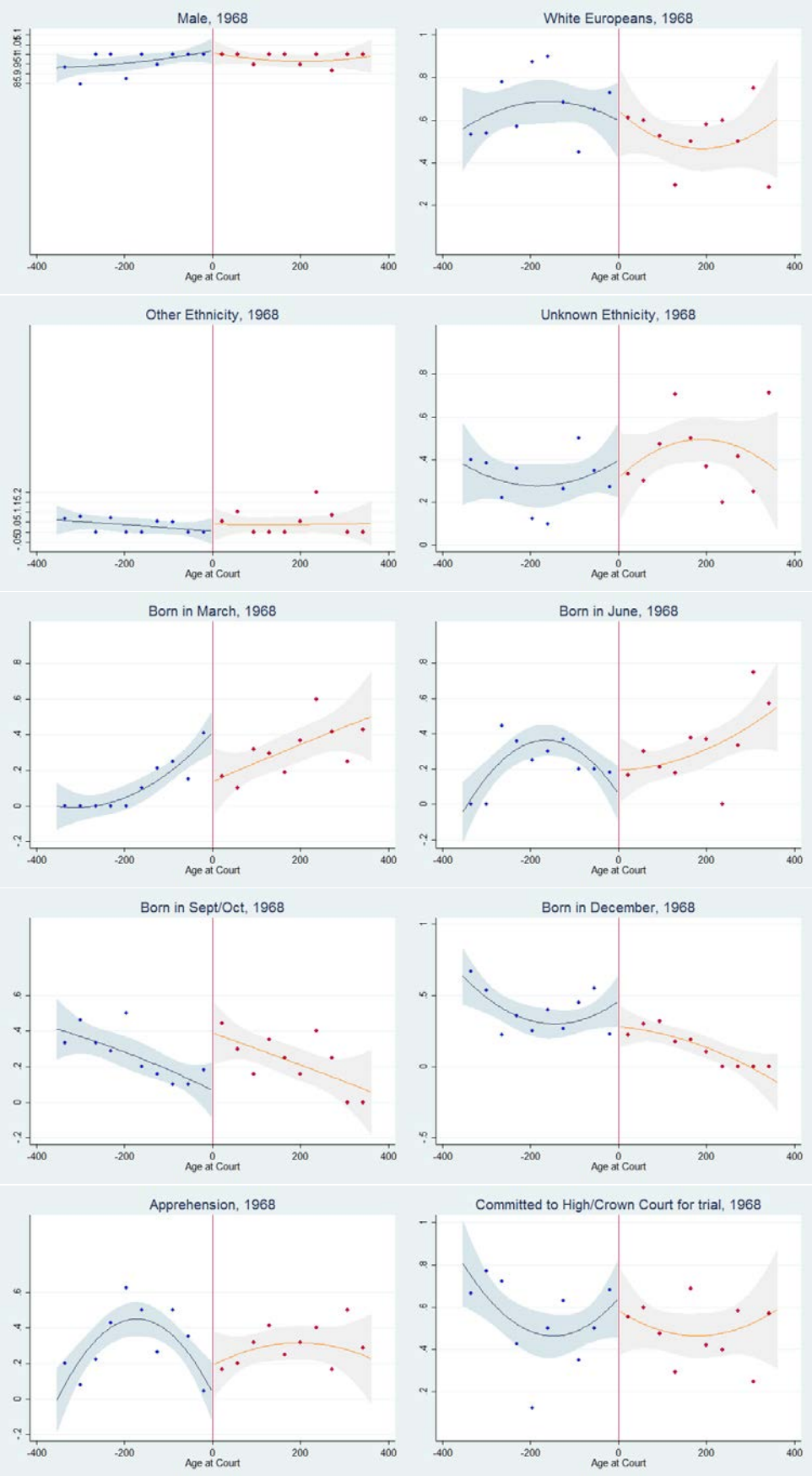

Committed to High/Crown Court for trial, 1963
plea==not guilty, 1963
Age at Court

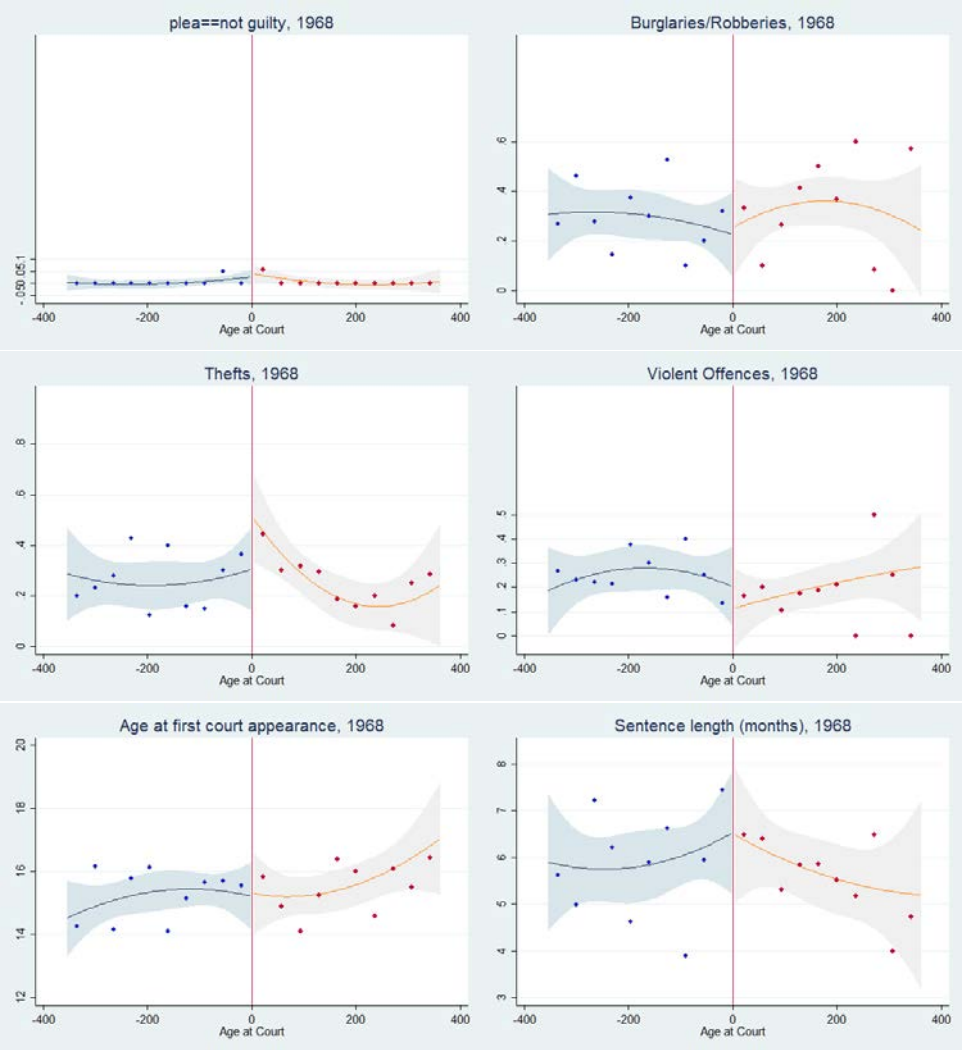

Burglaries/Robberies, 1963
Violent Offences, 1963
Age at Court


Thefts, 1963
Sentence length (months), 1963
Age at Court


Age at first court appearance, 1963
Age at Court

Panel B – 1968 cohort





Notes: The figures above refer to the two samples from the 1963 (Panel A) and 1968 (Panel B) cohorts of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate). The 1963 sample includes offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14. The 1968 sample includes offenders who were sentenced to young offender institutions or adults' prisons when being age 20/21 at the date of court appearance, whose sentence was equal or shorter than one year and who committed an offence before June 1990. On the x axis lies the variable age at court appearance, centred at 0 when age at court appearance is 21. Age at court appearance is positive (negative) when young offenders are older (younger) than 21. On the y axis there are the shares of pre-treatment characteristics: gender, month of birth, ethnicity, age at first court appearance, sentence length, proceedings type, plea and type of offence committed when 20/21 years old. The coloured areas represent the 90% confidence intervals around the separate lines of quadratic best fit plotted on the left and right hand side of the cut-off.

Appendix Table A1. 1. Proceedings Characteristics in More Detail

	Mean	Sd	Min	Max
	(1)	(2)	(3)	(4)
Panel A. 1963 cohort				
Proceedings				
Apprehension	0.292	0.455	0	1
Summons by police	0.016	0.126	0	1
Committed for sentence - young offenders institution (over 6 months)	0.002	0.042	0	1
Committed for sentence for offences triable either way	0.032	0.177	0	1
Committed to High/Crown Court for trial on indictment	0.573	0.495	0	1
Committed to High/Crown Court for sentence for offences tried summarily	0.002	0.042	0	1
Appearance for sentence after deferment without further conviction	0.004	0.060	0	1
Notice of Transfer	0.004	0.060	0	1
Breach of an order for conditional discharge	0.002	0.042	0	1
Breach of requirements of probation order	0.002	0.042	0	1
Breach of requirements of probation order over 1 year and up to 2 years (dealt with for original offence)	0.007	0.084	0	1
Breach of requirements of probation order over 2 years (dealt with for original offence)	0.004	0.060	0	1
Breach of probation order for 6 months following the commission of a fresh offence	0.002	0.042	0	1
Breach of probation order with a term of over 1 year and up to 2 years following the commission of a fresh offence	0.007	0.084	0	1
Breach of requirements of community service order	0.002	0.042	0	1
Breach of requirements of community service order; order revoked (dealt with for original offence)	0.016	0.126	0	1
Breach of sentence of imprisonment suspended for 1 year, no supervision order ever in force Breach of fully suspended sentence of imprisonment	0.007	0.084	0	1
Breach of sentence of imprisonment suspended for over 1 year and up to 2 years, no supervision order ever in force	0.027	0.162	0	1
Observations	558			

	Mean (1)	Sd (2)	Min (3)	Max (4)
Panel B. 1968 cohort				
Proceedings				
Summons by police	0.020	0.141	0	1
Summons other than by police	0.007	0.082	0	1
Committed for sentence for offences triable either way	0.054	0.227	0	1
Committed to High/Crown Court for trial on indictment	0.534	0.500	0	1
Committed to High/Crown Court for sentence for offences tried summarily	0.003	0.058	0	1
Breach of an order for conditional discharge	0.010	0.100	0	1
Breach of probation order for 6 months following the commission of a fresh offence	0.034	0.181	0	1
Breach of requirements of community service order	0.047	0.213	0	1
Breach of sentence of imprisonment suspended for 1 year, no supervision order ever in force Breach of fully suspended sentence of imprisonment	0.007	0.082	0	1
Summons by police	0.020	0.141	0	1
Summons other than by police	0.007	0.082	0	1
Committed for sentence for offences triable either way	0.054	0.227	0	1
Committed to High/Crown Court for trial on indictment	0.534	0.500	0	1
Committed to High/Crown Court for sentence for offences tried summarily	0.003	0.058	0	1
Breach of an order for conditional discharge	0.010	0.100	0	1
Breach of probation order for 6 months following the commission of a fresh	0.034	0.181	0	1
Breach of requirements of community service order	0.047	0.213	0	1
Breach of sentence of imprisonment suspended for 1 year, no supervision order ever in force Breach of fully suspended sentence of imprisonment	0.007	0.082	0	1
Summons by police	0.020	0.141	0	1
Summons other than by police	0.007	0.082	0	1
Committed for sentence - young offenders institution (over 6 months)	0.000	0.000	0	0
Committed for sentence for offences triable either way	0.054	0.227	0	1
Committed to High/Crown Court for trial on indictment	0.534	0.500	0	1
Committed to High/Crown Court for sentence for offences tried summarily	0.003	0.058	0	1
Breach of an order for conditional discharge	0.010	0.100	0	1
Breach of probation order for 6 months following the commission of a fresh	0.034	0.181	0	1
Breach of requirements of community service order	0.047	0.213	0	1
Breach of sentence of imprisonment suspended for 1 year, no supervision order ever in force Breach of fully suspended sentence of imprisonment	0.007	0.082	0	1
Committed to High/Crown Court for trial on indictment	0.534	0.500	0	1
Committed to High/Crown Court for sentence for offences tried summarily	0.003	0.058	0	1
Breach of an order for conditional discharge	0.010	0.100	0	1
Breach of probation order for 6 months following the commission of a fresh offence	0.034	0.181	0	1
Breach of requirements of community service order	0.047	0.213	0	1
Breach of sentence of imprisonment suspended for 1 year, no supervision order ever in force Breach of fully suspended sentence of imprisonment	0.007	0.082	0	1

	Mean	Sd	Min	Max
	(1)	(2)	(3)	(4)
Panel B. 1968 cohort – continuation				
Proceedings				
Committed to High/Crown Court for trial on indictment	0.534	0.500	0	1
Committed to High/Crown Court for sentence for offences tried summarily	0.003	0.058	0	1
Breach of an order for conditional discharge	0.010	0.100	0	1
Breach of probation order for 6 months following the commission of a fresh offence	0.034	0.181	0	1
Breach of requirements of community service order	0.047	0.213	0	1
Breach of sentence of imprisonment suspended for 1 year, no supervision order ever in force Breach of fully suspended sentence of imprisonment	0.007	0.082	0	1
Committed to High/Crown Court for trial on indictment	0.534	0.500	0	1
Committed to High/Crown Court for sentence for offences tried summarily	0.003	0.058	0	1
Breach of an order for conditional discharge	0.010	0.100	0	1
Breach of probation order for 6 months following the commission of a fresh offence	0.034	0.181	0	1
Breach of requirements of community service order	0.047	0.213	0	1
Breach of sentence of imprisonment suspended for 1 year, no supervision order ever in force Breach of fully suspended sentence of imprisonment	0.007	0.082	0	1
Observations	296			

Notes: This table reports the means, standard deviations, minima and maxima of the detailed proceedings of the two samples from the 1963 (Panel A) and 1968 (Panel B) cohorts of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate) at the time the offenders were sentenced to either youth custody/detention centres/young offender institutions or adults' prisons. The 1963 sample includes offenders who were sentenced to either youth custody/detention centres or adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14. The 1968 sample includes offenders who were sentenced to young offender institutions or adults' prisons when being age 20/21 at the date of court appearance, whose sentence was equal or shorter than one year and who committed an offence before June 1990. If the offender was sentenced for multiple offences at the court appearance, the proceedings of the offence for which the sentence was longer are reported.

Appendix Table A1. 2. Offence Characteristics in More Detail

	Mean	Sd	Min	Max
	(1)	(2)	(3)	(4)
Panel A. 1963 cohort				
Offence				
Manslaughter	0.002	0.042	0	1
Wounding and other acts endangering life (felonies)	0.014	0.119	0	1
Malicious wounding and other like offences (misdemeanours)	0.131	0.338	0	1
Assault	0.009	0.094	0	1
Rape	0.005	0.073	0	1
Indecent assault on a female	0.004	0.060	0	1
Unlawful sexual intercourse with girl under 16	0.002	0.042	0	1
Burglary in a dwelling (1979-)	0.158	0.365	0	1
Burglary, other than a dwelling	0.156	0.363	0	1
Going equipped for stealing	0.005	0.073	0	1
Robbery and assaults with intent to rob	0.054	0.226	0	1
Stealing in a dwelling other than from automatic machines and meters	0.002	0.042	0	1
Stealing by an employee (1976-)	0.004	0.060	0	1
Theft from vehicle	0.018	0.133	0	1
Stealing from shops and stalls (shoplifting) (1976-)	0.043	0.203	0	1
Stealing from automatic machines and meters (1976-)	0.009	0.094	0	1
Other stealings and unauthorised takings	0.115	0.319	0	1
Other frauds	0.038	0.190	0	1
Receiving/handling stolen goods	0.052	0.222	0	1
Arson	0.005	0.073	0	1
Other criminal Damage	0.005	0.073	0	1
Uttering or possessing counterfeit coin	0.011	0.103	0	1
Other offences (against the State and Public Order)	0.023	0.151	0	1
Perjury and false statements	0.002	0.042	0	1
Misuse of Drugs	0.020	0.139	0	1
Possession of firearms by persons previously convicted of	0.002	0.042	0	1
Bail Act 1976	0.005	0.073	0	1
Assault	0.014	0.119	0	1
Interference with a motor vehicle	0.004	0.060	0	1
Criminal and malicious damage	0.013	0.111	0	1
Non-patrial having only limited leave remains in United Kingdom beyond the time limit	0.002	0.042	0	1
Theft or unauthorised taking of motor vehicle	0.059	0.236	0	1
Dangerous driving	0.002	0.042	0	1
Driving licence offences	0.014	0.119	0	1
Observations	558			

	Mean (1)	Sd (2)	Min (3)	Max (4)
Panel B. 1968 cohort				
Offence				
Manslaughter	0.010	0.100	0	1
Wounding and other acts endangering life (felonies)	0.003	0.058	0	1
Malicious wounding and other like offences (misdemeanours)	0.186	0.390	0	1
Assault	0.003	0.058	0	1
Indecent assault on a female	0.003	0.058	0	1
Burglary in a dwelling (1979-)	0.145	0.353	0	1
Aggravated burglary in a dwelling	0.007	0.082	0	1
Burglary, other than a dwelling	0.145	0.353	0	1
Going equipped for stealing, etc.	0.007	0.082	0	1
Robbery and assaults with intent to rob	0.010	0.100	0	1
Blackmail	0.007	0.082	0	1
Kidnapping	0.003	0.058	0	1
Stealing in a dwelling other than from automatic machines and meters	0.007	0.082	0	1
Theft from vehicle	0.041	0.198	0	1
Stealing from shops and stalls (shoplifting) (1976-)	0.041	0.198	0	1
Stealing from automatic machines and meters (1976-)	0.003	0.058	0	1
Theft or unauthorised taking of motor vehicle	0.054	0.227	0	1
Other stealing and unauthorised takings	0.101	0.302	0	1
Other frauds	0.024	0.152	0	1
Receiving/handling stolen goods	0.017	0.129	0	1
Other criminal Damage	0.007	0.082	0	1
Uttering or possessing counterfeit coin	0.007	0.082	0	1
Violent disorder	0.014	0.116	0	1
Other offences (against the State and Public Order)	0.027	0.162	0	1
Perjury	0.003	0.058	0	1
Gross indecency with a child	0.003	0.058	0	1
Misuse of Drugs	0.020	0.141	0	1
Absconding from lawful custody	0.003	0.058	0	1
Bail Act 1976	0.010	0.100	0	1
Assault	0.027	0.162	0	1
Interference with a motor vehicle	0.007	0.082	0	1
Stealing and unauthorised taking	0.020	0.141	0	1
Criminal and malicious damage	0.003	0.058	0	1
Dangerous driving	0.020	0.141	0	1
Driving licence related offences	0.010	0.100	0	1
Observations	296			

Notes: This table reports the means, standard deviations, minima and maxima of the detailed offences of the two samples from the 1963 (Panel A) and 1968 (Panel B) cohorts of the Offenders Index Cohort Data (Home Office Research, Development and Statistics Directorate) at the time the offenders were sentenced to either youth custody/detention centres/young offender institutions or adults' prisons. The 1963 sample includes offenders who were sentenced either to youth custody/detention centres or to adults' prisons when being age 20/21 at the date of court appearance and who committed their first offence when older than 14. The 1968 sample includes offenders who were sentenced to young offender institutions or adults' prisons when being age 20/21 at the date of court appearance, whose sentence was equal or shorter than one year and who committed an offence before June 1990. If the offender was sentenced for multiple offences at the court appearance, the offence for which the sentence was longer is reported.

2

Empowering Mothers and Promoting Early Childhood Investment: Evidence from a Unique Preschool Program in Ecuador

ABSTRACT

Empowering women and enhancing children's early childhood development are two important goals that are often pursued by independent policy initiatives in developing countries. In this paper we study the consequences of a unique approach that exploits potential dynamic complementarities in pursuing both goals at the same time: empowering mothers through tools that also advance their young children's development. We evaluate the *PelCa* program operated in a poor neighbourhood of Quito, Ecuador, by AVSI, an Italian NGO. Targeted to parents of children from birth to age 5, the program provides family advisor-guided parent training sessions once every two weeks for groups of six to seven mothers with their children. Our evidence compares outcomes for women and children in families that participated in the program to a quasi-experimental control group. Our findings show that the program empowered women in various dimensions: treated mothers are more likely to be employed, more of them have a full time job and they are 19.3 percent more likely to have a formal-sector job. They also earn higher wages, and are more likely to make independent decisions. Treated mothers spend also more time with their children. The program significantly reduces the children's drop-out rates, and increases school grades and scores on cognitive tests, especially for girls.

JEL: I24, I25, I28, J13, J16

2.1 Introduction

Empowering women and enhancing children's early childhood development are two important goals which are often pursued by independent policy initiatives in developing countries. In this paper we study the consequences of a unique approach that exploits potential dynamic complementarities in pursuing both goals at the same time. The approach is based on empowering mothers through tools that also advance their young children's development. The mother's empowerment training relies largely on acquiring knowledge and undertaking home and personal practices that empower her, increase her role in her children's education, and enhance cognitive and non-cognitive skills of the family's young children. The children participate in the program sessions with their mothers. These sessions include joint activities with the mother, and separate activities with other children in the program.

Early childhood education programs have received attention in recent years in research and policy because of accumulating evidence that treatment is both more effective and less costly when it is undertaken at younger ages. However, questions remain about which policy tools offer the most-effective and least-costly ways to improve children's development in the long run.

Similarly, empowering women in developing countries has been also the focus of research and policy because of the recognition that the development process can be enhanced significantly by involving women as equal participants in the community and economy. Doing both - empowering women in a way that also enhances early childhood development - has not been widely studied; however, it could offer an additional valuable policy solution for achieving both goals. This approach offers promising potential to capitalize on the synergy between mother's empowerment and status in her home and wider community, along with the human capital development of her children. Women's empowerment is often defined as "improving the ability of women to access the constituents of development – in particular health, education, earning opportunities, rights, and political participation" (Duflo (2012)). Improving mothers' access to each of these domains can have positive externalities on their off

springs' early childhood development, with accompanying life-long benefits. For example, recent evidence suggests that improving children's health positively affects children's educational outcomes. Evidence of spillover effects from improved child development on the empowerment of their mothers is not yet available but potential mechanisms are offered in the literature.

In this paper we study the consequences of a home-preschool program that aims to enhance both women's empowerment and children's early childhood development. The *PelCa* (*preescolar en la casa* – home-pre-schooling) program started in Pisullì, one of the poorest neighbourhoods of Quito, Ecuador, in 2005. It is run by Association of Volunteers in International Service (AVSI), an international, non-governmental organization that focuses on human development. The program currently involves hundreds of children and mothers. Mothers with children 3 years old and younger were eligible to participate in the program. A qualified family advisor conducted training with groups of six to eight mothers. Children accompanied their mothers in these group parenting sessions, which were held every second week in the NGO offices. Each meeting consisted of three parts. In the first, mothers received structured training focused on strengthening their role in the family, and on learning parenting activities, particularly those that emphasized their children's early development. At the same time, children socialized using educational games and didactic materials. In the second part, family advisors taught both mothers and their children educational activities that could be reproduced at home. In the third part, advisors monitored the homework that had been assigned in the previous meeting.

The program was implemented non-experimentally, but since its initiation in 2005, new families have joined it every year. We use this gradual expansion of the program to select a control group from the list of applicants to the program in 2012. Assuming that new applicants resemble those who joined the program earlier, we selected a control group from the applicants in 2012. Following our suggestion, the NGO made a special effort to reach as many eligible families as possible in 2012. This provided us a large pool of applicants from which we selected our control group, which consisted of families that had an older child in any grade in primary

school and a younger child who would enrol in the program jointly with his mother. This setup can be viewed as a quasi-natural experiment, and we will demonstrate that it yields well-balanced treatment and control groups.

We evaluate the effect on women's empowerment after two to seven years of participation in the program by focusing on mothers' several domains: cognitive and non-cognitive skills, quantity and quality of inputs into child home care and schooling, labour-market participation and earnings, allocation of power within the household, and economic and social independence. We also examine the impact on children's educational outcomes, such as how likely children were to repeat a grade or drop out from school, and how they fared in cognitive tests. Our evidence shows that the program empowered women in various dimensions: mothers who participated in the program between three to seven years are 16.4 percent more likely to be employed, 19.4 percent more likely to have a full-time job, and 19.3 percent more likely to have a formal-sector job. Mothers who had been in the program also earn higher wages ($13.33 per week more in 2013, reflecting a 46.67 percent increase with respect to control mothers), and are 17.8 percent more likely to have their own money and to make independent decisions about how to spend it. Women's autonomy is also reflected in a higher likelihood (10.4 percent) of deciding by themselves whether to work outside of the home, and a higher likelihood (8.5 percent) of being a student in 2012 or 2013. Moreover, there is evidence that these women take on a greater role in overall intra-household decisions, especially on matters involving children's education and discipline. Mothers who participated in the program spend more time with their children. However, they are not more likely to engage in social activities, or to have greater self-esteem.

The program had mixed effects on children: it significantly reduced the drop-out rate and likelihood of temporarily withdrawing from school, and it improved scores in cognitive tests (though the latter is not precisely measured). However, we find no effect on children's attitudes towards schooling (as measured by whether the child indicates that he/she likes school; whether, from the mother's perspective, the child likes school; and whether, as a reward for participating in an interview, the child chooses a book over a toy).

The remainder of our paper is structured as follows. In Section 2 we give an overview of the literature review on early childhood development and on women's empowerment. Section 3 outlines the background and design of the quasi-natural experiment. In Section 4 we describe the data. Section 5 discusses the empirical analysis and results. In Section 6 we explore the potential mechanisms through which results are achieved and in Section 7 we conduct robustness checks. In Section 8 we conclude.

2.2 Literature Review

The present work is related to two different literatures: studies on women's empowerment and studies on early child development. The literature on women's empowerment is more extensive. Kabeer (2005) defines empowerment as the "ability to make choices" in ways that change power relations and affect women's education, employment, and political participation. Duflo (2012) defines women's empowerment as "improving the ability of women to access the constituents of development - in particular health, education, earning opportunities, rights, and political participation." Decision-making within the household is also recognized to be an important indicator of the distribution of power within the household (Alkire 2007, Narayan et al. 2000). We will follow this approach and will explore intra-household decisions, capturing women's power relations within the household and their access to the constituents of development (e.g., if they are allowed to work).

Different channels for empowering women have been explored. Education is sometimes thought to be one of the first drivers of empowerment (Oyitso 2012), but the evidence is mixed. There is substantial evidence that education can improve cognitive skills (fundamental for women's empowerment), aspirations, access to information (to bring awareness of their condition), access to the tools to deal with dis-equilibrium ('face the world') and the ability to use them (Kabeer 2005, LeVine 2001). More educated women also seem less likely to experience domestic violence (Kabeer 2005). This is consistent with the findings in West Bengal by Sen (1999). Mocan and Cannonier (2012) find that an increase in education in

Sierra Leone makes women “more intolerant of practices that conflict with their well-being”. However, whether this change in preferences translates into behaviour is unclear. Andrabi et al. (2012) demonstrate that maternal education positively affects maternal care towards children, but the study does not find an effect on intra-household decision-making.

Women can also be empowered if they accumulate wealth. Microfinance programs that help women gain access to credit can facilitate such accumulation of economic assets. However, evidence on the causal effect of microfinance programs on women empowerment is also mixed. Kabeer (2001, 2005) claims that women's access to credit improves women's self-perception, reduces domestic violence and increases women's power in the household decision-making process. In households where the loan recipient was male, women with some power in decision-making in relation to loan use, enterprise management and the allocation of profits were 20 percent; in female loanee households instead, females were the primary decision maker in 40 percent of the cases, and in total 90 percent of females participated somehow in the decision-making. Banerji et al. (2013) studied the impact of micro-credit in India and find no short- or long-run effects on women's empowerment.

Obviously, more research regarding how to enhance women’s empowerment is still needed. In particular, little is known about the effect of involving mothers in group training. Such group parenting sessions may be a cost-effective method of service delivery but to date this approach has not been properly studied (Baker-Henningham and Lopez Boo, 2010), especially the effect on mothers' longer term well-being and life course outcomes. Our study is the first to focus on long-term exposure to group parenting sessions and to study the impact on both mothers and their children.

The view that interventions at an early age have beneficial long-term effects is gaining empirical support. For example, the Abecedarian project, High Scope Perry Preschool Program, Chicago Child-Parent Centres and the Head Start Program led to improved schooling attainment, and better outcomes in adulthood (as measured by higher earnings, higher employment and lower crime rates). Focusing on educational outcomes, the pre-school treatment of the Abecedarian project affected children more strongly on reading than

mathematics: at age 8 the effect size on treated children was 0.75 for reading and 0.27 for mathematics, equivalent to scores almost 2 years higher for treated children. No significant difference on these outcomes by gender was found. By age 21, females who received the pre-school treatment completed 1.4 years of education more (12.6 vs. 11.3), while males acquired an equal amount of completed years of education (12 in the treatment group, 11.9 in the control group) (Campbell et al., 2002). Overall, grade retention by age 15 was 56% lower for children treated in the Abecedarian project; a similar estimate (60%) was found for children who attended the Chicago Child Parent Centres (Temple and Reynolds, 2007). The High Scope Perry preschool program affected the schooling outcomes of girls only: by age 19 treated females had a higher school GPA and completed a higher grade (Heckman et al., 2010); by age 27 treated females were 30% less likely to be a drop-out from high-school (Nores et al., 2005). However, these experiments target the most-disadvantaged groups. In addition, such programs may be unfeasible in most developing countries because they are expensive. Most related evidence in developing countries is often based on very short interventions and small samples (see Baker-Henningham and Lopez Boo (2010) and Nores and Barnett (2010) for a literature review). Few focus on longer treatment and long-term child outcomes, such as Watanabe et al. (2005) and Kagitcibasi et al. (2009). In both studies there is evidence of positive effects on the cognitive domain; Kagitcibasi et al. (2009) find positive effects on other socio-economic domains as well: children exposed to an early treatment entered the workforce later (due to longer schooling) and found jobs of a higher status when young adults.

2.3 Background and Design

AVSI, the Association of Volunteers in International Service, is an international not-for-profit, non-governmental organization (NGO) based in Italy. Founded in 1972, it operates in 35 countries in Eastern Europe, Africa, Latin America and the Middle East and it operates more than 80 long-term projects. It reached Ecuador in 2001, and its activities relate to infant and child development and education. In 2005 an AVSI branch was opened in Pisullí, a

disadvantaged, urban neighbourhood to the northwest of Quito. In collaboration with Fundación Sembrar, a local non-profit organization, and the local parish, AVSI funded a community development centre where it implements a modified version of *PelCa* (*Preescolar en la casa* - home preschool). The program expanded rapidly, including after-school programs and other services to more than 700 children, youth and their families in 2013. There are more than 50 members on the local staff.

2.3.1 The Intervention

PelCa is a preschool program targeted to parents of children age 5 and under, based on group-parenting sessions. Fortnightly meetings are held in the NGO for small groups (usually six to seven mothers – and occasionally a father or another guardian, such as a grandmother - with their children), under the guidance of a family advisor. In the first part of the meeting, children socialize with each other, playing games using didactic materials, while parents read and discuss material about family education. In the second part of the meeting, parents and children work together: they learn songs, educational games and various development activities that parents can reproduce with their children at home (e.g., reading books, playing with puppets, playing building games, etc.). The family advisor gives every child a notebook of age-appropriate activities that focus on different areas of development, and parents and children are expected to undertake these activities in the two weeks between the program sessions. In the last part of the meeting, the family advisors verify whether tasks that were assigned to the mothers in the previous two weeks were undertaken. The family advisors then verify the learning of each child, monitoring whether they have completed home assignments with the parent (e.g., by having children show drawings, or having children answer questions based on a story that was to be read to them by the parent). The family advisor gives each parent and child reinforcement activities to perform at home in the next two weeks. These activities are

geared towards mothers and children achieving specific targets and goals.[18] These NGO-set goals are the basis for our choices of outcomes that we evaluate in the paper.

Goals for the mother: Acquire more self-confidence and self-awareness and a greater ability to relate to the environment and its people; discover their value and the value of things.

At the personal level: Reawaken your interest in life; become responsible through a personal commitment; increase the perception of your own possibilities and abilities in order to take initiative; value correctly material things and saving.

At the level of relationships with the family and community: Practice patience and reflection; strengthen the level of involvement of each member of the family within the family; share with your partner the need to take responsibility for educational development of the children; develop an aptitude that favours the autonomy of children; build relationships of solidarity with the group in the meetings, and with the neighbours of the neighbourhood.

Goals for the children: Favour the integral growth of the children in their different areas of development (psychometric, language, cognitive, socio-affective).

Families usually acquire knowledge about the program through a poster hung outside the NGO and by word of mouth in the neighbourhood. Once they express interest, AVSI employees visit the family at home in order to collect information on the family circumstances, observe life conditions at home, assess the real need of support, and identify family weaknesses and strengths. Children up to 3 years old are eligible to enter the program (so that he/she can participate in the program for at least two years). The mother commits to participate in fortnightly meetings and to perform at home the assigned tasks. The selection process also takes into account a family's general financial standing and the proximity of the home to the NGO sites where sessions are held. Parents and children can remain in the program until the child is 11 years old but once the child is 5 they move to the NGO *PelCa* school program. The application process for the *PelCa* pre-school program starts at the end of April and lasts for

[18] The goals are taken from the NGO handbook.

two weeks. Approximately 50 families (the number can vary depending on funding for that year) are selected to start the program in September.

2.3.2 Design: choosing a comparison group

Our treatment group includes mothers who enrolled in the *PelCa* preschool program and their children who by now are in primary school. Naturally, these mothers were selected to the program from among many applicants. In the summer of 2012, we selected a control group by mimicking the program's selection process - but at a larger scale. Therefore, the NGO advertised the *PelCa* program in schools and through posters, as it had previously done in the program. However, it extended the period of application to approximately two months so as to reach as many families as possible, and, indeed, it attracted a much larger pool of applicants relative to a regular year.[19] We selected applicant families with a preschool-age child and, similar to our treated mothers, also had at least one child enrolled in primary school. The identifying assumption is that the sample of mothers with children in primary school age who did not participate before in *PelCa* but chose to do so now with a younger preschool age child constitute a good comparison group for representing the counterfactual for *PelCa* mothers and their primary-school-age children.

The families forming the control and treatment groups were invited to an interview in June-July 2012. The mother participated in a structured interview, while her primary school children were tested for cognitive and non-cognitive skills. The mother was asked to bring her children's vaccination certificate and birth certificate which includes a record of the child's birth height, weight and head circumference and the older child's school report cards of the previous and current years. We then selected from among the applicants, those who had an older child at a primary school age. We also held a follow up interview with these control and treatment group a year later in summer 2013.

[19] Usually the application period was closed as soon as the number of admitted families reached the target for that year - which usually was two weeks. However, we needed a much larger number of applicants in order to form an adequate control group.

2.4 Data

The data were collected through face-to-face interviews with mothers and children. The interview was based on a questionnaire we developed for the purpose of this study.[20] The questionnaire provides information on family members (mother, partner and children), demographic characteristics, labour-market activities (type of job, full-time/part-time, formal/informal sector, wage, etc.), intra-household decision-making, and parents' inputs into child rearing. All questions targeted current and retrospective information before enrolment in the program.[21] The interview lasted approximately 45 minutes. The mother was then asked to take the Big Five Personality Test[22] and the Rosenberg self-esteem scale.[23]

Meanwhile, each child took a test of cognitive.[24] Data on weight, height and head circumference at birth of the school age children were gathered through vaccination certificates and birth certificates. For some children, this information was incomplete or unavailable. Eventually we collected data on height at birth for 44 percent of the children, weight at birth for almost 41 percent, and head circumference at birth for almost 38 percent. We think that these low rates preclude the possibility of using these variables for meaningful analysis. The mother was also asked to bring the child's school report cards of 2010-11, 2011-12.

In 2012, 164 children and 115 mothers formed the control group, while 219 children and 166 mothers formed the treatment group: 383 children and 281 mothers in total. We interviewed a few grandmothers who participated in the program on behalf of the mothers, but

[20] We piloted the survey questionnaire in January 2012, interviewing 23 treated mothers: 12 of them had a primary-school-age child who participated in the *PelCa* preschool program and 11 of them with a primary-school-age child that did not participate in the *PelCa* program. We revised the questionnaire following this pilot test.

[21] We can provide details about the questionnaire upon request.

[22] The Big Five Personality Test is based on decades of research. In 1981 these factors became known as the "Big Five" to indicate the broad dimensions to which they refer. It has since been used intensely. It consistently evaluates five broad traits of personality through a series of questions: openness to experience, conscientiousness, extraversion, agreeableness, and neuroticism.

[23] The Rosenberg test was developed in 1965 by Dr. Morris Rosenberg, and it is widely used today by psychologists, sociologists, and social scientists. It has been translated into various languages (e.g., French, Norwegian, Spanish, Portuguese, Chinese, and Italian). Consisting of 10 Likert-type questions, it is used to evaluate the self-esteem of an individual.

we excluded these families from the analysis sample because we do not have grandmothers in the comparison group.

In summer 2013, we conducted follow-up interviews. Ten female interviewers from the area conducted home visits with all mothers in the sample. In order to obtain comparable information in the two rounds of data collection, we used the same questionnaire, but with slight modifications. We added questions about home technology, mother's health, how mothers deal with negative shocks, mother's life satisfaction, and mother's view of the NGO activities. Some questions were added in order to clarify puzzles we encountered in the 2012 survey. However, in all the new questions we also asked about retrospective information. Mothers were asked to bring the vaccination and birth certificates again (because many of these documents had been missing in the previous year) and the school report card of the child for the year 2012/13. The children were assessed again in Spanish and mathematics, using tests appropriate for the student's school grade. The follow-up sample included 136 control children (82.93 percent) and 98 control mothers (85.22 percent), while the treated sample cover up included 197 children (89.54 percent) and 150 mothers (89.82 percent).

2.4.1 Treatment-Control Comparisons: Balancing Tests

We examine in this section if pre-treatment covariates are balanced between treatment and control groups. The evidence suggests that mothers and children in both groups are very similar on observed and predetermined characteristics, supporting our view of the empirical setup as a quasi-natural experiment. The first two columns of Tables 2.1- 2.2 display the means for the treatment and control groups, while the last two columns present the difference in means between the two groups and its standard error. With respect to the child characteristics (Table 2.2), we can see that child age is unbalanced. This is probably due to the fact that we selected children from 1st to the 7th grades and that, as we will see, control children are more likely to repeat the school grade and therefore be older. The F-test on the significance of all the characteristics together suggests that overall children's characteristics are not linearly correlated with treatment status.

Mothers' characteristics (Table 2.1) are balanced in most of the dimensions, except that control mothers were more likely to be employed before treatment: 47.0 percent of treated mothers were working versus 60.9 percent of control mothers. Control mothers were also more likely to be working full time. The F-test on all of the maternal characteristics[25] before treatment is significantly different from zero. This is likely to be driven by the imbalance in the previous working condition of the mothers; as anticipated, we will control for this difference in multiple ways.

Pre-treatment paternal characteristics and most of the household characteristics are well balanced,[26] with a few exceptions: whether the family owned a house, the number of rooms and the availability of drinkable water in the house. Overall, 10 percent of the pre-treatment characteristics differences are significantly different at 10 percent level of significance. We therefore will include in the regressions pre-treatment control variables to capture these differences between the treatment and control groups. It will be shown that the estimates are not sensitive to adding these controls.

2.4.2 Entropy Balancing

One way to control for the differences in some of the pre-treatment characteristics is to use entropy balancing, as described in Hainmueller (2012). Entropy balancing is a data-preprocessing method to achieve covariate balance. It computes the means (or higher moments of covariate distributions) of the covariates in the treatment group and looks for a set of entropy reweights so that the means in the reweighted control group match the means in the treatment group. We implement entropy balancing for the means of the covariates that we will include as control variables in our analysis (child pre-treatment characteristics, household pre-treatment demographic characteristics and household pre-treatment economic characteristics).[27]

[25] Pre-treatment characteristics related to intra-household decisions before treatment are included.
[26] Details are provided in the Appendix Tables A1- A2.
[27] Among these covariates, we also include an indicator of whether mothers were working full time before treatment.

Entropy balancing makes the treatment-control covariate balance almost perfect: differences in means are not significantly different from zero for all covariates (Appendix Table A2.3).[28] This approach is preferred over a propensity-score matching because the former eliminates all treatment-control imbalances. In addition, the propensity-score matching requires treated and controlled units to be comparable within the common support. As a consequence, the individuals who do not lie in the common support (5 out of 281 mothers in 2012 and 22 out of 496 mothers in the pooled data) are dropped from the sample.[29]

2.5 Empirical Strategy and Results

2.5.1 Empirical Strategy

We estimate the effect of participating in the program on the outcomes of interest using the following regression model:

$$y_{it} = \beta_0 + \beta_1 Treatment_i + \beta_2 ChildCharacteristics_i + \beta_3 HouseholdDemographics_i + \beta_4 HouseholdEconomics_i + \beta_5 TimeFE_t + \beta_6 SchoolFE_t + \varepsilon_{it}$$

where i is the individual and t is time. y_{it} is a vector of maternal and child outcomes of interest. Since we face a multiple outcomes problem, we will also compute summary indices[30][31] for domains of outcomes. $Treatment_i$ is a dummy equal to 1 when mother and child participate in the *PelCa* program and 0 otherwise. In order to shed light on heterogeneous treatment effects by number of years of participation in the program, we will also use the specification outlined above with two treatment dummies, one indicating longer treatment exposure, and a second indicating shorter exposure. Exposure varies from two years and four

[28] We also obtain balanced samples through entropy balancing when we consider the children's samples or when we pool the two years of data together.
[29] For purposes of robustness, we also re-estimated the effects of the program through a propensity score matching, with and without replacement, and the results are very similar to the estimates we present in the paper (these results are available upon request).
[30] We followed Kling et al. (2007) to construct each summary index as an "equally weighted average of z-scores of its components, with the sign of each measure oriented [...] so that more beneficial outcomes have higher scores. The z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation."
[31] We developed a Stata package "mseffect" to calculate the mean effect size on the summary index with the advantage that we account for different weights, reversibility of outcome sign, and different types of robust standard errors.

months to seven years and eleven months. We divide the sample to two treatment groups with a dividing line at six years of exposure by 2012. This implies that 87 mothers participated in the program for less than six years.

$The\ ChildCharacteristics_i$ vector includes year of birth, birth order, number of siblings as of 2005, i.e. before the program started, and gender. $HouseholdDemographics_i$ are pre-treatment household demographic characteristics: mother's and father's age, their civil status (married, lived together, mother was single) at the time of the birth of the first child, and the parents' level of education before the birth of the first child, a dummy equal to 1 if the mother was born in Quito, a dummy equal to 1 if the parents came from the same city and the number of children the mother had in 2005. $HouseholdEconomics_i$ are pre-treatment household economic characteristics: whether the mother worked before treatment, whether the father worked, and the mean firm size of mother's and father's employer, average monthly family income before treatment. $TimeFE_t$ is a dummy equal to 1 when the observation corresponds to 2013, 0 if 2012; ε_{it} is the error term, clustered at the mother level when we run regressions pooling the observations in the two years together or when we analyse outcomes for children. $SchoolFE_t$ are school fixed effects.[32] They are included when we analyse outcomes for children. More detailed description of the control variables is provided in the Appendix.

2.5.2 Results based on Summary Indices

As we note in Section 3.1, the primary purposes of the *PelCa* program are to empower mothers, harmonize intra-family relations, and increase early childhood investment in health and education. The breadth of the goals implies that the tangible consequences of the program can be analysed from a variety of angles, and, for each of these angles, we can examine multiple dimensions. We decide to measure the following domains, each of which is composed of multiple outcomes for mothers and children. For mothers, the domains (and specific outcomes)

[32] 55 are the schools that children attend in 2012.

are: labour-market outcomes (whether the mother is working, working full-time, working with a contract and average family monthly income), mothers' economic and social independence (whether the mother has her own money, participates in voluntary activities, is currently studying and whether the mother alone or together with the partner decides on her working conditions), mothers' intra-household decision-making (mother or both mother and father decide what to do on child's education, mother/both decide what to do when the child is ill, mother/both decide on children's discipline, mother/both decide on spending, mother/both decide on food spending, mother/both decide on having children, mother/both decide on contraceptives, mother/both decide on important items, mother/both decide on mother's health and mother/both decide if mothers can visit), mothers' self-esteem and Big Five personality traits (Rosenberg scale, agreeableness, conscientiousness, extraversion, neuroticism and openness to experience), mothers' care of children (mother's time inputs for child, weekly help from mother, mother's aspirations for child's education, mother's expectations for child's education and whether her child feels that she helps him) and fertility choices (whether the mother is pregnant and whether she would like more children).

For children, the domains (and outcomes) are: test scores and report card grades (language test score, mathematics test score, report card mathematics grade and report card language grade), schooling dropout and grade repetition, attitude towards schooling (whether the child likes school, whether the child likes school from mother's prospective, whether the child feels that his parents demand children to follow certain behavioural rules and whether the child chose a book as gift[33]).

Before presenting the detailed estimates of the effects on each specific outcome, we analyse each domain by creating domain-specific summary indices. This allows us to control for the potential problem of over-rejection of the null hypothesis due to multiple inference. Because different outcomes have different data scales, simply averaging the estimators for the treatment effect is not likely to produce a meaningful statistic. To address this concern, we

[33] At the end of each interview, children were offered a gift. They could choose between a book and a game. We interpret the choice of a book as interest in schooling activities.

follow the summary-index approach as in Kling et al. (2007). The summary index of multiple outcomes is the average of z-scores of each outcome variable. Z-scores are calculated by subtracting the control mean from the outcome and dividing by the control standard deviation. Indeed, the summary index is a special case of the z-score[34] and is identical to the mean effect size of treatment if there is no missing value. In the regression specification this approach yields standardized estimators as follows: the treatment effects for K outcomes are aggregated and reflected in a single standard normal statistic,

$$\tau = \frac{1}{K}\sum_{k}\frac{\boldsymbol{\beta}_{1\mathrm{k}}}{\sigma_{k_C}}, \quad k = 1, \dots, K$$

where $\boldsymbol{\beta}_{1k}$ indicates the average treatment effect for outcome k and σ_{k_C} denotes the standard deviation of the k^{th} control outcome.[35] By doing so, the above equation can be thought of as a point estimator representing a collection of standardized treatment effects. In general, the sign of the summary index reveals information on the direction of the aggregate impact of a class of outcomes, and the more the summary index deviates from zero, the stronger is the implied aggregate effect.

We also construct a separate summary index for each year in order to give a general understanding of the domain in that specific year. Analysing these summary indices[36] in order to examine the effect on empowerment of mothers, we find evidence of a positive treatment effect on many domains. The program enhances mother's participation in the labour market; the corresponding summary index in 2012-13 is 0.503, positive and statistically different from zero at the 99 percent confidence interval (Table 2.4, Panel A). A large effect (0.276) is also observed on mothers' economic and social independence (Table 2.4, Panel B). A similar

[34] Here we replace the minuend and the divisor in the z-score by the control group mean and standard deviation respectively. In another words, we do require some dispersion in the controlled outcomes to guarantee the validity of standardization.

[35] Having included the covariates, the K average treatment effects ($\boldsymbol{\beta_1}$) and sample variances can be easily acquired through a linear regression. However, this paper also take account of the covariance of effects and therefore adapt a seemingly uncorrelated regression (O'Brien, 1984; Kling et al., 2007):

$$\mathbf{Y} = \boldsymbol{I_K}\otimes(\boldsymbol{T} \quad \boldsymbol{X})\boldsymbol{\beta} + \boldsymbol{\upsilon}$$

where $\boldsymbol{T}$ is the treatment indicator(s), and $\boldsymbol{X}$ consists of controlled regressors as well as a constant term.

[36] Estimated effects on summary indices of mothers are presented in the first row of Tables 2.3-2.6 and of Appendix Tables A2.5-A2.6.

positive pattern is evident for the household decision-making outcomes (Table 2.6), albeit the estimated coefficient for the summary index is smaller in size (0.093). We note that the estimates are robust to the inclusion of control variables and also to a re-weighting with entropy balancing. Taken together, these results suggest that probing further investigation on each specific aspect of mothers' outcomes would be of interest. We do not find a strong treatment effect on mothers' self-esteem or on personality traits (openness to experience, conscientiousness, extraversion, agreeableness, neuroticism): the summary index coefficient capturing the program effect on these aspects is trivial (0.040) and not precisely estimated. On the contrary, access to credit was shown to improve women's self-esteem by Kabeer 2001. However, our findings do not necessarily imply the absence of a treatment effect on self-esteem or personality traits, as it could also be that the instruments used to measure these outcomes were not the most appropriate.

The program encouraged boosting parental inputs into early child development. In examining the program's effects on childhood development, the overall index capturing mothers' care towards children did not show improved results.[37] However, four measures of children outcomes (test scores, report cards, drop-out rates, repeating grades) showed improvements for children who were in the program. One measure (attitude toward school) showed no change. It is therefore likely that the better children schooling outcomes resulted from other improvements generated by the program such as the increase in family income, and the direct cognitive and non-cognitive skills training that the children received in the biweekly meetings with the family adviser. The treatment effects on children's outcomes are presented in Table 2.8. Children in the *PelCa* program made progress in test scores and report card grades, even though the estimate is not precisely measured (0.163, se=0.122). Treated children are significantly less likely to drop out from school or repeat the grade (-0.182, se=0.073). However, attitudes toward schooling among treated children did not improve by a large extent:

[37] The five relevant outcomes are mother's time inputs toward the child, weekly help from mothers, mother's aspirations for child's education, mother's expectation for child education, whether the child feels his/her mother's help (Appendix Table A2. 5).

the estimated mean effect on the summary index is only 0.026 (se=0.071). The last row of Table 2.8 exhibits an overall index that aggregates all the outcomes of children[38]: its coefficient is 0.206 and it is statistically significant at 1 percent level even when all control variables are included. This estimate confirms an overall positive impact of the *PelCa* program on children, too.

In the rest of the paper we will further investigate program-treatment effect on the individual outcomes that we aggregate for each of the mothers' indices. First we will present and discuss estimates based on the full sample, with and without reweighting the pre-treatment covariates. Second, we will check if our results hold when the sample is reweighted through entropy balancing. Finally, we will perform other robustness checks.

Next, in order to gain a better insight on the treatment effects on mothers, we explore treatment heterogeneous effects. Table 2.7 presents the aggregate-estimated effect on the summary indices with control for all covariates and for subsamples of mothers. In columns 3-4, we present estimates by mother's education as measured before enrolment in the program, in two sub-samples, mothers who completed up to primary school and mothers with more than primary school education. These are almost equal samples, 136 and 144, respectively. At a first glance, treated mothers with a higher education have better labour market outcomes (the estimated coefficient is 0.611) than their less-educated counterparts (0.462), both estimates being statistically significant at 1 percent level. Similarly, mothers who initially had higher levels of education are also more likely to have a role in family decision-making: the mean effect size for mother's "decision power" in the household decision-making process is 0.205 (se=0.087) for more educated mothers relative to 0.098 (se=0.076) for mothers with a lower education. However, the F-tests on treatment effect differences in labour-market outcomes and household decision-making (in square brackets) show that these differences are not statistically significant at 10 percent. It is interesting that the mean effect on the summary index of all mothers' economic and social independence outcomes is higher for mothers with a lower

[38] We reverse the sign for adverse outcomes of schooling dropout and grade repetition when calculating the overall summary index on children's outcomes.

education (0.420 vs. 0.386), however, the F-test statistic suggests that they are not statistically different from each other in this domain.

Next, we explore heterogeneous treatment effects by mothers' pre-treatment role in decision-making. One may also expect that mothers who initially were less involved in family decision-making will benefit more from the program in this regard. To identify these mothers, we divide the full sample into two: mothers who had more impact, and mothers who had less impact on intra-household decisions up to 2005.[39] The evidence presented in Table 2.7, columns 5-6, suggests that the latter group had larger gains in the labour market, the economic and social independence domain and household decision-making. The estimated effect for both groups on labour-market outcomes is large, 0.729 (se=0.145) and 0.394 (se=0.131), respectively. The summary index, which normalizes the aggregate treatment effects into [0, 1] interval, shows a truly large estimated impact (0.729) on mothers who had a lesser role in family decision-making before joining the program. In addition, the F-test for the difference between these two groups strongly rejects the hypothesis that the effect on the labour- market-outcomes index is equal. With respect to the effect on decision-making in the family, findings show similar differences between the two groups, with a result of 0.197 (se=0.073) for the first group and an estimate close to zero for the second group. The F-statistic (2.987) implies that the difference between the two groups is statistically significant. Finally, mothers who entered the program with a lesser role in family decision-making have slightly better economic and social independence outcomes (0.482 vs. 0.415), but the difference is not significantly different from zero.

We also examined the subsamples of mothers by pre-treatment working conditions, and we present this evidence in columns 7-8 of Table 2.8. Among the group of treated mothers, 148 reported that they were working when their interviewed child was born, and 133 who reported they were not working at that time. The estimated results for the two subgroups show

[39] We made use of pre-treatment variables of mothers' intra-household decisions. After calculating the number of total household decisions that mothers made in 2005, we divide the full sample into two by the mean of total decisions. In the 2012 sample of mothers there are 179 and 93 mothers who made more decisions and fewer decisions respectively.

that mothers who were not working before joining the *PelCa* program, are enjoying more job opportunities compared with treated mothers who were already working before. Their summary indices integrating 7 related outcomes are 0.794 (se=0.191) and 0.340 (se=0.126) respectively, with the F-test on their difference strongly rejecting their similarity at 1 percent significance level.

In terms of intra-household decisions, the same pattern emerges between the two treated subsamples. Although the F-statistic is less significant, the estimated effect for mothers who were not working before is greater in size (0.213) and statistically significant at the 5 percent level; compared to the effect for mothers who previously were working of 0.086, a figure that is smaller and statistically insignificant. The program has the same effect for the two groups on economic and social independence outcomes (0.484 in the sample of mothers who were not working before and 0.351 for mothers who had worked before).

2.5.3 Detailed Effects on Mothers' Outcomes

Labour-market Outcomes

Labour-market outcomes epitomize the empowerment and emancipation of women. As we pointed out in the previous section, the summary index in Table 2.4, Panel A suggests an overall significant improvement in mothers' employability and family income. In 2012 treated mothers are 17.6 percent more likely to be working, 20.7 percent more likely to be working full-time and 20.4 percent more likely to be working in the formal sector (Table 2.3, Panel A). The percentage effects relative to the untreated mothers in terms of employment, full-time employment and formal employment are 36.1 percent, 132.3 percent and 234.5 percent, indicating a very large increase in mother's employability. When we pool 2012 and 2013 observations together, and cluster the standard errors at the mother level (Table 2.4, Panel A), we find that treated mothers are still 16.4 percent more likely to be working, 19.4 percent more likely to be working full-time and 19.3 percent more likely to be working in the formal

sector.[40] These estimated coefficients are significantly different from zero at the 1 percent significance level, and they are not affected by adding any of the control variables in the regression. These estimates reflect a large gain relative to the control group mean.

We estimated the treatment effect in sub-samples stratified by number of years in the program. The first group consists of mothers who were in the program for six years or more, and the other group consists of mothers who were in the program for less than six years. We present the estimates, based on the summary index as outcomes, in the first two columns of Table 2.7. These estimates show a larger effect on labour-market outcomes for the group who participated in the program for a longer period of time, but the treatment difference is small and not significantly different from zero (0.545 vs. 0.450). A similar pattern is seen with the estimated impact on the disaggregated outcomes. Mothers who participated longer are 20.6 percent more likely to be employed, 25 percent more likely to be full-time workers, and 23.6 percent more likely to be hired in the formal sector; the corresponding treatment effects for mothers who spent less time in the program are 15.1 percent, 16.7 percent and 17.2 percent. The pooled-data analysis confirms 2012 results.[41]

Moreover, we find that treated mothers have more stable employment: 69.33 percent of the working mothers from the program were working in both 2012 and 2013, whereas only 48.97 percent of the working mothers in the control group were working in both 2012 and 2013. Another important result shows that the 2012 average monthly income of treated families is $44.50 higher (Table 2.3, Panel A) than the families in the control group – a finding that we attribute largely to increased wages for mothers. The median wage of workers in the neighbourhood is the minimum wage ($292 per month in 2012) and, therefore, the income gain from the program is large. In 2013 we collected the data on mothers' wages. Using this information we estimate that family income is up mainly because an increase in mothers' wages: treated mothers earn $13.30 more per week than control mothers, i.e. more than $57

[40] The types of jobs that treated mothers hold are typically low-skilled jobs: mainly domestic cleaners, but also seamstresses and shopkeepers. More details on the job categories are available upon request.
[41] These results are available upon request.

extra per month (Table 2.4, Panel A), while fathers' wages are not significantly different in the two groups.

Economic and Social Independence Outcomes

This section examines the effects of the *PelCa* program on mothers' independence from both an economic and social perspective. As outlined above, the relevant summary index estimated in Table 2.4, Panel B, shows a strong aggregate impact of the *PelCa* program on mothers' independence (0.276), with a p-value smaller than 1 percent. In Panel B of Tables 2.3 and 2.4, we see that treated mothers in 2012 are 22.2 percent (se=0.064) more likely to have their own money relative to 44.7 percent (49.6 percent increase) of control mothers. The increase is 17.8 percent (se=0.051) when we pool the 2012 and 2013 data together relative to 48.7 percent (36.6 percent increase) of control mothers. Again, these effects are unchanged when we add to the regression each set of control variables.

Another sign of the program's effect on women's empowerment is that 8.5 percent of treated mothers are studying at the survey date, which is about 160 percent point higher then controlled mothers. This estimate is significant at the 1 percent level and holds even once we add all the control variables. For both 2012 and 2013, 6.71 percent of treated mothers were studying, whereas only one mother from the control group was studying in both years. A concern may be that entering the job market may lower the incentives and time for studying, and mothers who participated in the program were more likely to be working. To address this issue, we check whether mothers who gave up studying in 2013 also found a job in 2013; we find little evidence of this.

Another sign of the program's effect on women's empowerment regards her say about her working status. In the 2012 survey, we asked mothers who decides whether they can work. Treated mothers were 13.2 percent (se=0.044) more likely to answer that the decision about whether they can work outside of the home is made either on their own or together with their partner. The estimated impact relative to the mean of control group is 16.4 percentage points higher. This holds true when we pool data from the 2012 and 2013 surveys: treated mothers

are 10.4 percent (se=0.031) more likely to have a role in this decision. We find no significant difference in this change by the number of years mothers are in the program (Table 2.7, columns 1-2), though the estimate is larger for mothers who have remained in the program for the longest period of time (0.397) than for other mothers (0.288). Both estimates are statistically significant at the 1 percent level, but not significantly different from each other.

Household Decision-Making Outcomes

In the first rows of Tables 2.5 and 2.6, we show that the estimated impact on the summary index for the household decision-making domain is strictly positive (0.126 in 2012 and 0.093 in the pooled data) and statistically significant at the 10 percent level.[42] Focusing here on the individual outcomes that make this index, we estimate that treated mothers are more likely to make decisions alone or with their partners about issues related children's education (9.9 percent effect, significant at the 1 percent level) and on children's discipline (8.7 percent effect, significant at the 5 percent level), which are 11.6 percentage and 10.4 percentage greater, respectively, than the outcome means of control mothers. These estimated effects remain unchanged when controls are added. In the 2012 and 2013 pooled data, we estimated similar effects. However, when it comes to other domains (intra-household decisions on spending, on having children, on contraceptives and on what to do when children are ill), we find little difference between control and treated mothers. In the follow-up survey in 2013, we also collected intra-household decision outcomes on who decides on issues related to mothers' health, on purchasing important items and on whether mothers can visit friends and relatives. Effects on these intra-household decisions are non-conclusive. When stratifying the sample by length of participation in the program, we find that mothers who have been in the program for a longer period of time are more likely to be involved in household decision-making.

The estimated effect on the summary index for more-treated mothers is 0.210 in 2012 (significant at the 1 percent level), while being negligible and statistically insignificant for the

[42] The estimated index in the pooled data is smaller and less significant because we include three additional outcomes from 2013. These additions have insignificant effects.

less-treated (Table 2.7). The above facts hold true when we add or remove controls and/or pool the two years together. The F-statistic on the difference-in-treatment effects between the two groups is 1.700 (significant at the 10 percent level), which suggests that the longer mothers stay in the program the greater their empowerment within in the family.

Together with the results based on the single-treatment dummy, it seems safe to conclude that mothers treated with the *PelCa* program assume greater intra-household responsibilities and participate more fully in intra-household decision-making.

Access to credit was also found to have a positive effect on women's power in household decision-making, for decisions related to the loan (Kabeer 2001, 2005). The effect of access to credit seems higher than what we find here. However, this is due to the lower initial power of women who received the loan in the analysis by Kabeer (2001, 2005), where 20 percent of women in the comparison group had some sort of role in decision-making, compared to the women in our study, where even before the treatment 70/80 percent of women had some power within the household.

2.5.4 Effect on Children Outcomes

The summary indices on children's outcomes are summarized in Table 2.8, columns 1-3: the overall estimated effect that aggregates all children outcomes is 0.206 (se=0.080), suggesting that children appear to be positively affected by participating in the *PelCa* program together with their mothers. The index aggregating test scores and report card grades in Spanish language and mathematics is 0.163 when including all the controls. This implies that treated children probably have slightly higher school records. The effect becomes clearer when we divide the children's sample by gender.[43] Female students in the *Pelca* program appear to have a better performance (0.313, se=0.160) than male students (0.195, se=0.150) on this domain, although the difference between the two groups is statistically insignificant. This is consistent

[43] Table 2.9 reports the heterogeneous effects on children's outcomes by gender and mothers' education.
In columns 1 and 2, the estimated summary indices are based on 191 female and 192 male students separately.

with the effects of the High Scope Perry Preschool program on schooling, which are stronger for female students (Heckman et al., 2010).

We also sub-classify the children's sample by their mothers' educational levels (Table 2.9, columns 3-4).[44] Interestingly, children whose mothers have acquired less education obtain higher cognitive test scores and report-card grades (0.390, se=0.130) compared with children with more educated mothers (-0.323, se=0.235). F-statistics for group differences in the treatment effects are reported in square brackets and show that the estimated impacts for the two groups are significantly different. In addition, those more educated mothers were more likely to be employed initially and most likely would have still been working during the *Pelca* program, and therefore perhaps spent less time with their children. The estimates presented in Table 2.9 (columns 5-6) support this expectation. We stratify the sample by mother's pre-treatment employment status and find that children with unemployed mothers at baseline had significantly higher test scores and report card grades (0.458 vs. 0.0114).

For children's school dropout rates and grade repetition, the mean effect size is -0.192 (se=0.082) when we exclude covariates; it becomes -0.182 (se=0.073) when adding all the relevant controls, indicating that treated children generally have better educational attainment. In Table 2.9, we find that estimated impacts on female students and male students are similar: girls are less likely to dropout or repeat the grade (-0.256 se=0.089) compared to boys (-0.087 se=0.116), but not significantly so. The result goes in the direction of other pre-school programs such as the Abecedarian project, the High Scope Perry Preschool program and the Chicago Child-Parent Centres, where grade retention is significantly lower for treated students (Nores et al., 2005; Temple and Reynolds, 2007).

The estimated influence on children's attitude towards schooling is unclear (0.026). However, if we sub-classify the sample by gender, we find evidence of a positive program effect on the attitude toward schooling among male participants. The estimated index is 0.142

[44] We divide the full sample of children into two groups according to whether their mothers have completed primary school; our data show that 210 mothers finished primary school, and 174 mothers did not.

(se=0.097) on boys, while the effect on girls is almost zero (0.008) and statistically insignificant.

2.6 Robustness Checks

2.6.1 Evidence on Entropy Balancing

In this section we examine whether treatment effects remain even when reweighting the sample with entropy balancing, The results after imposing entropy balancing are reported in columns 4-6 of Tables 2.3-2.6, Table 2.8 and Appendix Tables A2.3-A2.7.

With respect to mothers' outcomes, we also calculate summary indices for the reweighted sample. The estimated effects on the indices of mothers' labour-market outcomes, independence and household decision-making exhibit similar patterns to those previously found. We note that the estimated impacts on summary indices tend to suggest a slightly larger overall effect on mothers' employment in 2012 (0.577 vs. 0.482 in Table 2.3). However, the 95 percent confidence interval around the estimated coefficient with re-weighting (0.388, 0.766) overlaps with the confidence interval without re-weighting. The pooled data in Table 2.4 do tell the same story. When it comes to disaggregated labour-market outcomes of mothers, the estimated parameters remain roughly the same: in 2012 treated mothers are 21.6 percent more likely to be working (the estimated coefficient in the reweighted sample is slightly higher than in the sample without reweighting, where we estimated a 17.6 percent increase), 23.4 percent more likely to be working full time (20.7 percent without reweighting), and 22.0 percent more likely to be working in the formal sector (20.4 percent without reweighting). These estimates are not statistically different from the estimates in the original sample. The same pattern is observed regarding the estimated effects on average monthly family wage in 2012, and mothers' wage in 2013.

The estimated effect on mother's independence does not change much either when we use the entropy re-weighting. Treated mothers are 20.5 percent more likely to control their own money, consistent with the 22.2 percent likelihood we found earlier. The effects of the program

on other outcomes are also similar to previous results: treated mothers in the reweighted sample are approximately 9.5 percent more likely to be studying either in 2012 or 2013 or both; and they are about 11.6 percent more likely to participate in making the decision on their job status. We find small effects on mothers' engagement in social voluntary activities. Again, the interval estimates at 1 percent for economic and social independence outcomes based on the reweighted sample overlap greatly with the estimates using the original sample.[45]

The estimated effects on intra-household decision-making also exhibit the same pattern with entropy re-weighting: positive effect on mother's power in the decisions on children's education and children's discipline (columns 4-5 in Tables 2.5-2.6) and no significant effect on other decision-making outcomes. The estimated aggregate effect on intra-household decision-making in 2012 is 0.124 (statistically significant at the 1 percent level), which is, again, almost identical to the previous result without re-weighting (0.126). In the pooled data, the estimated effects on the indices using the weighted data are smaller and less significant, which is consistent to our finding using the unweighted data, partly because the effects on the additional outcomes in the panel data are negligible.

We can see the effects on the children's sample adjusted by entropy balancing in Table 2.8 (columns 4-6). We find the very same pattern that we found in the unadjusted data. As before, the estimated treatment impact on children's attitude towards schooling is still vague. The estimates on test scores, report card grades and educational attainment are also consistent with those found in the original sample. Their interval estimates at the 5 percent level overlap with point estimates of the original sample (with or without covariates).

We also check whether improved women's status hinders men's status in the family. Reassuringly, we find no evidence of a change in the economic status of fathers with entropy-balancing either.

2.6.2 Additional Robustness Checks

[45] This is true both for 2012 data and the pooled data.

Our analysis relies on the comparability between treatment and control groups. When we examined pre-treatment characteristics, we found that 10 percent were not balanced at the 10 percent significance level. We interpreted this difference in observable pre-treatment characteristics as random, in support of the two groups being comparable. Our identification strategy contributes to the comparability of the two groups by controlling for the self-selection coming from the willingness to participate in such a program: only mothers who were willing to participate in the program could enter the control or the treatment group. The validity of our identification strategy lies on the assumption that mothers who decided to participate in 2012 did not participate before for reasons that will not affect the outcomes we analysed. However, if control mothers were not willing to participate before 2012 because, for example, they were more likely to be working[46] the imbalance in the previous working condition would be non-random and might affect later outcomes, hindering our analysis. In order to check for this, we re-do the analysis by subsamples: first we analyse the treatment effects in the group of treated and control mothers who were not working at baseline, and secondly in a sub-sample of treated and control mothers who were working at baseline (2005). The estimated treatment effects obtained from these two sub-samples are very similar to each other and to the estimates we obtained from the full sample. This evidence rules out the possibility that our results are driven by non-comparable treatment and control groups in terms of employment rate at baseline.

It is still important however to understand why control group mothers did not enrol in the program previously. From the pie chart in Figure 2.2, we can see that most of the mothers did not enrol in the program because they did not know about it (44.55 percent); because they lived too far from the NGO offices (25.74 percent); due to issues related to their application forms (for example, lost application forms (11.88 percent), and due to previous affiliations with other NGOs in the area (8.91 percent). These answers exclude the possibility that mothers chose not to enrol in the program because they questioned its effectiveness as this is not one of the

[46] We have already shown that treated mothers were less likely to be working before, 47.3 percent vs. 60.9 percent (Table 2.1).

reasons stated. We also note here that none of these is correlated with the previous working condition or with working full-time before.

As a further robustness check, we re-estimate all models by limiting the sample to the control mothers who did not enrol in the program either because they were already affiliated with another NGO, or because there was a problem with their application forms. The first group of control mothers, if anything, should be more attentive than treated mothers. The second group can be interpreted as randomly assigned to the control group. Unfortunately the sample here is very small (21 mothers), but the estimated effects we obtain from this sample are qualitatively similar to those obtained from the full sample but they are much less precisely estimated.

As another robustness check, we also re-estimated the various models using propensity score matching (with replacement). The mean bias is reduced to 6.7 percent and the median bias to 4.0 percent. The sample consists of 58 treated mothers and 90 control mothers. We also estimate a propensity score matching without replacement: the mean bias is reduced to 5.4 percent and the median bias to 5.0 percent. The sample consists of 90 treated mothers and 90 control mothers. As we already indicated above, we prefer entropy balancing which allow maintaining the sample size, even though when we reduce the sample size through propensity score matching, we obtain very similar results.

Finally, we do not have data on dropout rates from the program, but potential attrition could make the treatment mothers a selective group. To check for this we exploit the fact that the sample of treated mothers who stay in the program for a shorter time is less selective than the sample of treated mothers who stay for longer. Hence, as a further robustness check we re-run our analysis including a dummy for treatment years above the median. As presented in column 1 of Table A2.8, estimated mean effect sizes of longer treatment exposure with respect to shorter exposure on both mothers' outcomes (Panel A) and children's outcomes (Panel B) are nearly zero and are very statistically insignificant. The estimated coefficients on the treatment variable (column 2), measuring the impact of being treated shortly compared to the control group, are generally quite similar to the effects of the whole treatment group as we

discussed earlier. Above facts hold true when we use the pooled data or look into each maternal and child specific outcome. Therefore it is safe to conclude that the treatment effect is not driven by treated mothers or children who have been in the NGO for longer.

2.7 Conclusions

In the current study we analyse an innovative method to empower women and help children to develop: *PelCa*, a home-preschool program that involves mothers, putting them at the centre of their children's education. Relying on a quasi-natural experiment we are able to identify and measure the effects of the program on both mothers and children.

First, we find that the intervention empowers women across different domains. It facilitates their entry in the labour market: treated mothers are much more likely to be working, more likely to be working full-time and more likely to be working in the formal sector. All of these estimates are significantly different from zero at the 1 percent level and robust to the inclusion of covariates. Moreover, treated mothers become more financially independent, and more likely to possess money for which they make the sole spending decisions; to be studying; and to decide whether they can work outside of the home. The treatment further modifies the allocation of power in the house: mothers become more likely to take part in decisions on children's education and to take part in decisions on children's discipline. However, we find no effect on mother’s role in decisions on what to do when the child is ill, on various types of expenditures, on having children, or on the use of contraceptives. Treatment intensity plays a role here: the longer the mother stays in the program, the more empowered within the household she becomes. Moreover, mothers who were less powerful at baseline are the ones who gain the most power in the household following participation in treatment.

All of the above results hold when we estimate aggregate treatment impacts, using summary indices instead of individual outcomes, in order to account for multiple inference, when we use entropy balancing to adjust for differences in pre-treatment covariates, and when we use other robustness checks.

We also evaluate the program's impact on children. We firstly probe into children's cognitive tests. The estimated impact on the summary index of cognitive achievement of treated children is 0.163 (se=0.122), marginally significant. More specifically, the estimations by subgroups allow us to track the source of these effects: girls participating in the program appear to make more progress (0.313, se=0.092) in test scores and report card grades than boys (0.195, se=0.095), even though the estimates are not significantly different; children whose mothers have a lower educational attainment benefit much more from the program in terms of improved cognitive tests (0.390, se=0.130) than children of more-educated mothers (-0.323, se=0.235). Our findings suggest the existence of differential treatment effects based on mothers' pre-treatment working status – kids with less-educated mothers in 2005 are helped more by the program regarding their cognitive test.

Moreover, consistent with findings from other pre-school programs, students in the *Pelca* program are much less likely to drop out of school or to repeat a grade. These effects are marginally larger for female students, but the difference between genders is not significant. Although we do not find a treatment effect on students' attitudes toward schooling, the overall summary index aggregating all children's outcomes confirms that children tend to be positively affected by participating in the *PelCa* program. Overall, there is evidence that the home-preschool program that we study here helped mothers raise their children in a more learning conducive environment which led to positive effects on children, as well as on empowering mothers, both in society and within their household.

Appendix

Construction of control variables

Household demographics controls

• Mothers' and fathers' education (separately): dummy primary not completed, dummy primary completed, dummy secondary not completed, dummy secondary completed, dummy university (these dummies take value 0 if the variable on previous education was missing) + dummy “unknown education” equal to 1 if the variable on previous education was missing

• Mother age (equal to minimum age if missing value) + one dummy equal to 1 if the variable is missing

• Father age (equal to minimum age if missing value) + one dummy equal to 1 if the variable is missing

• Dummy mother was married, dummy mother cohabitated, dummy mother was single (these dummies take value 0 if the variable on previous civil status was missing) + one dummy “unknown status” equal to 1 if the variable on previous civil status was missing

• Mother from Quito (equal to 0 if missing value) + NO extra dummy

• Parents from same city (equal to 0 if missing value) + NO extra dummy

• Number of children in 2005 (0…10) (the variable becomes 0 if the number of children is missing) + one dummy equal to 1 if the number of children is missing

Household economics controls

• Dummy mother was working before (equal to 0 if missing value) + dummy equal to 1 if the variable is missing

• Dummy father was working before (equal to 0 if missing value) + dummy equal to 1 if the variable is missing

• Mother's mean firm size (0, 1, 3.5, 8, 15.5, 35.5, 75.5, 300.5) (equal to 0 if missing value) + dummy equal to 1 if the variable is missing

• Father's mean firm size (0, 1, 3.5, 8, 15.5, 35.5, 75.5, 300.5) (equal to 0 if missing value) + dummy equal to 1 if the variable is missing

• Family average income before (0, 50, 200, 350, 450, 600, 800) (equal to 0 if missing value) + dummy equal to 1 if the variable is missing

Child controls

• Birth order (1…10) (the variable becomes 1 if birth order is missing) + one dummy equal to 1 if birth order is missing

• Child age (5…14)

• Number of young siblings in 2005 (0…3) (the variable becomes 0 if the number of young siblings is missing) + one dummy equal to 1 if the number of young siblings is missing

• School fixed effects

Figures and Tables

Figure 2.1. Identification Strategy

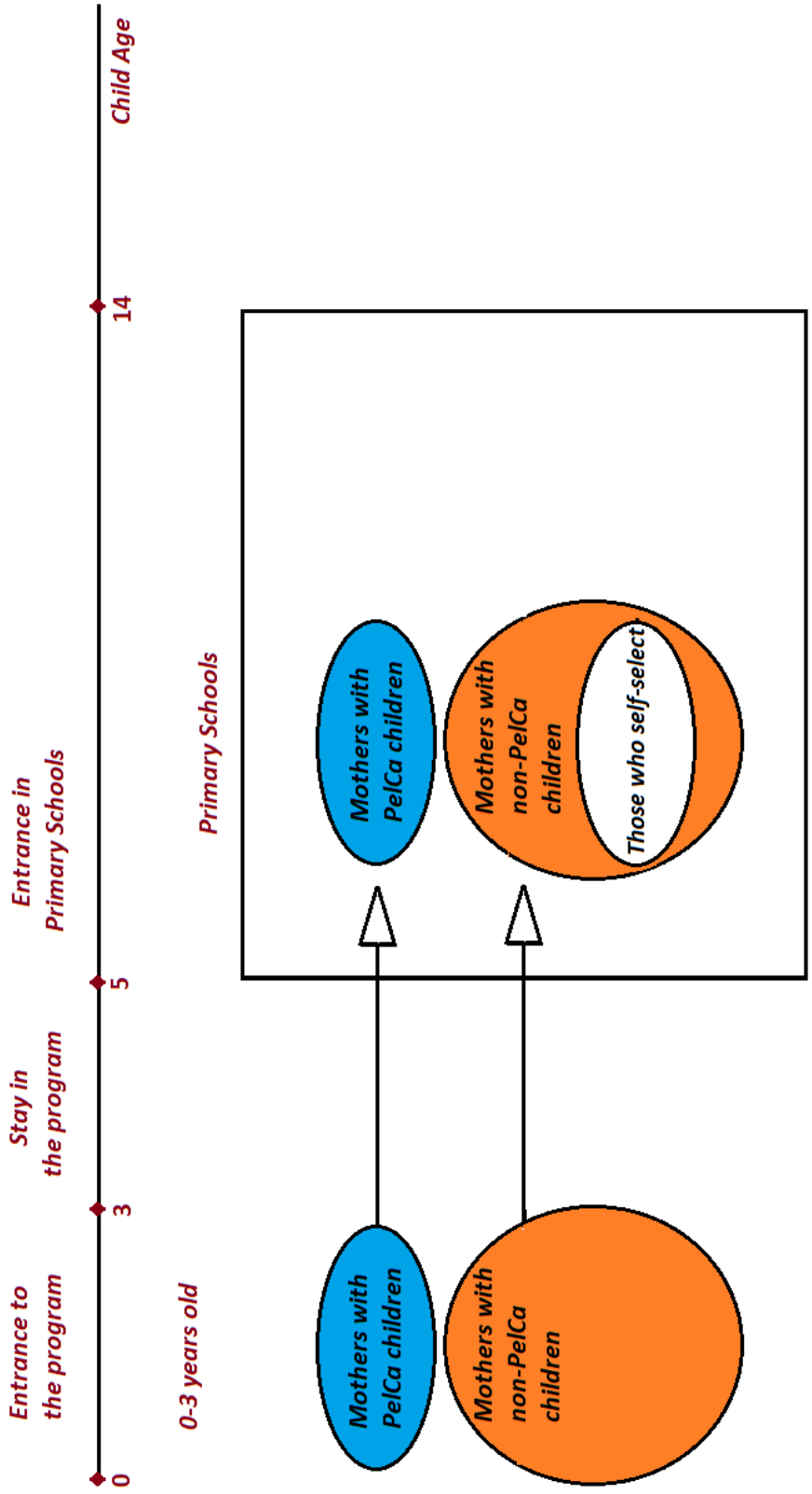


Figure 2.2. Why control mothers did not enrol before in the program

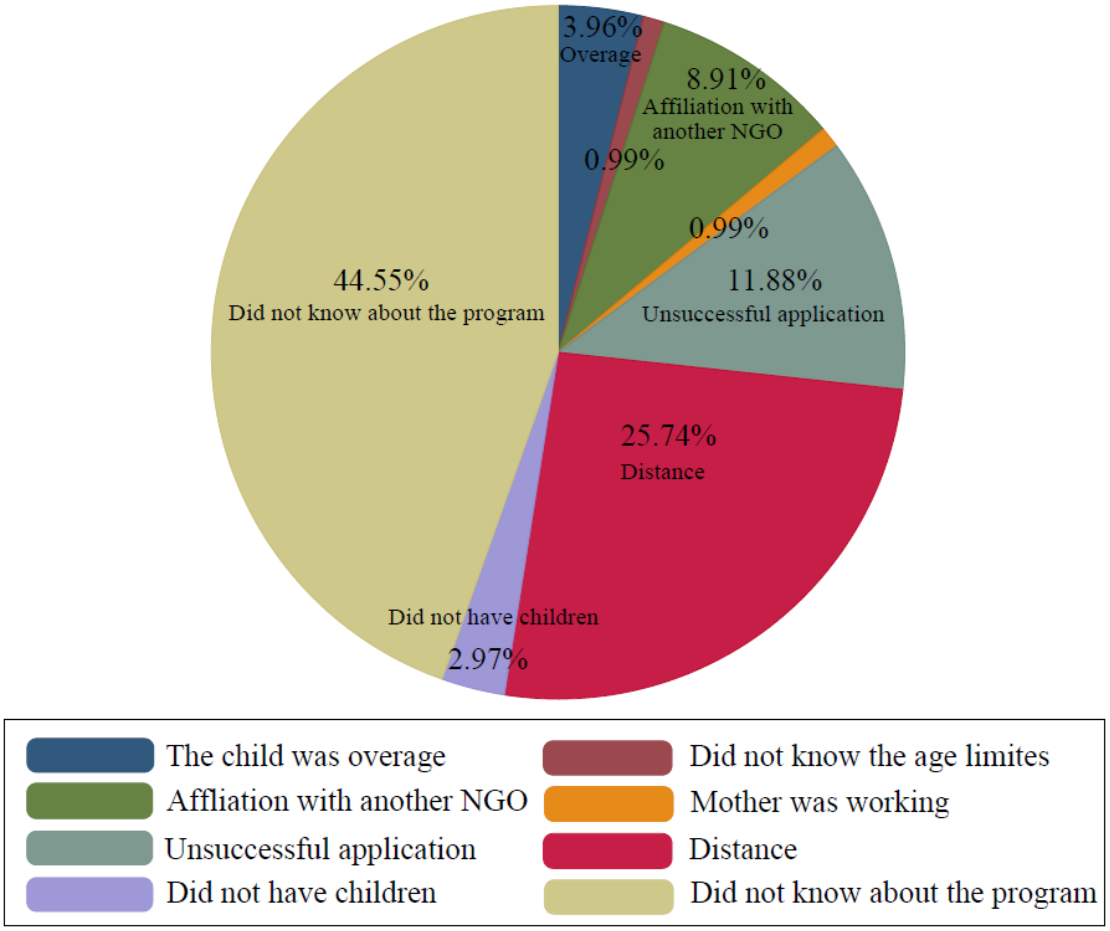


Table 2.1. Mothers' Characteristics and Pre-program Outcomes (2012)

	Treatment	Control	Difference in	Std. error
	(1)	(2)	(3)	(4)
A: Mothers' characteristics before Treatment				
Mother age	31.988	31.183	0.805	(0.729)
Mother from Quito	0.560	0.496	0.065	(0.061)
Parents from same city	0.510	0.456	0.053	(0.064)
Mother lived together with partner	0.801	0.817	-0.016	(0.048)
Mother divorced/separated/widow	0.018	0.009	0.009	(0.014)
Mother divorced/separated/widow	0.018	0.009	0.009	(0.014)
No. of children in 2005	1.849	1.632	0.218	(0.180)
Did not complete primary	0.114	0.148	-0.033	(0.041)
Completed primary	0.392	0.374	0.018	(0.059)
Did not complete secondary	0.295	0.304	-0.009	(0.056)
Completed secondary	0.169	0.165	0.003	(0.045)
Started university	0.024	0.009	0.015	(0.016)
Not religious	0.096	0.070	0.027	(0.034)
Christian	0.831	0.861	-0.030	(0.044)
B: Mothers' pre-program outcomes				
Money of her own	0.582	0.526	0.056	(0.061)
Worked	0.470	0.609	-0.139**	(0.060)
Worked full time	0.551	0.729	-0.177**	(0.078)
Self-employed	0.808	0.786	0.022	(0.067)
Worked in the formal sector	0.256	0.300	-0.044	(0.074)
Mean firm size	10.182	12.739	-2.557	(5.855)
Not working because of children	0.333	0.252	0.081	(0.056)
Not working because there was no job	0.073	0.078	-0.006	(0.032)
Not working because partner did not want	0.073	0.052	0.021	(0.030)
Not working for other reasons	0.048	0.009	0.040*	(0.021)
Mother/both decided on child's education	0.899	0.875	0.024	(0.066)
Mother/both decided when ill	0.963	0.968	-0.005	(0.040)
Mother/both decided on discipline	0.875	0.903	-0.028	(0.069)
Mother/both decided spending	0.764	0.693	0.071	(0.054)
Mother/both decided food spending	0.758	0.789	-0.032	(0.051)
Mother/both decided mother working	0.800	0.770	0.030	(0.050)
Mother/both decided having children	0.878	0.858	0.020	(0.041)
Mother/both decided contraceptives	0.896	0.856	0.040	(0.040)
F(23, 36) =1.2918				
Observations	166	115	281	

Notes: Statistics are based on the 2012 survey of mothers. Standard errors are presented in parentheses in the Column (4); * p < 0.1, ** p < 0.05, *** p < 0.01. An F-test on the overall significance of the pre-treatment variables is shown at the end of the table.

Table 2.2. Children's Characteristics before Treatment (2012)

	Treatment mean	Control mean	Difference in means	Std. error
	(1)	(2)	(3)	(4)
Female	0.521	0.470	0.051	(0.052)
Child age	8.344	8.835	-0.491**	(0.198)
Mean birth order	2.023	1.878	0.145	(0.136)
1 younger sibling in 2005	0.201	0.201	-0.000	(0.041)
2 younger sibling in 2005	0.027	0.049	-0.021	(0.019)
3 younger sibling in 2005	0.000	0.006	-0.006	(0.005)
Height at birth (cm)	48.258	48.790	-0.531	(0.462)
Weight at birth (gram)	3046.155	3029.197	16.957	(74.818)
Head circumference at birth (cm)	33.723	33.572	0.151	(0.283)
Dummy grade 1/2	0.320	0.305	0.015	(0.048)
Dummy grade 3/4	0.365	0.348	0.018	(0.050)
Dummy grade 5/6	0.242	0.268	-0.026	(0.045)
Dummy grade 7	0.073	0.079	-0.006	(0.027)
F(11, 103) = 1.3029 Prob > F = 0.2334				
Observations	219	164	383	

Notes: Statistics are based on the 2012 survey of children. Standard errors are presented in parentheses in the Column (4); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. An F-test on the overall significance of the pre-treatment variables is shown at the end of the table.

Table 2.3. Estimated Effects on Labour Market Outcomes and Mothers' Economic and Social Independence (2012)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Labour market outcomes						
Summary index	0.494***	0.467***	0.482***	0.581***	0.577***	0.577***
	(0.100)	(0.099)	(0.098)	(0.114)	(0.101)	(0.096)
Mother works	0.206***	0.172***	0.176***	0.213***	0.216***	0.216***
	(0.058)	(0.061)	(0.062)	(0.081)	(0.069)	(0.066)
Mother working fulltime	0.222***	0.205***	0.207***	0.233***	0.238***	0.234***
	(0.054)	(0.057)	(0.059)	(0.061)	(0.052)	(0.051)
Mother working with contract	0.192***	0.202***	0.204***	0.221***	0.223***	0.220***
	(0.047)	(0.048)	(0.049)	(0.044)	(0.041)	(0.041)
Average family monthly income	44.848**	44.538**	44.479**	44.507*	43.193*	43.242**
	(20.526)	(21.189)	(20.462)	(24.849)	(22.267)	(20.884)
B: Mothers' economic and social independence						
Summary index	0.295***	0.336***	0.366***	0.249***	0.275***	0.302***
	(0.074)	(0.075)	(0.076)	(0.089)	(0.081)	(0.070)
Mother has her own money	0.213***	0.233***	0.222***	0.202**	0.204***	0.205***
	(0.059)	(0.062)	(0.064)	(0.082)	(0.067)	(0.065)
Mother participates in voluntary activities	0.077	0.105	0.142*	0.069	0.087	0.105
	(0.066)	(0.071)	(0.072)	(0.092)	(0.076)	(0.074)
Mother currently studying	0.081**	0.111***	0.123***	0.087**	0.086**	0.095***
	(0.036)	(0.039)	(0.040)	(0.037)	(0.036)	(0.034)
Mother/both decide on mother working	0.122***	0.115***	0.132***	0.112*	0.108**	0.116**
	(0.039)	(0.042)	(0.044)	(0.061)	(0.054)	(0.052)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	281	281	281	281	281	281

Notes: Each cell reports the estimated treatment effect from a separate regression based on the 2012 survey. Estimated summary indices of corresponding outcomes are reported in shading rows. Columns (1)-(3) present results using the original sample without entropy balancing. Columns (4)-(6) stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.4. Estimated Effects on Labour Market Outcomes and Mothers' Economic and Social Independence (Pooled 2012 and 2013)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Labour market outcomes						
Summary index	0.554***	0.502***	0.503***	0.660***	0.664***	0.665***
	(0.099)	(0.103)	(0.098)	(0.116)	(0.105)	(0.102)
Mother works	0.221***	0.167***	0.164***	0.204**	0.204***	0.204***
	(0.056)	(0.061)	(0.060)	(0.081)	(0.068)	(0.067)
Mother working fulltime	0.217***	0.192***	0.194***	0.240***	0.241***	0.241***
	(0.049)	(0.053)	(0.052)	(0.055)	(0.050)	(0.050)
Mother working with contract	0.190***	0.193***	0.193***	0.218***	0.218***	0.218***
	(0.038)	(0.038)	(0.037)	(0. 036)	(0.035)	(0.034)
Mother weekly wage†	18.168***	14.848***	13.331***	18.504***	17.886***	17.081***
	(4.460)	(4.852)	(4.874)	(5.713)	(4.999)	(4.966)
B: Mothers' economic and social independence						
Summary indices	0.231***	0.259***	0.276***	0.263***	0.285***	0.288***
	(0.054)	(0.055)	(0.056)	(0.072)	(0.061)	(0.062)
Mother has her own money	0.194***	0.187***	0.178***	0.146**	0.146***	0.146***
	(0.050)	(0.052)	(0.051)	(0.068)	(0.056)	(0.053)
Mother participates in voluntary activities	0.034	0.051	0.067	0.017	0.019	0.019
	(0.050)	(0.053)	(0.054)	(0.073)	(0.059)	(0.055)
Mother currently studying	0.063**	0.077**	0.085***	0.087***	0.088***	0.088***
	(0.028)	(0.031)	(0.031)	(0.028)	(0.028)	(0.027)
Mother/both decide on mother working	0.085***	0.093***	0.104***	0.120**	0.123***	0.125***
	(0.029)	(0.030)	(0.031)	(0.048)	(0.045)	(0.041)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	496	496	496	496	496	496

Notes: Each cell reports the estimated treatment effect from a separate regression based on 2012 and 2013 surveys. Estimated summary indices of corresponding outcomes are reported in shading rows. Columns (1)-(3) present results using the original sample without entropy balancing. Columns (4)-(6) stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses and are clustered at the maternal level. † Estimated results for mothers weekly wage is based on the data of 2013.

Table 2.5. Estimated Effects on Mothers' Intra-household Decision-making (2012)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
Summary index	0.119**	0.118**	0.126**	0.126*	0.125**	0.124**
	(0.055)	(0.056)	(0.057)	(0.072)	(0.061)	(0.055)
Mother/both decide on child's education	0.099***	0.099***	0.102***	0.143**	0.145**	0.143**
	(0.034)	(0.038)	(0.039)	(0.062)	(0.059)	(0.056)
Mother/both decide when ill	0.016	0.003	0.006	–0.019	–0.018	–0.018
	(0.029)	(0.032)	(0.032)	(0.025)	(0.025)	(0.026)
Mother/both decide on discipline	0.087**	0.081*	0.087**	0.117*	0.116*	0.115*
	(0.039)	(0.041)	(0.042)	(0.061)	(0.060)	(0.059)
Mother/both decide on spending	0.050	0.052	0.069	0.106	0.106	0.103
	(0.050)	(0.054)	(0.055)	(0.077)	(0.067)	(0.063)
Mother/both decide on food spending	0.017	-0.001	0.017	0.030	0.031	0.029
	(0.049)	(0.051)	(0.053)	(0.078)	(0.067)	(0.062)
Mother/both decide on having children	0.007	0.007	0.005	–0.004	–0.005	–0.006
	(0.024)	(0.026)	(0.027)	(0.024)	(0.023)	(0.023)
Mother/both decide on contraceptives	–0.026	–0.008	–0.019	–0.027	–0.030	–0.030
	(0.031)	(0.033)	(0.034)	(0.040)	(0.030)	(0.030)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	281	281	281	281	281	281

Notes: Each cell reports the estimated treatment effect from a separate regression based on the 2012 survey. Estimated summary indices of corresponding outcomes are reported in shading rows. Column (1)-(3) present results using the original sample without entropy balancing. Columns (4)-(6) stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.6. Estimated Effects on Mothers' Intra-household Decision-making (Pooled 2012 and 2013)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
Summary index	0.106**	0.101**	0.093*	0.076	0.068	0.070
	(0.053)	(0.051)	(0.051)	(0.068)	(0.056)	(0.052)
Mother/both decide on child's education	0.076***	0.063***	0.066***	0.095**	0.095***	0.095***
	(0.025)	(0.026)	(0.026)	(0.035)	(0.031)	(0.030)
Mother/both decide when ill	0.025	0.018	0.021	0.007	0.007	0.007
	(0.032)	(0.034)	(0.033)	(0.032)	(0.031)	(0.031)
Mother/both decide on discipline	0.083**	0.088**	0.086**	0.119**	0.119**	0.119**
	(0.033)	(0.036)	(0.037)	(0.056)	(0.054)	(0.053)
Mother/both decide on spending	0.087	0.083	0.090	0.104	0.103	0.104*
	(0.055)	(0.057)	(0.057)	(0.078)	(0.064)	(0.060)
Mother/both decide on food spending	0.033	0.011	0.031	0.031	0.031	0.031
	(0.052)	(0.052)	(0.051)	(0.076)	(0.065)	(0.060)
Mother/both decide on important items	0.062	0.058	0.048	0.016	0.016	0.015
	(0.045)	(0.051)	(0.052)	(0.052)	(0.047)	(0.046)
Mother/both decide on having children	0.001	0.001	–0.003	–0.018	–0.018	–0.019
	(0.026)	(0.026)	(0.027)	(0.022)	(0.021)	(0.021)
Mother/both decide on contraceptives	–0.039	–0.018	–0.024	–0.024	–0.026	–0.025
	(0.031)	(0.033)	(0.036)	(0.043)	(0.033)	(0.032)
Mother/both decide on mother's health	–0.011	–0.006	–0.021	–0.053*	–0.052*	–0.054*
	(0.037)	(0.040)	(0.040)	(0.031)	(0.031)	(0.030)
Mother/both decide if mothers can visit	0.020	0.022	0.013	0.040	0.043	0.043
	(0.045)	(0.049)	(0.049)	(0.062)	(0.058)	(0.058)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	496	496	496	496	496	496

Notes: Each cell reports the estimated treatment effect from a separate regression based on 2012 and 2013 surveys. Estimated summary indices of corresponding outcomes are reported in shading rows. Columns (1)-(3) present results using the original sample without entropy balancing. Columns (4)-(6) stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses and are clustered at the maternal level; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.7. Estimated Effects on Mothers' Outcomes by Treatment Intensity and by Treatment Heterogeneity (2012)

	Treatment intensity		Mothers' education		Mothers' pre-treatment role in decision-making		Mothers' pre-treatment employment status	
	2-6 years	6-7 years	Up to primary	More than primary	Below average	Above average	Not worked before	Worked before
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Labour market outcomes	0.450***	0.545***	0.462***	0.611***	0.729***	0.394***	0.794***	0.340***
summary index	(0.123)	(0.120)	(0.130)	(0.163)	(0.145)	(0.131)	(0.191)	(0.126)
	[0.830]		[0.514]		[2.650]*		[4.120]**	
Mothers' economic and	0.288***	0.397***	0.420***	0.386***	0.482***	0.415***	0.351***	0.484***
social independence	(0.091)	(0.088)	(0.105)	(0.113)	(0.118)	(0.108)	(0.120)	(0.095)
summary index	[0.616]		[0.049]		[0.164]		[0.776]	
Household decisions-	0.076	0.210***	0.098	0.205**	0.197***	–0.017	0.213**	0.086
making summary index	(0.067)	(0.067)	(0.076)	(0.087)	(0.073)	(0.083)	(0.083)	(0.079)
	[1.700]*		[0.861]		[2.987]*		[1.191]	
Observations	281		136	144	93	179	133	148

Notes: Each cell reports the estimated aggregate effect on a summary index from a separate regression based on the 2012 survey. Covariates of child characteristics, household demographics and household economics are included in regressions. In columns (1) and (2), F-test statistic for coefficients difference in treatment intensities are presented in squared brackets. In Columns (3) – (8), F-test (Chow-test) statistic for subgroup difference in treatment effects are presented in square brackets. Estimated results are based on the original sample without entropy balancing. Standard errors are presented in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.8. Estimated Effects on Children's Outcomes: Summary Indices (2012)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
Tests scores and report card grades	0.158	0.181	0.163	0.026	0.155	0.179
	(0.122)	(0.117)	(0.122)	(0.193)	(0.109)	(0.112)
Schooling dropout and grade repetition	–0.192**	–0.170**	–0.182**	–0.112	–0.174*	–0.181*
	(0.082)	(0.073)	(0.073)	(0.089)	(0.096)	(0.098)
Attitude towards schooling	0.080	0.027	0.026	0.106	0.072	0.054
	(0.063)	(0.072)	(0.071)	(0.069)	(0.072)	(0.072)
Overall summary index†	0.194***	0.213***	0.206***	0.084	0.174**	0.193***
	(0.073)	(0.077)	(0.080)	(0.104)	(0.075)	(0.072)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	383	383	383	383	383	383

Notes: Each cell reports the estimated effect on a summary index from a separate regression based on the 2012 survey. Columns (1)-(3) present results using the original sample without entropy balancing. Columns (4)-(6) stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses and are clustered at the maternal level; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

† Signs of outcomes of schooling dropout and grade repetition are reversed when calculating the overall summary index.

Table 2.9. Estimated Effects on Children's Outcomes by Gender, by Parental Education and by Mothers' Employment Status (2012)

	Gender		Mothers' education		Mothers' pre-treatment employment status	
	Male	Female	Up to primary schooling	More than primary	Not worked before	Worked before
	(1)	(2)	(3)	(4)	(7)	(8)
Tests scores and report card grades summary index	0.195	0.313***	0.390***	–0.323	0.458**	0.014
	(0.095)	(0.092)	(0.130)	(0.235)	(0.191)	(0.151)
	[0.307]		[7.117]***		[3.457]**	
Schooling dropout and grade repetition summary index	–0.087	–0.256***	–0.073	-0.133*	-0.076	-0.225***
	(0.089)	(0.063)	(0.101)	(0.079)	(0.104)	(0.089)
	[1.332]		[0.219]		[1.177]	
Attitude towards schooling summary index	0.142	0.008	0.003	–0.041	-0.051	0.098
	(0.082)	(0.082)	(0.111)	(0.114)	(0.114)	(0.094)
	[0.975]		[0.021]		[1.742]	
Observations	192	191	207	176	185	198

Notes: Each cell reports the estimated aggregate effect on the summary index from a separate regression based on the 2012 survey. F-test (Chow-test) statistic for subgroup difference in treatment effects is presented in square brackets. Estimated results are based on the original sample without entropy balancing. Covariates of child characteristics, household demographics and household economics are included in regressions. Standard errors are presented in parentheses and are clustered at the maternal level; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.10. Correlations between Working Condition before Treatment and Reasons Why Control Mothers Did Not Apply

	Worked before	Working now	Worked fulltime before	Working fulltime now
Worked before	1			
Working now	0.2108*	1		
Did not know about the program	-0.0440	-0.0952	0.1217	-0.0616
Did not have children	-0.1049	-0.0497	0.0803	0.0838
Distance	0.0833	-0.0292	-0.1444	-0.0694
Problems with the application	-0.0306	0.1407	-0.1118	0.0921
Mother was working	0.0777	0.1051	0.0803	0.2305*
Affiliation with another NGO	0.0277	-0.0193	0.0855	-0.0405
Did not know the age limits	0.0777	-0.0952	-0.2008	-0.0434
The child was over age	-0.0519	0.0101	0.1145	0.0509

Notes: Each cell reports the correlation coefficient based on the 2012 survey; * $p < 0.05$.

SUPPLEMENTAL ONLINE APPENDIX

Appendix Table A2. 1. Fathers' Characteristics before Treatment (2012)

	Treatment mean	Control mean	Difference in means	Std. error
	(1)	(2)	(3)	(4)
Father age	35.053	33.645	1.408	(0.979)
Father from Quito	0.506	0.513	-0.007	(0.061)
Primary school	0.446	0.505	-0.059	(0.063)
Secondary school	0.516	0.466	0.050	(0.064)
University	0.038	0.029	0.009	(0.023)
Not religious	0.118	0.094	0.023	(0.039)
Christian	0.843	0.858	-0.015	(0.045)
Worked before	0.873	0.870	0.004	(0.041)
Worked full time	0.938	0.880	0.058	(0.036)
Self-employed	0.828	0.838	-0.011	(0.049)
Worked in the formal sector	0.375	0.460	-0.085	(0.064)
Mean firm size	26.693	42.388	-15.695	(10.792)
F(10, 198) = 1.3881				
Prob > F = 0.1879				
Observations	166	115	281	

Notes: Statistics are based on the 2012 survey of parents. Standard errors are presented in parentheses in the Column (4); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. An F-test on the overall significance of the pre-treatment variables is shown at the end of the table.

Appendix Table A2. 2. Household Characteristics before Treatment (2012)

	Treatment mean	Control mean	Difference in means	Std. error
	(1)	(2)	(3)	(4)
Family lived in Pisulli	0.675	0.583	0.092	(0.058)
House was owned	0.285	0.122	0.163***	(0.049)
House had drinkable	0.770	0.878	-0.109**	(0.047)
House had electricity	0.970	0.991	-0.022	(0.018)
House had toilet inside	0.430	0.383	0.048	(0.060)
Average number of rooms	3.667	3.209	0.458**	(0.218)
Family who had no vehicles	0.946	0.913	0.033	(0.031)
Family who had bicycles	0.024	0.052	-0.028	(0.022)
Family who had other means of transport	0.030	0.035	-0.005	(0.021)
Family average monthly wage (USD)	248.788	247.807	0.981	(17.250)
F(9, 286) = 3.0513				
Prob > F = 0.0017				
Observations	166	115	281	

Notes: Statistics are based on the 2012 survey of children. Standard errors are presented in parentheses in the Column (4); * p < 0.1, ** p < 0.05, *** p < 0.01. An F-test on the overall significance of the pre-treatment variables is shown at the end of the table.

Appendix Table A2. 3. Balancing Tests after Reweighting with Entropy Balancing (2012)

	Treatment	Control	Difference	Std. error
	(1)	(2)	(3)	(4)
A: Child characteristics				
Child age	8.633	8.633	0.000	(0.273)
Child birth order	1.819	1.819	0.000	(0.274)
No. of children mother had in 2005	1.849	1.849	0.000	(0.326)
No. of young siblings in 2005	0.307	0.307	0.000	(0.094)
B: Mother characteristics				
Mother age	31.988	31.989	-0.001	(1.035)
Mother worked before	0.470	0.470	0.000	(0.082)
Mother worked full time	0.259	0.259	0.000	(0.066)
Mother's mean firm size	10.120	10.123	-0.002	(6.924)
Single before	0.181	0.181	0.000	(0.063)
Mother from Quito	0.560	0.560	0.000	(0.082)
Did not complete primary (mother)	0.114	0.121	-0.006	(0.053)
Completed primary (mother)	0.392	0.392	0.000	(0.083)
Did not complete secondary (mother)	0.295	0.295	0.000	(0.072)
Completed secondary (mother)	0.169	0.169	0.000	(0.060)
Started university (mother)	0.024	0.024	0.000	(0.027)
C: Father characteristics				
Father age	33.873	33.874	-0.001	(1.396)
Father worked before	0.873	0.874	0.000	(0.057)
Father's mean firm size	17.373	17.394	-0.020	(8.161)
Did not complete primary (father)	0.072	0.072	0.000	(0.037)
Completed primary (father)	0.349	0.349	0.000	(0.077)
Did not complete secondary (father)	0.355	0.355	0.000	(0.086)
Completed secondary (father)	0.133	0.133	0.000	(0.051)
Started university (father)	0.036	0.036	0.000	(0.027)
D: Household characteristics				
Parents were married	0.458	0.458	0.000	(0.084)
Parents cohabited	0.343	0.343	0.000	(0.076)
Parents from the same city	0.470	0.470	0.000	(0.083)
Family monthly wage	247.289	247.302	-0.013	(23.870)
Observations	166	115	281	

Notes: Statistics are based on the 2012 survey of children. Standard errors are presented in parentheses in the Column (4); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A2. 4. Estimated Effects on Fathers' Labour Market Outcomes (2012)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
Summary index	–0.110	–0.126	–0.113	–0.084	–0.080	–0.097
	(0.103)	(0.104)	(0.105)	(0.128)	(0.108)	(0.105)
Father working	–0.017	–0.017	–0.021	–0.023	–0.023	–0.024
	(0.025)	(0.028)	(0.029)	(0.025)	(0.024)	(0.024)
Father working fulltime	–0.030	–0.034	–0.029	–0.006	–0.006	–0.012
	(0.043)	(0.045)	(0.046)	(0.065)	(0.048)	(0.046)
Father working with contract	–0.070	–0.085	–0.063	–0.045	–0.040	–0.051
	(0.065)	(0.070)	(0.071)	(0.089)	(0.084)	(0.082)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	281	281	281	281	281	281

Notes: Each cell reports the estimated treatment effect from a separate regression based on the 2012 survey. Estimated summary indices of corresponding outcomes are reported in shading rows. Columns (1)-(3) present results using the original sample without entropy balancing. Columns (4)-(6) stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A2. 5. Estimated Effects on Mothers' Care of Children (2012)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
Summary index	0.025	0.041	0.045	0.018	0.018	0.010
	(0.063)	(0.063)	(0.065)	(0.072)	(0.065)	(0.064)
Mother's time inputs for child	0.035**	0.035**	0.039**	0.036	0.036*	0.037*
	(0.016)	(0.016)	(0.017)	(0.023)	(0.019)	(0.019)
Weekly help from mother (minutes)	–2.137	23.479	15.565	–3.564	0.230	–3.698
	(51.028)	(53.791)	(56.288)	(58.471)	(51.812)	(51.201)
Mother's aspirations for child's education	0.013	0.018	0.016	–0.012	–0.012	–0.012
	(0.023)	(0.024)	(0.025)	(0.018)	(0.018)	(0.019)
Mother's expectations for child's education	0.047	0.047	0.076	0.081	0.082	0.081
	(0.049)	(0.052)	(0.054)	(0.074)	(0.066)	(0.065)
Child feels mother helps	–0.173	–0.177	–0.200	–0.111	–0.109	–0.114
	(0.121)	(0.130)	(0.135)	(0.165)	(0.138)	(0.136)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	281	281	281	281	281	281

Notes: Each cell reports the estimated treatment effect from a separate regression based on the 2012 survey. Estimated summary indices of corresponding outcomes are reported in shading rows. Columns (1)-(3) present results using the original sample without entropy balancing. Columns (4)-(6) stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A2. 6. Estimated Effects on Self-esteem, Big Five Personality Traits and Fertility Choices (2012)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
Summary index	0.038	0.034	0.040	0.027	0.022	0.024
	(0.055)	(0.057)	(0.057)	(0.083)	(0.070)	(0.063)
Rosenberg scale	0.034	0.019	0.017	–0.006	–0.004	–0.001
	(0.056)	(0.058)	(0.060)	(0.077)	(0.060)	(0.059)
Agreeableness	–0.015	–0.023	–0.017	0.015	0.017	0.017
	(0.067)	(0.070)	(0.072)	(0.101)	(0.088)	(0.082)
Conscientiousness	0.010	–0.014	0.002	0.019	0.018	0.019
	(0.078)	(0.083)	(0.086)	(0.109)	(0.095)	(0.091)
Extraversion	–0.080	–0.062	–0.028	–0.048	–0.053	–0.053
	(0.066)	(0.071)	(0.073)	(0.097)	(0.087)	(0.085)
Neuroticism	0.080	0.092	0.088	0.078	0.082	0.083
	(0.070)	(0.075)	(0.077)	(0.093)	(0.084)	(0.082)
Openness to experience	0.063	0.082	0.091	0.128	0.129	0.130
	(0.075)	(0.081)	(0.083)	(0.117)	(0.103)	(0.100)
Pregnant	–0.017	–0.016	–0.018	–0.043	–0.042	–0.041
	(0.019)	(0.021)	(0.022)	(0.044)	(0.038)	(0.038)
More children (including pregnant women)?	0.068	0.066	0.063	0.011	0.012	0.010
	(0.048)	(0.051)	(0.053)	(0.074)	(0.064)	(0.062)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	281	281	281	281	281	281

Notes: Each cell reports the estimated treatment effect from a separate regression based on the 2012 survey. Estimated summary indices of corresponding outcomes are reported in shading rows. Columns (1)-(3) present results using the original sample without entropy balancing. Columns (4)-(6) stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A2. 7. Estimated Effects on Fathers' Labour Market Outcomes (Pooled data of 2012 and 2013)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
Summary index	0.011	–0.026	–0.006	–0.075	–0.062	–0.065
	(0.083)	(0.081)	(0.078)	(0.107)	(0.089)	(0.084)
Father working	0.006	–0.008	–0.006	–0.020	–0.015	–0.016
	(0.032)	(0.031)	(0.030)	(0.035)	(0.028)	(0.028)
Father working fulltime	0.032	0.018	0.023	–0.008	–0.002	–0.003
	(0.043)	(0.044)	(0.043)	(0.049)	(0.043)	(0.041)
Father working with contract	–0.023	–0.045	–0.023	–0.057	–0.053	–0.054
	(0.053)	(0.060)	(0.058)	(0.080)	(0.068)	(0.065)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	496	496	496	496	496	496

Notes: Each cell reports the estimated treatment effect from a separate regression based on the 2012 survey. Estimated summary indices of corresponding outcomes are reported in shading rows. Columns (1)-(3) present results using the original sample without entropy balancing. Columns (4)-(6) stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses and are clustered at the maternal level; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A2. 8. Estimated Effects on Mothers' and Children's Outcomes by Treatment Intensity: Summary Indices (2012)

	Not weighted		Weighted	
	Longer treated vs. shorter treated	Shorter treated vs. non-treated	Longer treated vs. shorter treated	Shorter treated vs. non-treated
	(1)	(2)	(1)	(2)
A: Mothers' outcomes: summary indices				
Labour market outcomes	0.031	0.453***	0.028	0.641***
	(0.117)	(0.126)	(0.153)	(0.196)
Mothers' economic and social independence	0.108	0.305***	0.125	0.281**
	(0.091)	(0.096)	(0.113)	(0.126)
Household decisions-making	0.023	0.108	0.000	0.166*
	(0.079)	(0.073)	(0.084)	(0.092)
B: Children's outcomes: summary indices				
Tests scores and report card grades	0.012	0.155	-0.230	0.405**
	(0.154)	(0.169)	(0.151)	(0.180)
Schooling dropout and grade repetition	0.026	–0.194***	0.027	–0.242***
	(0.109)	(0.074)	(0.096)	(0.097)
Attitude towards schooling	0.008	0.020	-0.057	0.052
	(0.087)	(0.026)	(0.105)	(0.113)
Overall summary index†	-0.049	0.240**	-0.178*	0.316***
	(0.096)	(0.105)	(0.097)	(0.107)

Notes: Each cell reports the estimated effect on a summary index from a separate regression based on the 2012 survey. Covariates of child characteristics, household demographics and household economics are included in regressions. Longer treatment group and shorter treatment group are divided by whether the respondent has been in the NGO for more than 5 years and 8 months (the median). Column (1) presents the estimated effect of being longer treated with respect to being treated shorter. Column (2) displays the effects of shorter treatment exposure compared to the control group. Estimated results are based on the original sample without entropy balancing. Standard errors are presented in parentheses and are clustered at the maternal level; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

† Signs of outcomes of schooling dropout and grade repetition are reversed when calculating the overall summary index.

3

Minimum Wages and Informal Employment in Developing Countries

ABSTRACT

We present new empirical evidence on the implications of minimum wages on informal employment in developing countries, analysing a unique dataset assembled from a set of micro surveys collected in 61 low and middle income countries. Our identification strategy exploits relative variation in minimum wages across labour market groups within countries. Our estimates show that a higher minimum wage is associated with a larger self-employment share. The effect is approximately linear in the relative level of the minimum wage, even if higher levels of minimum wages are associated with higher levels of non-compliance. The estimated impact of the minimum wage on informality is economically significant: one standard deviation raise in the minimum wage is associated with an 11 percent increase in the self-employment rate.

JEL Codes: J38, O17, J21.

3.1 Introduction

Minimum wages are perhaps the most popular labour market policy tool in developing countries. The motivations for raising minimum wages are various, reducing inequality and fighting poverty being commonly held rationales. However, the potential for the minimum wage in achieving these goals may be hampered by informality. If workers in the formal sector are pushed into informal jobs as a consequence of the minimum wage hike, the impacts of the minimum wage on inequality and/or poverty may be nil, or even negative. As of today, there is no consensus in the empirical literature regarding the impact of the minimum wage on informality in the developing world.

This lack of consensus possibly rests on two complementary explanations. The largest fraction of the literature has focused on analysing the effects of minimum wage legislation in advanced economies (Neumark and Wascher, 1992; Card, 1992; Dickens, Machin, and Manning, 1999; Dube, Lester, and Reich, 2010; Draca, Machin, and Reenen, 2011; Addison and Ozturk, 2012; Addison, Blackburn, and Cotti, 2013; Neumark, Salas, and Wascher, 2014).

The literature studying the impacts of the minimum wage in developing countries is instead sparser, and has mostly focused in Latin American economies. In addition, even among middle income countries existing evidence is inconclusive. Large negative effects of the minimum wage on formal employment are found in Honduras (Gindling and Terrell, 2009), but in Costa Rica (Gindling and Terrell, 2007), Colombia (Maloney, Nunez, Cunningham, Fiess, Montenegro, Murrugarra, Santamaria, and Sepulveda, 2001) and Vietnam (Nguyen Viet, 2010) the effects are small, and not statistically significant in Mexico (Bell, 1997), Brazil (Lemos, 2009) and Thailand (Del Carpio, Messina, and Sanz-de Galdeano, 2014).

Naturally, the minimum wage may have heterogeneous effects across countries, possibly depending on interactions with other labour market policies and structural features of the labour market. An important source of heterogeneity that is often ignored in the literature is how binding the minimum wage is. For example, in Mexico the minimum wage is relatively low, at 29 percent of the 70th percentile of the distribution of wages in the formal sector. In Colombia instead, the minimum wage is much close to the 70th wage percentile (64 percent),

and above the median for certain labour market groups (e.g. young and low educated workers). If the impact of the minimum wage is non-linear, e.g., kicks-in only after a certain threshold, some of these apparent contradictory results may be reconciled. This paper presents new evidence on the impact of the minimum wage on informality by assembling a unique dataset of micro surveys from 61 middle income developing countries observed in the period 1995-2012. Informality is measured as the share of self-employed and family workers, i.e. those workers who are by definition not covered by minimum wage legislation, in total employment outside of agriculture. However, we present robustness checks for alternative definitions of informal workers.

Assessing the impact of the minimum wage across countries, or within countries over time, is extremely difficult because other labour market policies and macro shocks may be changing in sync with changes in the minimum wage. Instead, our empirical strategy consists in contrasting the relative effectiveness of the minimum wage across labour market groups (e.g. young female with basic education and young male with basic education) within countries and years. This interaction between the wage of labour market groups and a country/year minimum wage is informative about the relative bite of the minimum wage policy. The identifying assumption is that variations in minimum wage levels do not affect the shape of the underlying wage distribution for each labour market group. Our strategy follows Lee (1999), who assesses the impact of the federal minimum wage on US wage inequality using the variation in the relative bite of the minimum wage across US states.

We assemble a pooled individual-level dataset for 61 countries and calculate the effectiveness of the minimum wage for a set of twelve labour market groups defined on the basis of individual age, gender and level of education in each country and year. The effectiveness of the minimum wage is calculated as the ratio of the country/year minimum wage to the 70th percentile of formal sector wages in each group. We call this variable the minimum wage ratio. Such a high percentile in the wage distribution is unlikely to be affected by minimum wages, but we provide robustness checks for different cut-off points. This rich data set allows

us to analyse not only the average effects of the minimum wage on informality, but also whether those impacts are heterogeneous across the same labour market groups we use for identification.

The paper also analyses possible non-linearities in the impact of the minimum wage on informality. Informality does not need to be an exclusion state. It may instead be a worker choice, either because the worker does not value sufficiently the amenities of a formal job (e.g. a right to a pension or health insurance) or because he/she values highly some particular attribute of the informal job (e.g. greater flexibility in working hours). Empirical evidence against labour market segmentation has been found in several Latin American countries (see Maloney, 1999, and Bosch and Maloney, 2010). In this context, if minimum wages are sufficiently low (i.e., close to the market wage) they may have no effects on formality, or effects may be even positive if they provide a sufficient incentive for workers to accept a formal job. If the minimum wage is instead relatively high with respect to the underlying worker productivity, the disincentive effect on job creation is likely to more than compensate for the increased worker willingness to take a formal job[47]. Hence, different levels of the minimum wage with respect to market wages may have different consequences on informality.

Our estimates show that a higher minimum wage is associated with a larger share of self-employment. Our estimated effects are not modest: our baseline model indicates that a one standard deviation increase in the minimum wage ratio raises informality by 11 percent. Interestingly, the estimated effect appears fairly linear, with higher minimum wages having a larger negative effect on formal employment. In line with these results, the negative effects on formality concentrate among low-paid workers: young, less educated and female employees are more likely to be fund in informal sectors when the minimum wage increases. We acknowledge and document the substantial heterogeneity across countries regarding the coverage of the minimum wage laws, which often vary across sectors (e.g. excluding agriculture), occupations (e.g. high vs. low skilled), workers' age (e.g. young and apprentices vs. prime-aged) and

[47] See Brown, Merkl and Snower (2014) for a similar discussion in the context of the impact of the minimum wage on employment.

geographical coverage (e.g. nation-wide vs. regional or provincial). However, a battery of robustness checks suggests that such heterogeneity has a small impact on the estimated results.

The paper is organized as follows. Section 3.2 introduces the identification strategy, section 3 provides a description of the data, section 4 presents the empirical findings, section 5 includes robustness checks, and section 6 concludes.

3.2 Research Design

Our analysis investigates the implication of different levels of minimum wage stringency on a measure of informality, i.e. the share of self-employment plus family workers in total employment outside of agriculture. We identify the impact of the minimum wage by exploiting the different levels of effectiveness of the minimum wage across different labour market groups. This identification strategy allows us to control for country/year unobservable factors and other potential determinants of informality that are country/year specific.

Our baseline estimates are obtained using a unique individual-level dataset collected across 61 developing countries. Our baseline specifications rely on individual data in which the dependent variable is a binary indicator for employed workers who are self-employed or family workers. However, our variable of interest only varies across country/year/labour market group dimensions. We cluster the standard errors at the group level to avoid over-stating the precision of the estimates (Cameron and Miller 2015). We alternatively work with a country/year/labour market group aggregated dataset and report weighted average regressions that mimic the individual data.

We define labour market groups or cohorts by country/year on the basis of workers' age, gender, and education. For each country/year/group observation we calculate a measure of the effectiveness of the minimum wage. Following Lee (1999), we define the strictness of the minimum wage, MW_{ct}, as the ratio of the prevailing minimum wage in a country/year and some measure of centrality (or location) of wages for each labour market group. Lee (1999) uses the median wage as an indicator of location. The underlying assumption is that the median is

unaffected by the minimum wage, and hence provides a valid benchmark against which one can assess how stringent the minimum wage is. Minimum wages in developing countries close to or even above median wages are not rare, in particular for young, female and less educated workers. To limit possible spillover effects of the minimum wage into our measure of centrality our benchmark specification uses the 70th percentile of the group-specific distribution of wages as a measure of location. In robustness checks we show that variations in the measure of centrality do not affect the results.

The choice of the dimensions that define the groups is subject to a trade-off between having a large number of cohorts *C* with few individuals per cohort n_C (with imprecise estimates of the appropriate minimum wage by cohort and population cohort means when analysing aggregate data) and a smaller number of cohorts with more individuals and therefore more precise estimates of the population cohort means. Labour market groups should be chosen in order to have homogeneous individuals within each cohort in order to minimize the variance of the measurement error when calculating the average minimum wage's stringency across groups. In addition, cohorts should be sufficiently heterogeneous with enough between variation in order to obtain more precise estimates of the parameter of interest. Our model will adopt robust standard errors in order to account for the heteroskedasticity arising from the difference in the precision of the calculation of averages of cells of different sizes. In the baseline specifications we consider a total of 12 possible groups by interacting gender with three age groups (16-29; 30-59; 50-65) and two education levels (primary or less and more than primary).

Our individual-level model is therefore:

$$y_{it} = \alpha MW_{cjt} + \beta_c' Z_{it} + \mu_c + \mu_t + \varepsilon_{it} \text{ for } i = 1, \dots, N \quad j = 1, \dots, J \quad t = 1, \dots, T \quad (1)$$

where y_{it} is an informality dummy for individual *i* observed at time *t*, $\overline{MW}_{cjt}$ is our measure of minimum wage's strictness for each country *c*, cohort *j* and year, Z_{it} is a set of individual characteristics in terms of age, gender and educational attainment measured by country specific dummies across time, X_{it} is a set of other individual-level characteristics and μ_c and μ_t are, respectively, country and year fixed effects.

The effects of the minimum wage on informality is therefore defined by exploiting the within country, time and cohort dimensions variation in the data. Our research design therefore resembles that of an experiment where the definition of a minimum wage level at the country, region or sectoral level would impose alternative minimum wage stringency levels across cohorts on the basis of their representative 70th wage percentile.

Each cell in our setting should therefore represent a variation in the minimum wage stringency level of equal weight in order to avoid a bias induced by the difference in relative numerosity of each cell. Our baseline individual-level estimates are therefore weighted in order to give equal weight to each cell.[48]

An alternative setting would consist in estimating our model on averaged data, such as:

$$\bar{y}_{jt} = \alpha MW_{ct} + \beta_c' \bar{Z}_{jt} + \mu_c + \mu_t + \varepsilon_{jt} \text{ for } j = 1, \dots, J \quad t = 1, \dots, T \qquad (2)$$

where $\bar{y}_{jt}$ and $\bar{Z}_{jt}$ are now measured at the group level, i.e. for group *j* at time *t*.

Group level averages are likely to be characterized by sampling error in finite samples if the cell size is too small. In our setting this may induce a measurement error in our measure of minimum wage by cohort and year generated by the small size of the cohort/year cells and induce an attenuation bias in our individual-level estimates as well as in the estimates on aggregated data. Since the cell size is inversely proportional to the number of cohorts used in the analysis, and therefore a lower number of cells *JT* increases the variance of the estimator, then the problem that the researcher faces in this case is typically a trade-off between bias and variance. According to Verbeek and Nijman (1992, 1993) and Nunziata (2015) a cell size of 100 should typically eliminate the bias. In what follows we adopt a minimum cell dimension of 100 individuals and perform some robustness checks adopting cells of a different size. One element that we have to bear in mind is that dropping those cells whose dimension is smaller than the adopted minimum threshold may also introduce a selection bias in our estimates. All these aspects are discussed in the details in the empirical findings section.

[48] In order to give the same weight to each cell we assign a weight to each individual observation which is equal to 1 over the number of observations in the cell.

3.3 The Data

Our estimations are performed on a rich and unique newly assembled dataset covering 61 developing countries. We use two main sources to construct it: first, the new International Income Distribution (I2D2 henceforth) data set, a global harmonized household survey database that allows us to compare different countries around the world and across time. The vast majority of the surveys included in the I2D2 are nationally representative. The dataset is extremely rich and comprehensive in coverage, but it is also noisy, as it collects data from surveys that were not designed to be comparable. Our empirical strategy limits the impact of these flaws by restricting the identification to comparisons across groups within country/year waves, i.e., by effectively eliminating variation across countries or within countries over time.

We merge the I2D2 with the International Labour Organization Global Wage dataset, which covers minimum wage levels across the world. We use the ILO dataset to build indicators of the effectiveness of the minimum wage across 12 different labour market groups. These groups are: female/male between 18 and 29 years old who are low/highly educated, female/male between 30 and 50 years old who are low/highly educated, female/male between 51 and 65 years old who are low/highly educated.

In order to construct our final estimation sample we need to drop a number of country/year surveys. When we merge the I2D2 with the ILO database we have 425 waves for 88 countries. We first drop the waves for which the minimum wage in the ILO Global Wage Database is missing either because minimum wages do not exist, or because the information could not be found. We also limit the sample to the years 1995-2012. We are left with 75 countries and 382 waves.

Secondly, we analyse the distribution of wages and minimum wages for the remaining countries. In some cases minimum wages are at odds with the wage distribution in the country/year (e.g. they are too high or too low). This is possibly due to measurement errors in one of the two databases or problems with the units of measurement in the I2D2 that could not be solved. When we drop these problematic waves we are then left with 64 countries and 333 survey waves.

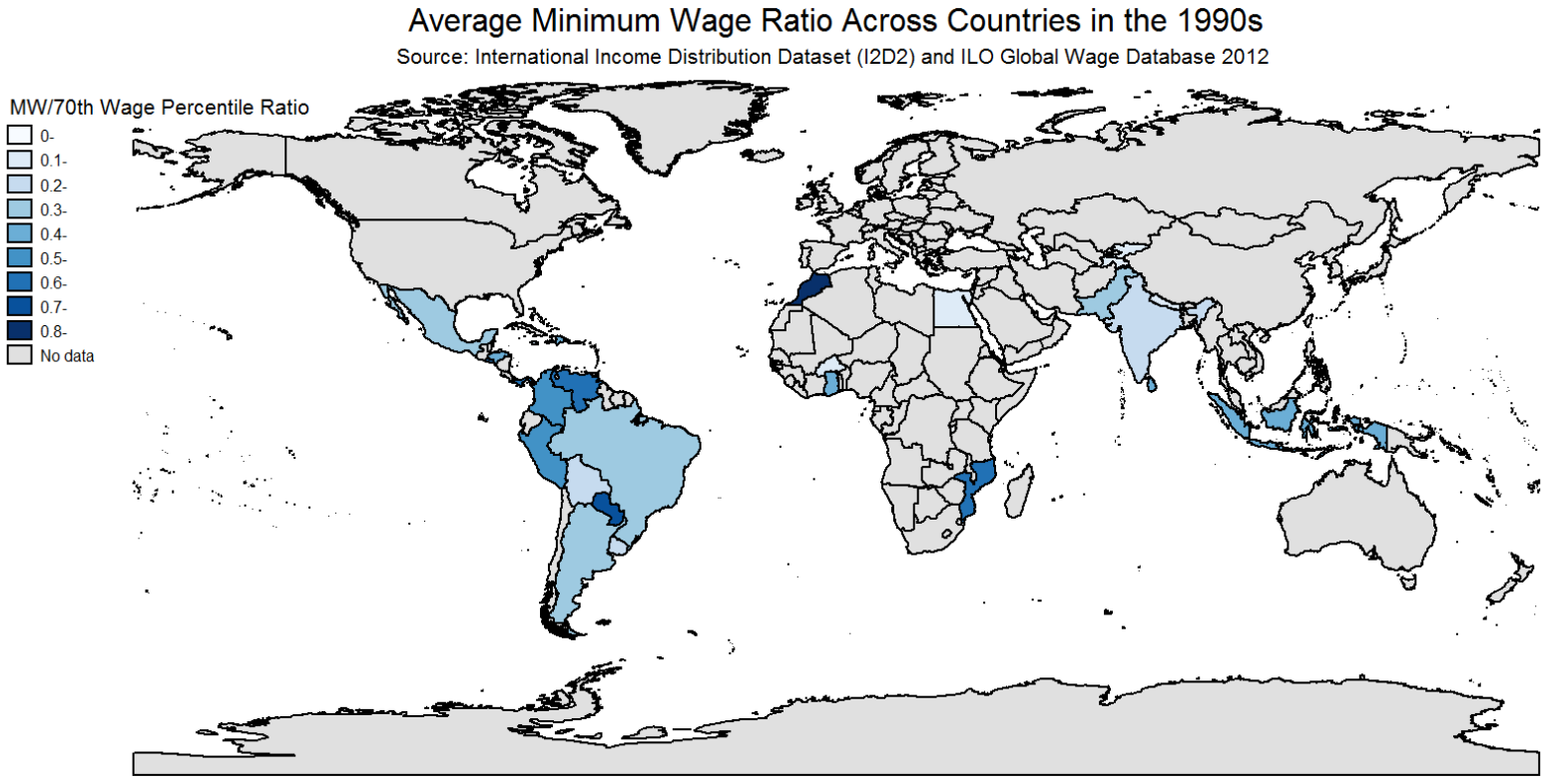
Thirdly, we drop surveys for very small countries in which the majority of the cells have less than 50 observations; we are left with 62 countries and 325 survey waves.

Fourth, we exclude two waves that show some problems in specific variables: Togo 2006 (missing education) and Tanzania 2009 (most values for industry are missing). This leaves us with 323 waves relative to 61 countries. Fifth, in order to derive meaningful measures of the effectiveness in the minimum wage we include in the empirical analysis only those cells with at least 100 observations. We are then left with 61 countries and 321 waves.

We also exclude observations for which the minimum wage ratio over the 70th percentile wage is strictly greater than one. This is a clear indicator that there is a measurement problem in the wage distribution, or the minimum wage. Finally, as we want to focus our attention on the share of self-employed outside agriculture, we exclude the individuals who work in this sector. We are left with 61 countries, 321 waves and 2,485 labour market groups, for a total of 10,095,393 individual-level observations.

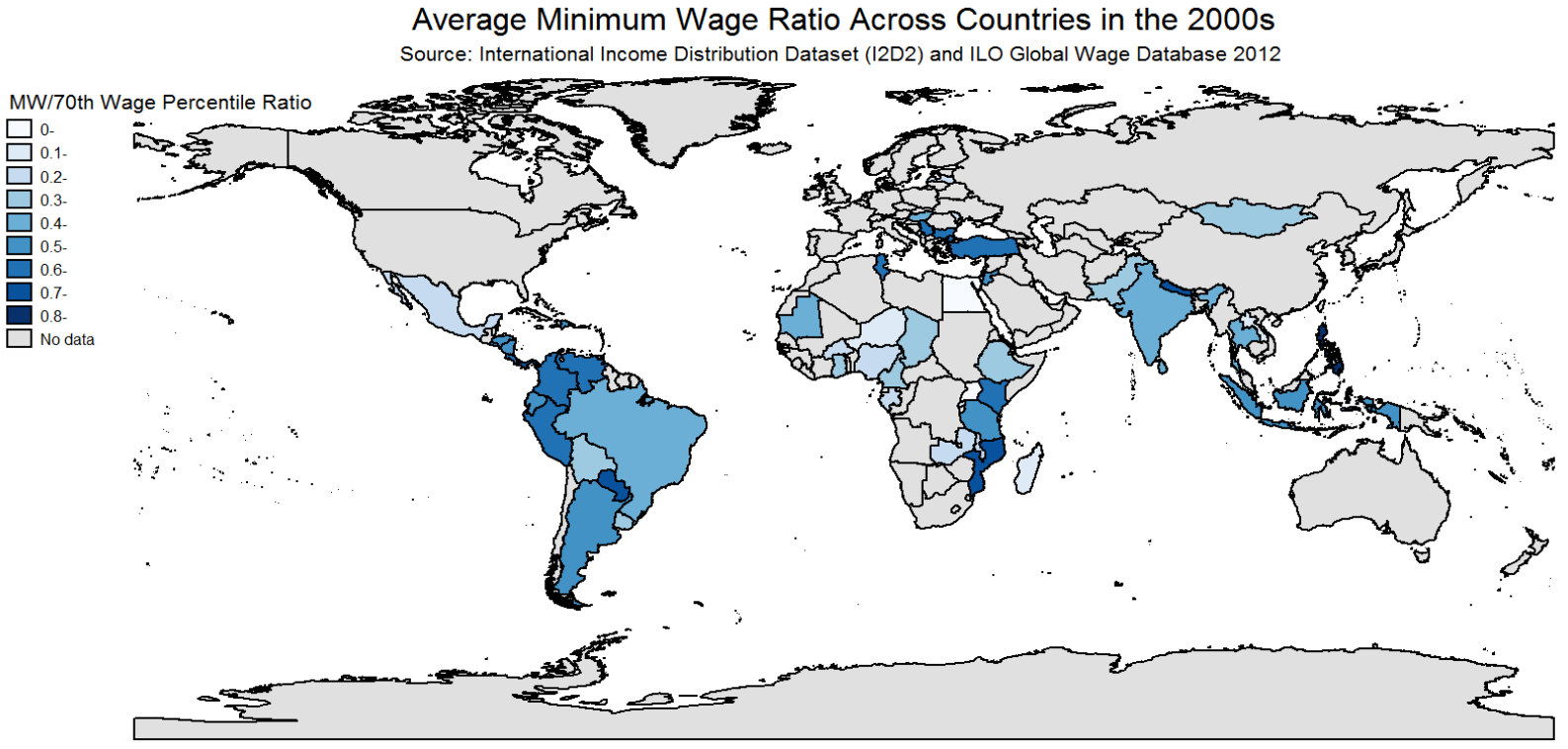
Figure 3.1 displays the country coverage in our sample and the average minimum wage ratio across countries. The latter is characterized by a significant variability across countries. Countries where the minimum wage is more binding across cohorts and time are mainly from the Central and Latin American region (Colombia, Costa Rica, El Salvador, Panama, Paraguay, Peru, and Venezuela) and Europe and Central-Asia (Bulgaria, Serbia, Turkey). The effectiveness of the minimum wage in the developing world is actually quite high. On average, across all countries considered, the ratio of the minimum wage and the 70th percentile is at 0.5. We can also infer from the data that the generosity of the minimum wage has increased in recent decades, as indicated by the comparison of Figure 3.1, displaying the ratio in the 1990s, with Figure 3.2 that displays the ratio in the 2000s.

Figure 3.1. Minimum Wage Ratio across Countries in the 1990s



Notes: The wage ratio shown in the map is a weighted average of the minimum wage/70th percentile wage in the country across cohorts (where the weights are the shares of each cohort wage employment in total wage employment). If there are more survey waves for a country, we take a simple average of the country averages across years. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labour market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile. Years 1995-1999.

Figure 3.2. Minimum Wage Ratio across Countries in the 2000s



Notes: The wage ratio shown in the map is a weighted average of the minimum wage/70th percentile wage in the country across cohorts (where the weights are the shares of each cohort wage employment in total wage employment). If there are more survey waves for a country, we take a simple average of the country averages across years. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labour market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile. Years 2000-2012.

Such high figures in the effectiveness of the minimum wage may be not as harmful on formal employment as expected in the presence of non-compliance. As expected, non-compliance with the minimum wage law in developing labour markets is high, and increasing with the level of the minimum wage. This is illustrated in Figures 3.7 and 3.8.

In some cases, more than 50 percent of the workers' wages are below the minimum wage. The pervasiveness of non-compliance with the law suggests that the impact of the minimum wage on formality may be less obvious than the high levels of effectiveness of the minimum wage may suggest, and potentially non-linear. If the minimum wage is very low, firms may not need to resort to informal employment. If instead the minimum wage is too high, firms may prefer risking fines by paying wage workers a wage below the legislated minimum. Thus, firms are likely to trade off two forms of informality in developing labour markets: wage employment below the minimum wage and self-employment.

This heterogeneity in the bite of the minimum wage does not only concern time and countries, but also groups of individuals: indeed, we find that the minimum wage has a different level of effectiveness depending on the group of workers considered. To show this we calculate the difference between the strictness of the minimum wage for particular groups and the country averages. We then pull the data across countries and years.

Figure 3.4 shows a kernel of the relative effectiveness of the minimum wage across age groups. As expected, young workers (18-29) are the most affected by the minimum wage legislation: the minimum wage for them is more binding than the average. The least affected by the minimum wage are instead the prime age workers (30-50). In Figure 3.5 we also find that the minimum wage is generally more binding for less educated workers. Differences across gender are less pronounced, in part because female wages are more compressed than males, but there is some indication that the minimum wage is more binding for women than men (Fig. 3.6). Our identification strategy exploits this heterogeneity in the minimum wage's strictness across labour market groups.

3.4 Empirical Findings

3.4.1 Baseline Specification

Table 3.1 presents our baseline empirical findings. The results are presented by column augmenting the model with additional controls, starting with country fixed effects only in column (1), adding year fixed effects in column (2), their interaction in column (3), *Z* dummies in column (4), country-specific *Z* dummies in column (5), and individual-level controls including an indicator of urban/rural, industry classification (mining, manufacturing, public utilities, construction, retail and wholesale trade, transport and communications, financial and business services, public administration or other unspecified services), whether the individual is the head of the household/spouse/other relative and the household size.

Our results are consistent across columns, with some differences in the point estimates. A higher minimum wage is typically associated with a larger share of informality. According to the model including the largest set of controls in column (6), a 1 percentage point increase in the ratio of the minimum wage over the 70th percentile of wages outside agriculture in a specific cohort, increases the self-employment share of that specific cohort by 0.16 percentage points. Given that the standard deviation of the minimum wage ratio in the estimation sample is 0.221 and that the average informality share is 0.312, a one standard deviation in the minimum wage ratio increases informality by 11.1 percent[49]. The estimated effect is significant at the 1 percent level. Similar results are obtained by estimating our model on aggregated data (3). Here the point estimates of the preferred specification in column (5) are somewhat larger at 0.26, but note that here we are not controlling for individual characteristics and sectoral composition.

Some countries are observed for more than one year in our sample, while others are only observed once. As a result, our results may be over-representing countries where we have more waves. We check whether this may affect our estimated effect in Table A3.1 in the

[49] 11.1% is given by ((0.160*0.221)/0.312)*100.

appendix (where we keep only the first available year for each country) and find that the results are basically unchanged.

Since the minimum wage legislation usually applies to full-time workers only, Table 3.2 presents results from surveys in which we can exclude part-time workers to compute the minimum wage ratio. The number of observations drops to 9.4 million but the point estimates are very close to the estimates on the full sample.

In Table 3.3 we test whether the effect of minimum wage legislation on informality is non-linear. In order to do that, we identify in every country/year the groups for which the minimum wage lies within the 1st, 2nd, 3rd, 4th, 5th, 6th and 7th deciles of the wage distribution. We then replace in the regressions the minimum wage ratio for a set of indicator variables for each of the minimum wage deciles. Our results show that the impact of the minimum wage on informality increase almost monotonically with the decile of the minimum wage. If the minimum wage lies in the 70th decile of the cohort's wage distribution, the self-employed share is 0.096 percentage point higher than if the minimum wage lies in the first decile of the wage distribution. We find no evidence of the impact of the minimum wage on informality levelling out as the minimum wage level approaches or even crosses the median. On the contrary, we find that moving the minimum wage from the 6th to the 7th decile of the distribution of wages presents the largest step jump in informal employment, with a difference of 0.019 points.

3.4.2 Subsamples

We also estimate our model on subsamples defined on the basis of age, gender and education (Tables 3.5-3.11), finding that the effect of the minimum wage is heterogeneous across individual characteristics, with the effects being larger on young workers (Table 3.7-3.8), low educated (Table 3.10) and females (Table 3.5).

In column (6) of Table 3.5 we show that in the model including the largest set of controls, a one percentage point increase in the ratio of the minimum wage over the 70th percentile of wages outside agriculture in a specific cohort of female workers increases the self-

employment share of that specific cohort by 0.124 percentage points. The estimated effect is significant at the one percent level and more than doubles the effect found for male workers (Table 3.6). In terms of age, workers who are affected the most by the rise in minimum wages belong to the 18-29 age group (Table 3.7) and to the 30-50 years age group (Table 3.8). Finally, high educated workers (with secondary or post-secondary schooling) are not affected by rises in the minimum wage (Table 3.11).

3.5 Robustness Checks

As a robustness check, we estimate our model restricting the sample to cohorts whose sample average is calculated using at least 200 observations, which limits the possibility that our measure of the minimum wage ratio is corrupted by measurement error due to the small size of the cohort cells. We face a trade-off between the more accurate measures obtained by the larger cell size and the possible bias introduced by the selection of those cells to be large enough. Our sample is now slightly smaller, since we lose around two percent of the observations. The estimations presented in Table A3.2 show that our results are mostly unaffected, indicating that we are unlikely to suffer from sampling error. Robustness checks with larger cohorts have been performed and are available upon request.

All the results presented above consider individuals who work outside the agricultural sector for those surveys were sectors can be identified. However, the information on the occupational sector in some surveys has a large number of missing values. As a further robustness check we then exclude from our sample those surveys where the percentage of missing values in the variable indicating the sector of occupation is higher than 10 percent, i.e. those surveys where we are forced to select a share of the respondents that is smaller than 90 percent. The estimates presented in Table A3.3 show that our findings are unaffected. We also check whether the non-linearity of the minimum wage effects are also present in the sub-samples considered above. Our findings in Tables A3.4-A3.10 suggest that across labour market groups effects also appear to be linear. Finally, we check whether our results are robust to different definitions of informality. In the paper we defined informality as the share of self-

employed and family workers, i.e. those workers who are by definition not covered by minimum wage legislation, in total employment outside of agriculture. In Table A3.13 informal workers are defined as the share of self-employed in total employment outside agriculture; in Table A3.14 informal workers are defined as the share of low educated[50] self-employed only in total employment outside agriculture; in Table A3.15 informality is defined as the share of low educated self-employed and unpaid family employees in total employment outside agriculture. Results vary somewhat in magnitude across definitions, but irrespective of the definition adopted, results go in the same direction.

Finally, we check whether our results hold when we also change the definition of the effectiveness of the minimum wage. In the baseline specifications we defined it as the ratio of the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector. In Table A3.16 we define it as the minimum wage over the median wage of each labour market group: results hold.

3.6 Conclusions

We presented a set of new empirical findings on the economic implications of minimum wage legislation in developing countries obtained from a unique newly assembled dataset containing 321 micro surveys from 61 developing countries during the years 1995-2012. The focus of our analysis is on the relationship between minimum wages and informality, measured as the share of self-employment and family workers. We avoid common pitfalls of cross-country comparisons by relying on the effectiveness of the minimum wage across labour market groups for identification. Our identification strategy exploits the relative bite of the minimum wage across age, gender and education groups within country years.

Our estimates show that a more generous minimum wage is typically associated with a larger share of informality. Our preferred baseline effect indicates that a one standard deviation increase in the ratio of the minimum wage over the 70th percentile of formal wages

[50] Low education indicates primary education or no schooling.

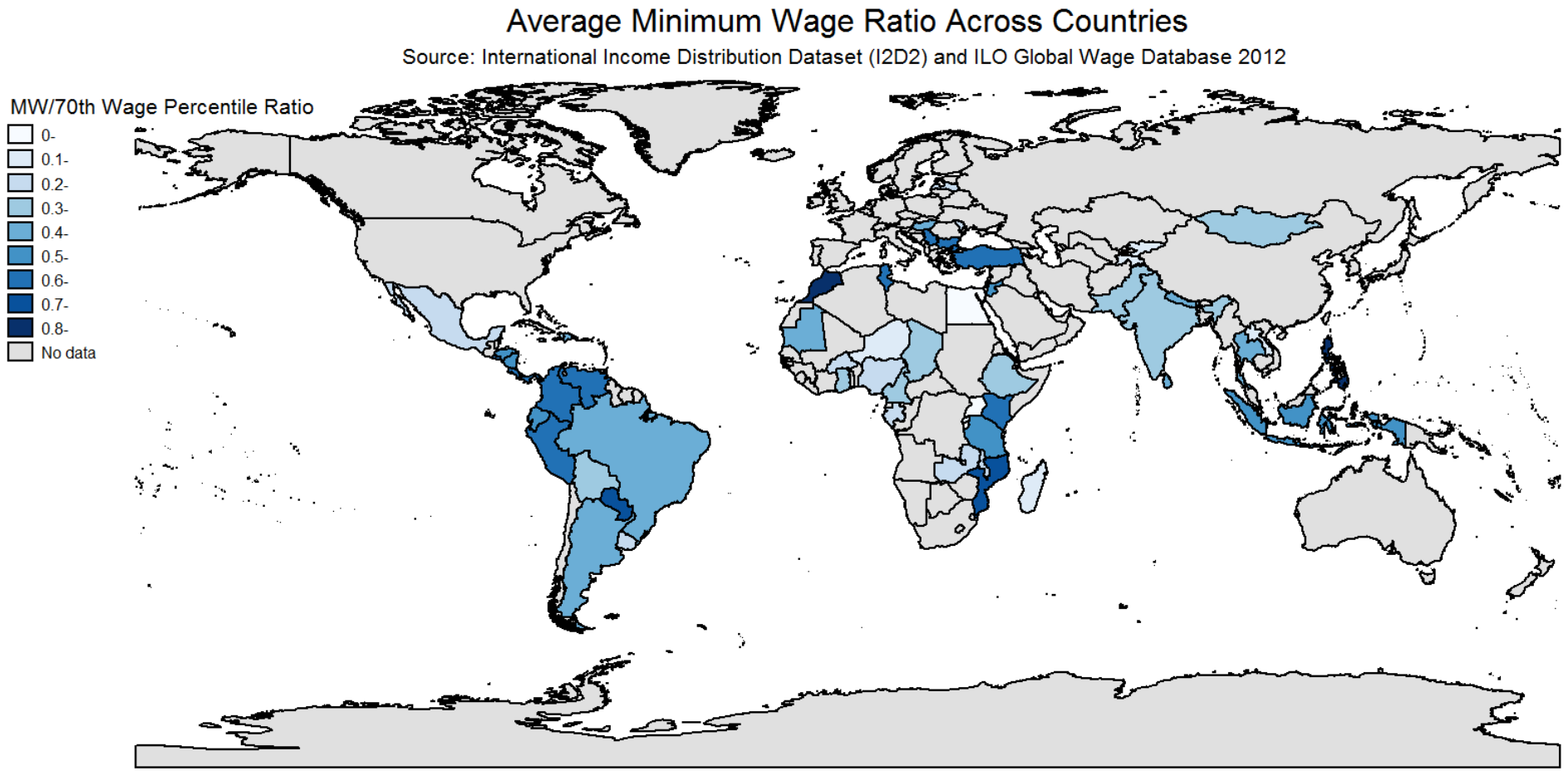
is associated with an increase in the self-employment share of that specific cohort by 11.1 percent. The effect is highly significant and very robust across a large number of alternative specifications.

The negative impact of the minimum wage on formality is larger among low-paid workers: the young, female and low educated. We find however that the effects are virtually linear in the degree of effectiveness, although the largest jumps in the negative effects on formality are found for those groups where the minimum wage is above the median formal wage.

Our results show that, on average, the minimum wage is likely to have relatively important effects on informality in the developing world, a feature that should be borne in mind when evaluating the welfare consequences of minimum wages. Governments aiming to reduce poverty using the minimum wage as a policy lever should take into account that the effects on informality are likely to be concentrated precisely across those groups that are more vulnerable to poverty, in particular the young, the women and the less educated.

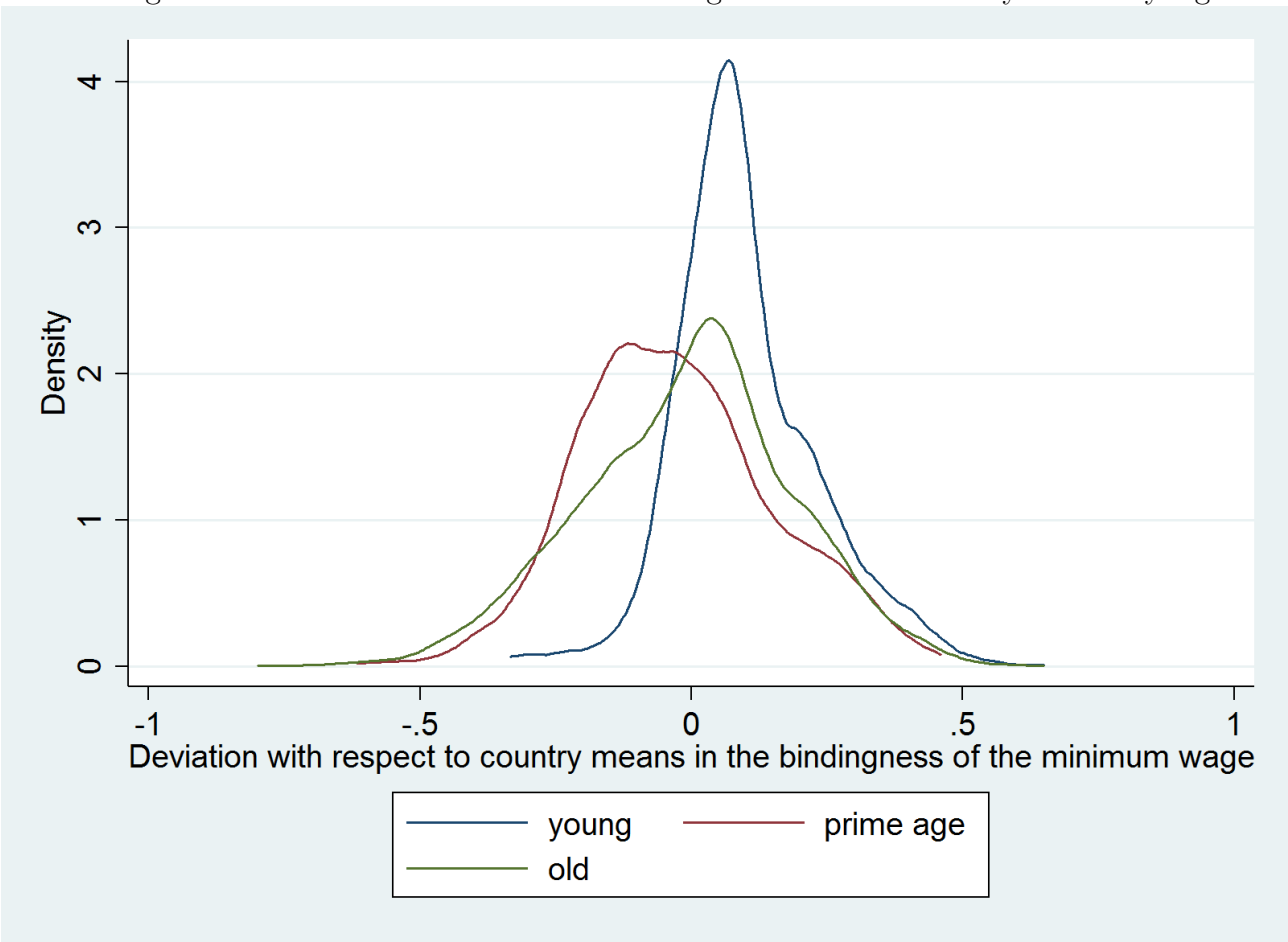
Figures and Tables

Figure 3.3. Minimum Wage Ratio Across Countries



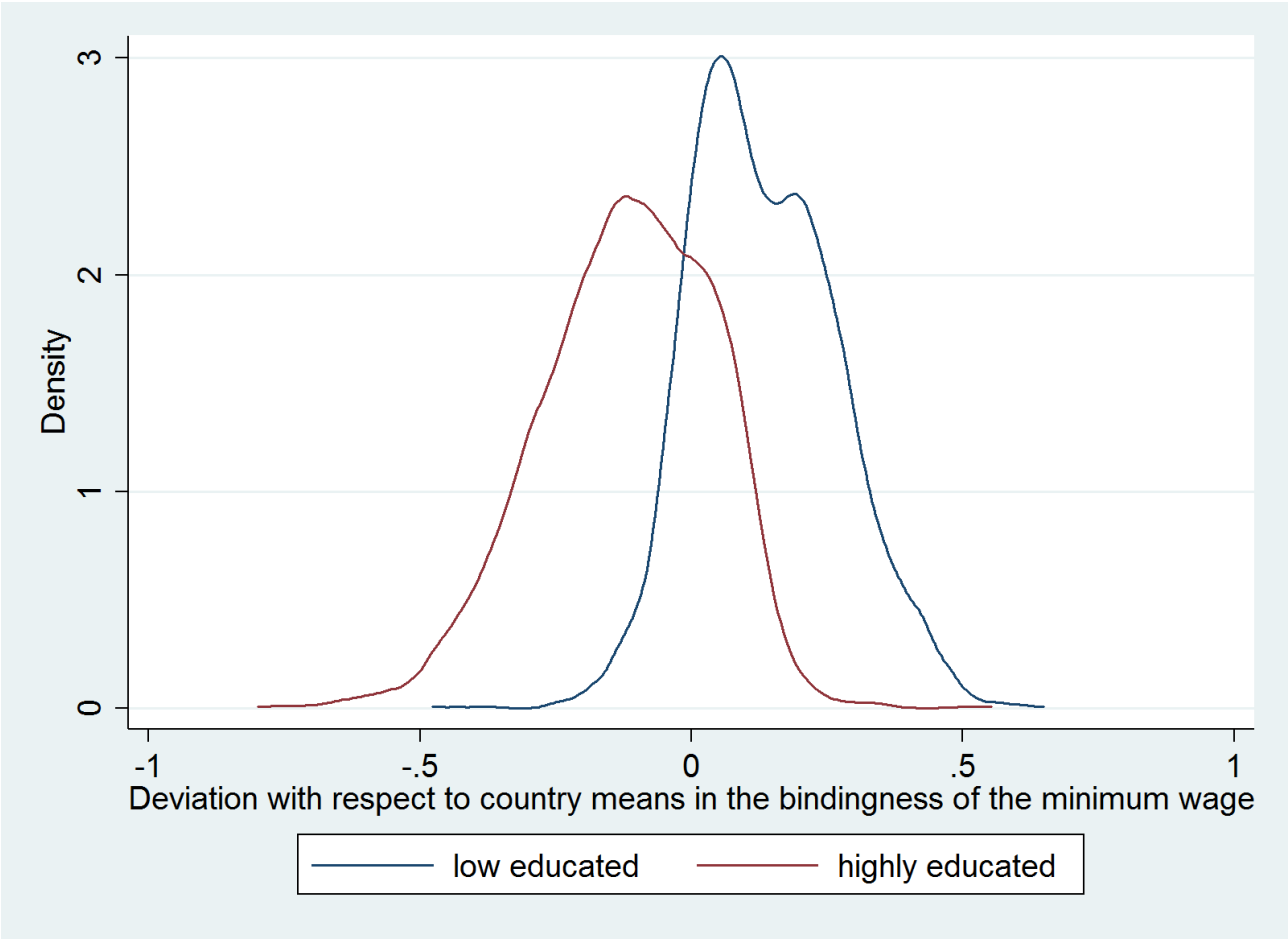
Notes: The wage ratio shown in the map is a weighted average of the minimum wage/70th percentile wage in the country across cohorts (where the weights are the shares of each cohort wage employment in total wage employment). If there are more survey waves for a country, we take a simple average of the country averages across years. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the median wage. Years 1995-2012.

Figure 3.4. Deviations in the Minimum Wage Ratio from Country Means by Age



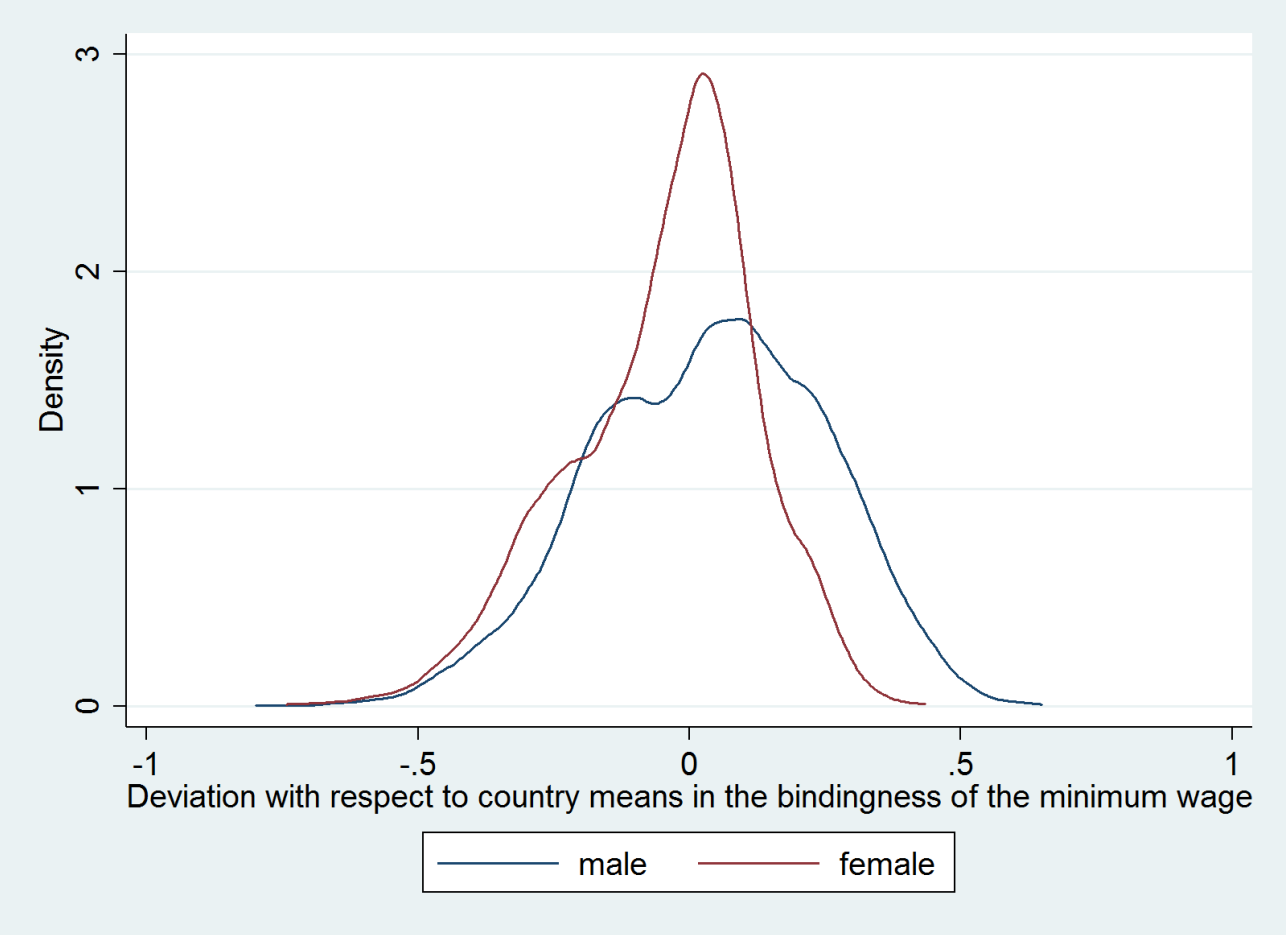
Notes: The figure shows the kernel density estimation of the deviations of cohorts' minimum wage ratios with respect to countries' average minimum wage ratios, by age. Young workers are 18-29 years old, prime age workers are 30-50 years old, old workers are 51-65 years old. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the median wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio in a cohort is defined as the minimum wage over the cohort 70th percentile wage. The deviations are calculated as the difference between the cohort minimum wage ratio and the country average minimum wage ratio across years.

Figure 3.5. Deviations in the Minimum Wage Ratio from Country Means by Education



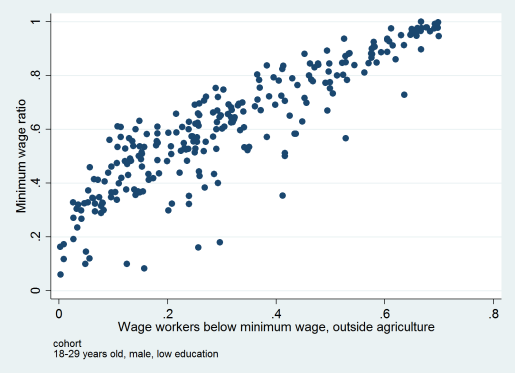
Notes: The figure shows the kernel density estimation of the deviations of cohorts' minimum wage ratios with respect to countries' average minimum wage ratios, by education level .Low educated workers have a level education up to primary (included); highly educated workers have at least secondary education. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the median wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio in a cohort is defined as the minimum wage over the cohort 70th percentile wage. The deviations are calculated as the difference between the cohort minimum wage ratio and the country average minimum wage ratio across years.

Figure 3.6. Deviations in the Minimum Wage Ratio from Country Means by Gender

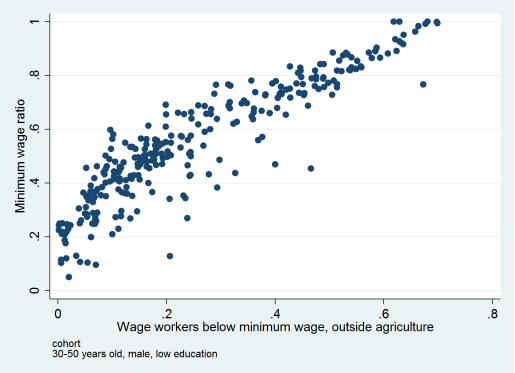


Notes: The figure shows the kernel density estimation of the deviations of cohorts' minimum wage ratios with respect to countries' average minimum wage ratios, by gender. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the median wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio in a cohort is defined as the minimum wage over the cohort 70th percentile wage. The deviations are calculated as the difference between the cohort minimum wage ratio and the country average minimum wage ratio across years.

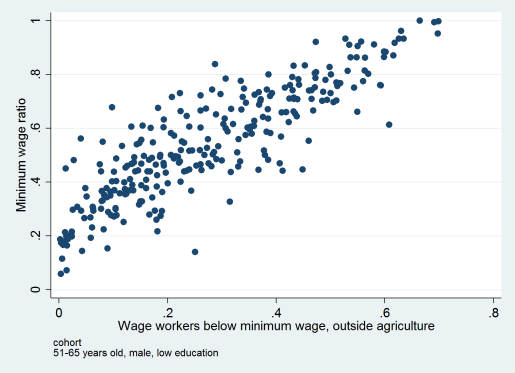
Figure 3.7: Non-Compliance and Minimum Wage Ratio in Low Educated Cohorts



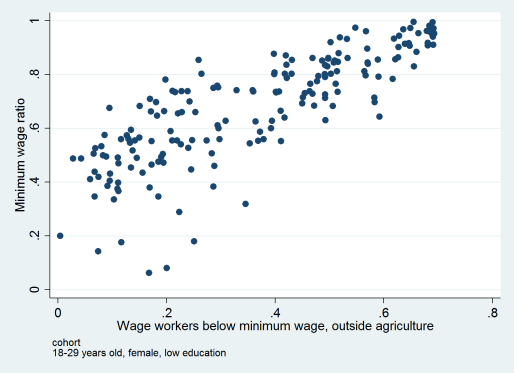
(a) Low educated 18-29 years old men.



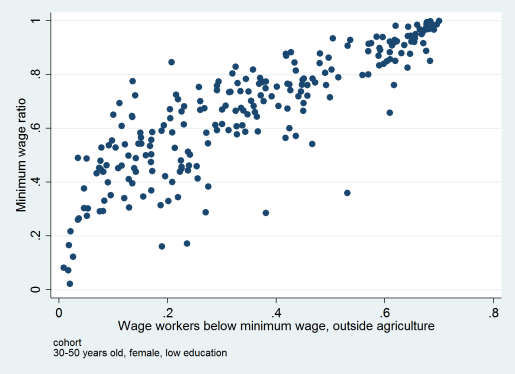
(b) Low educated 30-50 years old men.



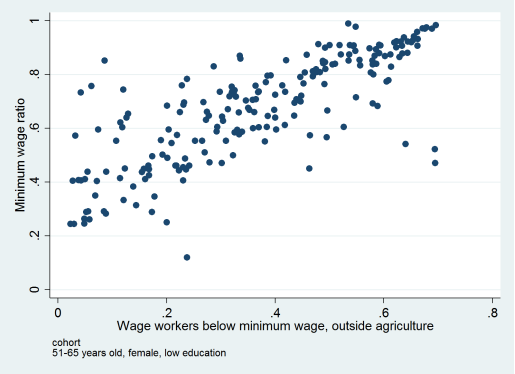
(c) Low educated 51-65 years old men.



(d) Low educated 18-29 years old women.



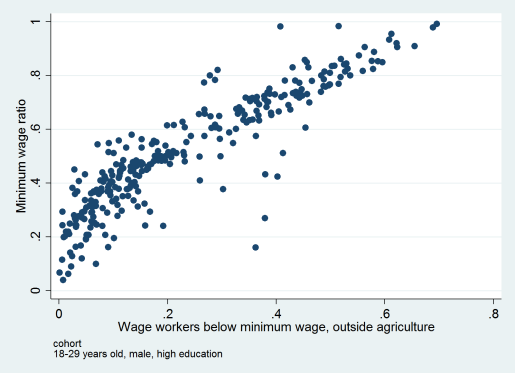
(e) Low educated 30-50 years old women.



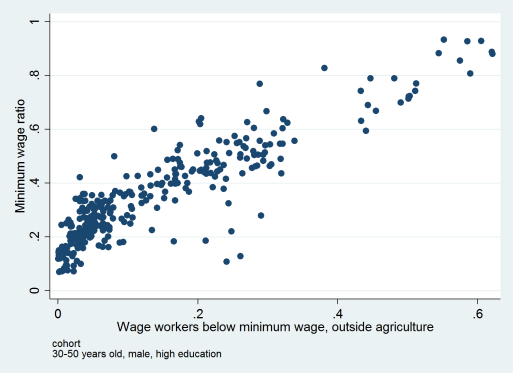
(f) Low educated 51-65 years old women.

Notes: The figure shows the positive relationship between the minimum wage ratio (y axis) and the non-compliance rate (x axis) in cohorts of individuals who are low educated (primary or no schooling). The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the median wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio in a cohort is defined as the minimum wage over the cohort 70th percentile wage. The non-compliance rate in the cohort is defined as the share of workers outside agriculture whose wages are below the minimum wage.

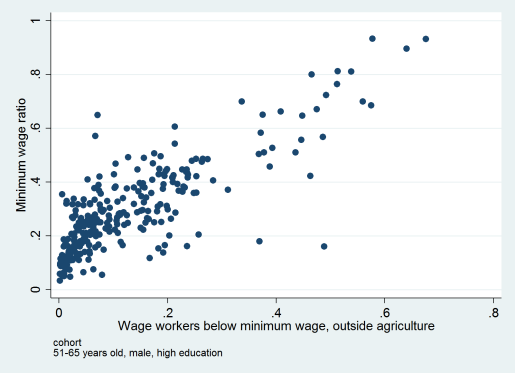
Figure 3.8. Non-Compliance and Minimum Wage Ratio in Highly Educated Cohorts



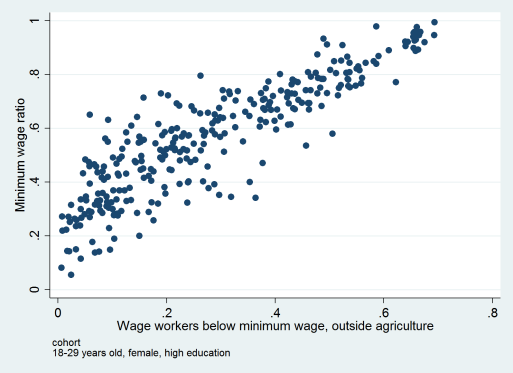
(a) Highly educated 18-29 years old men.



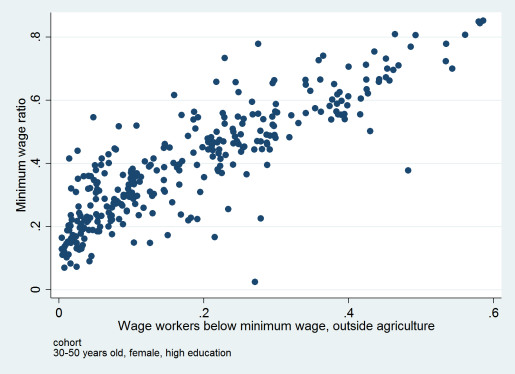
(b) Highly educated 30-50 years old men.



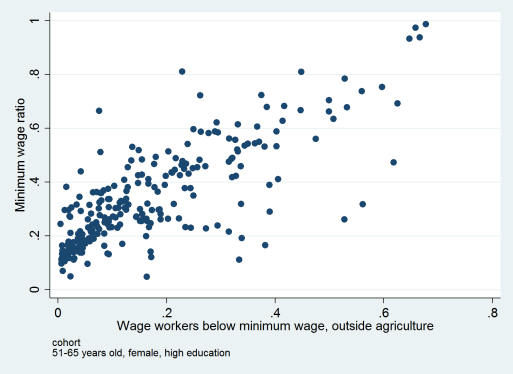
(c) Highly educated 51-65 years old men.



(d) Highly educated 18-29 years old women.



(e) Highly educated 30-50 years old women.



(f) Highly educated 51-65 years old women.

Notes: The figure shows the positive relationship between the minimum wage ratio (y axis) and the non-compliance rate (x axis) in cohorts of individuals who are high educated (secondary or post-secondary schooling). The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the median wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio in a cohort is defined as the minimum wage over the cohort 70th percentile wage. The non-compliance rate in the cohort is defined as the share of workers outside agriculture whose wages are below the minimum wage.

Table 3.1. Effects of MW Ratio on Share of Self-employed

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.227***	0.236***	0.282***	0.233***	0.252***	0.160***
	(0.020)	(0.020)	(0.019)	(0.028)	(0.020)	(0.015)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	2484	2484	2484	2484	2484	2377
No. Countries	61	61	61	61	61	59
R-sqr overall	0.075	0.076	0.085	0.110	0.131	0.245
Observations	10095393	10095393	10095393	10095393	10095393	9586249

The table reports the effect of an increase in the minimum wage ratio on the share of self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.2. Effects of MW Ratio on Share of Self-employed (Full-Time Only)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.220***	0.234***	0.268***	0.235***	0.251***	0.165***
	(0.020)	(0.021)	(0.021)	(0.030)	(0.021)	(0.017)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	2210	2210	2210	2210	2210	2103
No. Countries	53	53	53	53	53	51
R-sqr overall	0.073	0.074	0.081	0.106	0.126	0.240
Observations	9397982	9397982	9397982	9397982	9397982	8933502

The table reports the effect of an increase in the minimum wage ratio on the share of full-time self-employed only. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time only) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.3. Effects of MW Ratio on Share of Self-employed (Aggregate Data)

	(1) Share self-employed Share	(2) Share self-employed Share	(3) Share self-employed Share	(4) Share self-employed Share	(5) Share self-employed Share
MW ratio	0.134***	0.140***	0.183***	0.177***	0.257***
	(0.013)	(0.013)	(0.014)	(0.021)	(0.017)
CountryFE	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes
Degrees of freedom	2423	2406	2163	2159	1964
No. Cohorts	456	456	456	456	456
No. Countries	61	61	61	61	61
R-sqr overall	0.518	0.522	0.576	0.783	0.943
Observations	2485	2485	2485	2485	2485

The table reports the effect of an increase in the minimum wage ratio on the share of self-employed in the aggregate data. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. The data is aggregated at the labor market group (cohort) level. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labour market groups formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are robust: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.4. Effects of MW Ratio on Share of Self-employed (Non-linear Effects)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW 2nd decile wages	0.037***	0.041***	0.067***	0.050***	0.039***	0.022***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.005)	(0.004)
MW 3rd decile wages	0.062***	0.066***	0.098***	0.076***	0.062***	0.038***
	(0.010)	(0.010)	(0.009)	(0.010)	(0.006)	(0.005)
MW 4th decile wages	0.093***	0.100***	0.141***	0.110***	0.083***	0.046***
	(0.013)	(0.013)	(0.012)	(0.013)	(0.008)	(0.007)
MW 5th decile wages	0.111***	0.117***	0.156***	0.123***	0.103***	0.062***
	(0.016)	(0.016)	(0.016)	(0.018)	(0.009)	(0.008)
MW 6th decile wages	0.145***	0.152***	0.200***	0.160***	0.120***	0.077***
	(0.019)	(0.019)	(0.017)	(0.018)	(0.011)	(0.009)
MW 7th decile wages	0.202***	0.207***	0.252***	0.192***	0.142***	0.096***
	(0.018)	(0.017)	(0.016)	(0.018)	(0.012)	(0.009)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	2484	2484	2484	2484	2484	2377
No. Countries	61	61	61	61	61	59
R-sqr overall	0.077	0.077	0.087	0.111	0.130	0.245
Observations	10095393	10095393	10095393	10095393	10095393	9586249

The table reports the non-linear effects of an increase in the minimum wage ratio on the share of self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. *MW 2nd decile wages* is a dummy equal to 1 if the minimum wage is between the 10th and 20th percentile of the cohort wage distribution of (possibly full-time) wage workers outside of agriculture and the public sector. *MW 3rd decile wages* is a dummy equal to 1 if the minimum wage is between the 20th and 30th percentile of the cohort wage distribution of (possibly full-time) wage workers outside agriculture and the public sector, etc. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.5. Effects of MW Ratio on Share of Self-employed (Female)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.269***	0.280***	0.330***	0.341***	0.263***	0.124***
	(0.024)	(0.024)	(0.022)	(0.033)	(0.020)	(0.021)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	1027	1027	1027	1027	1027	978
No. Countries	55	55	55	55	55	53
R-sqr overall	0.102	0.103	0.117	0.149	0.162	0.321
Observations	3652341	3652341	3652341	3652341	3652341	3435857

The table reports the effect of an increase in the minimum wage ratio on the share of female self-employed only. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old and a dummy for individuals who are 30-50 years old. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.6. Effects of MW Ratio on Share of Self-employed (Male)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.146***	0.155***	0.196***	0.106***	0.158***	0.085***
	(0.019)	(0.018)	(0.019)	(0.027)	(0.019)	(0.017)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	1456	1456	1456	1456	1456	1398
No. Countries	61	61	61	61	61	59
R-sqr overall	0.068	0.069	0.075	0.094	0.104	0.188
Observations	6443052	6443052	6443052	6443052	6443052	6150392

The table reports the effect of an increase in the minimum wage ratio on the share of male self-employed only. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old and a dummy for individuals who are 30-50 years old. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.7. Effects of MW Ratio on Share of Self-employed (Young)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed 2	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.178***	0.208***	0.337***	0.089	0.172***	0.185***
	(0.029)	(0.032)	(0.050)	(0.060)	(0.037)	(0.040)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	880	880	880	880	880	849
No. Countries	54	54	54	54	54	52
R-sqr overall	0.098	0.099	0.111	0.114	0.127	0.192
Observations	3172997	3172997	3172997	3172997	3172997	2981582

The table reports the effect of an increase in the minimum wage ratio on the share of 18-29 years old self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling) and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.8. Effects of MW Ratio on Share of Self-employed (Prime Age)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.413***	0.430***	0.515***	0.306***	0.283***	0.179***
	(0.026)	(0.026)	(0.021)	(0.048)	(0.025)	(0.020)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	980	980	980	980	980	942
No. Countries	61	61	61	61	61	59
R-sqr overall	0.097	0.099	0.116	0.117	0.139	0.264
Observations	5560550	5560550	5560550	5560550	5560550	5305033

The table reports the effect of an increase in the minimum wage ratio on the share of 30-50 years old self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling) and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.9. Effects of MW Ratio on Share of Self-employed (Old)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.338***	0.362***	0.415***	0.222***	0.113***	0.097***
	(0.021)	(0.020)	(0.015)	(0.033)	(0.024)	(0.023)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	622	622	622	622	622	584
No. Countries	39	39	39	39	39	37
R-sqr overall	0.081	0.082	0.090	0.091	0.102	0.264
Observations	1361846	1361846	1361846	1361846	1361846	1299634

The table reports the effect of an increase in the minimum wage ratio on the share of 51-65 years old self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling) and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.10. Effects of MW Ratio on Share of Self-employed (Low Education)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.077**	0.091***	0.161***	0.179***	0.138***	0.070**
	(0.032)	(0.035)	(0.039)	(0.045)	(0.028)	(0.028)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	1023	1023	1023	1023	1023	977
No. Countries	53	53	53	53	53	51
R-sqr overall	0.091	0.091	0.099	0.118	0.136	0.236
Observations	3699524	3699524	3699524	3699524	3699524	3514383

The table reports the effect of an increase in the minimum wage ratio on the share of low educated (primary education or no schooling) self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.11. Effects of MW Ratio on Share of Self-employed (High Education)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	-0.153***	-0.174***	-0.211***	-0.129**	-0.036	-0.035
	(0.024)	(0.026)	(0.027)	(0.058)	(0.026)	(0.030)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	1460	1460	1460	1460	1460	1399
No. Countries	59	59	59	59	59	57
R-sqr overall	0.056	0.058	0.064	0.067	0.081	0.213
Observations	6395869	6395869	6395869	6395869	6395869	6071866

The table reports the effect of an increase in the minimum wage ratio on the share of high educated (secondary or post-secondary education) self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 12: Summary Statistics

Country	Informality	Minimum Wage Ratio	Male (%)	Low Educated (%)	Age	Living in Urban Area (%)	Head of the Household (%)	No. of Waves
East Asia and Pacific								
Indonesia	0.3	0.5	26.8	46.0	35.7	62.8	57.2	11
Lao PDR	0.6	0.1	22.9	25.7	34.0	59.1	48.6	2
Mongolia	0.1	0.4	48.6	0.0	37.6	76.1	47.7	3
Philippines	0.3	0.8	44.3	1.9	38.7	100.0	47.2	11
Solomon Islands	0.4	0.2	30.9	18.3	33.1	50.1	52.6	1
Thailand	0.2	0.4	49.1	31.4	35.2	100.0	40.3	4
Europe and Central Europe								
Azerbaijan	0.2	0.1	43.8	0.0	39.1	55.4	48.7	1
Bulgaria	0.1	0.6	23.5	0.0	42.5	68.7	57.6	4
Hungary	0.1	0.4	48.0	40.9	40.5	66.7	51.8	1
Kyrgyz Republic	0.0	0.1	51.1	37.9	36.4	59.1	42.6	1
Moldova	0.1	0.2	39.9	0.0	40.3	68.3	62.4	1
Serbia	0.1	0.6	39.1	7.5	40.9	.	39.7	1
Tajikistan	0.3	0.1	31.2	0.0	36.6	48.3	48.3	2
Turkey	0.2	0.7	14.4	38.2	36.4	81.4	67.2	6
High Income Non OECD								
Latvia	0.0	0.3	53.2	0.7	41.3	54.0	45.4	4
Malta	0.1	0.4	31.8	5.1	38.3	100.0	47.3	2
Uruguay	0.2	0.3	46.5	25.0	39.8	97.4	49.0	17
Latin America and The Caribbean								
Argentina	0.2	0.5	40.3	24.7	38.4	100.0	51.3	16
Bolivia	0.4	0.4	39.4	29.9	34.8	87.7	51.9	11
Brazil	0.3	0.4	44.1	49.1	36.1	93.9	50.7	16
Colombia	0.5	0.6	46.0	23.3	37.8	96.3	47.7	14
Costa Rica	0.1	0.6	10.0	42.5	31.1	48.0	53.1	9
Dominican Republ	0.2	0.5	15.9	37.5	31.5	77.4	50.9	14
Ecuador	0.4	0.6	42.6	33.0	38.2	78.6	46.3	10
El Salvador	0.2	0.6	50.7	74.2	31.0	68.9	37.1	8
Haiti	0.5	0.3	0.0	62.7	38.5	57.7	51.9	1
Honduras	0.2	0.5	43.8	59.9	29.6	74.0	39.7	16
Jamaica	0.2	0.3	47.2	0.0	32.6	56.5	46.2	4
Mexico	0.2	0.3	41.2	32.1	36.3	84.3	47.8	10
Nicaragua	0.2	0.5	34.7	0.0	28.5	90.0	25.5	2
Panama	0.2	0.7	4.5	16.4	33.8	68.1	57.5	17
Paraguay	0.3	0.7	38.9	10.0	35.0	83.0	48.2	12
Peru	0.4	0.7	38.2	8.9	35.9	86.4	42.1	16
Venezuela, RB	0.4	0.7	42.3	30.9	37.0	18.5	43.8	9
Middle East and North Africa								
Egypt, Arab Rep.	0.1	0.1	16.8	35.6	36.6	68.4	56.9	2
Jordan	0.1	0.6	15.1	37.1	36.1	75.9	62.8	2
Morocco	0.2	0.8	0.0	100.0	39.5	86.8	73.1	1
Tunisia	0.2	0.6	22.9	52.1	35.6	72.8	50.8	1
South Asia								
India	0.4	0.3	13.4	37.0	36.2	61.7	56.7	2
Nepal	0.5	0.4	25.4	51.2	35.2	70.4	55.1	2
Pakistan	0.4	0.4	6.7	52.2	34.7	55.6	52.8	7
Sri Lanka	0.3	0.4	29.5	19.7	37.4	27.7	43.3	10
Sub Saharan Africa								
Burkina Faso	0.4	0.2	7.5	54.9	32.7	86.4	64.4	3
Burundi	0.3	0.1	27.8	31.8	34.5	98.4	62.1	1
Cameroon	0.5	0.3	26.4	30.9	31.7	81.7	59.2	2
Chad	0.3	0.3	0.0	68.6	38.7	66.3	91.6	1
Ethiopia	0.4	0.4	34.6	50.3	34.0	94.2	59.5	7
Gabon	0.3	0.2	33.4	22.8	35.9	88.5	64.1	1
Ghana	0.3	0.4	37.8	9.9	35.2	70.0	62.9	3
Kenya	0.4	0.6	22.8	41.9	34.0	62.1	66.2	1
Madagascar	0.3	0.1	42.5	31.1	35.0	83.6	54.8	1
Malawi	0.6	0.3	20.5	70.8	32.5	37.2	71.0	2
Mauritania	0.5	0.5	0.0	69.8	39.9	69.5	81.2	1
Mauritius	0.2	0.2	34.5	34.8	39.0	.	46.9	5
Mozambique	0.4	0.7	0.0	100.0	39.2	75.5	87.6	2
Niger	0.2	0.2	0.0	0.0	38.2	12.7	90.3	1
Nigeria	0.5	0.3	13.9	46.7	42.2	54.9	83.8	1
Rwanda	0.3	0.2	29.1	90.9	30.2	39.0	51.0	2
Tanzania	0.5	0.5	29.9	73.0	35.2	75.7	63.9	1
Uganda	0.5	0.1	29.6	48.1	30.7	47.6	62.3	1
Zambia	0.5	0.2	47.2	22.3	29.9	83.9	40.5	1

The table reports the country averages for informality, minimum wage ratio, percentage of male, percentage of low educated individuals, average age, percentage of people living in an urban area, percentage of individuals being the head of the household and number of waves per country. The variable signaling the percentage of people living in an urban area is missing for Serbia and Mauritius. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. Informality is defined as the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full-and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights.

Appendix. Details on the Dataset Construction and Sample

When we merge the I2D2 with the ILO Global Wage Database 2012 and we limit our sample to the 1995-2012 period we start with 425 waves for 88 countries.

We then drop the waves for which the minimum wage in the ILO Global Wage Database 2012 is missing either because it does not exist in the years we need or because it is unknown. This way we lose the following countries: Bangladesh, Belize, Comoros, Democratic Republic of Congo, Guyana, Former Yugoslav Republic of Macedonia, Maldives, Puerto Rico, Suriname, Timor-Leste, Turkmenistan, West Bank and Gaza, Republic of Yemen. We are left with 382 waves for 75 countries.

Then we have a look at the distribution of wages and minimum wages. When wages do not seem representative of the country we drop the wave. We are left with 333 waves corresponding to 64 countries. The countries that we lose are Afghanistan, Albania, Botswana, Cambodia, China, Guatemala, Lithuania, Romania, Sierra Leone, Syrian Arab Republic and Vietnam.

We drop cells where the share of self-employed outside of agriculture seems to be zero, and we are left with 333 waves corresponding to 64 countries. We have dropped Croatia because all the self-employed and non-paid employees in the 2004 wave belong to the agriculture sector. Hence, the share of self-employed outside of agriculture in Croatia is zero.

We now exclude cells formed by less than 50 individuals. This way we drop the Russian Federation. We are left with 325 waves corresponding to 62 countries.

We then exclude 2 waves that show some problems in the variables: Togo 2006 (where education labels are not appropriate) and Tanzania 2009 (many values of industry agriculture, mining, manufacturing, public utilities - are missing). This way we also lose the last wave of Togo. We are left with 323 waves relative to 61 countries and 584 cohorts, corresponding to 3,164 observations in the aggregate data, 14,823,986 observations in the individual-level dataset.

If then we exclude cells based on less than 100 individuals we are left with 61 countries, 321 waves, 476 cohorts and 2,746 observations in the aggregate data, 14,611,085 observations

in the individual-level dataset.

We then exclude observations for which the ratio minimum wage over the 70th percentile wage is strictly greater than 1. We are left with 61 countries, 456 cohorts, 321 waves, 2,485 observations in the aggregate data and 13,488,764 observations in the individual-level dataset. As we want to focus our attention on the share of self-employed outside agriculture, we exclude the individuals who work in this sector. We are left with 61 countries, 321 waves, 456 cohorts, 2,485 observations in the aggregate data and 10,095,393 in the individual-level dataset.

List of Countries in our Sample

Argentina 1995; Argentina 1996; Argentina 1997; Argentina 1998; Argentina 1999; Argentina 2000; Argentina 2001; Argentina 2002; Argentina 2003; Argentina 2005; Argentina 2006; Ar-gentina 2007; Argentina 2008; Argentina 2009; Argentina 2010; Argentina 2012; Azerbaijan 1995; Burundi 1998; Burkina Faso 1998; Burkina Faso 2003; Burkina Faso 2009; Bulgaria 2001; Bulgaria 2003; Bulgaria 2007; Bulgaria 2008; Bolivia 1997; Bolivia 1999; Bolivia 2000; Bolivia 2002; Bolivia 2003; Bolivia 2005; Bolivia 2007; Bolivia 2008; Bolivia 2009; Bolivia 2011; Bolivia 2012; Brazil 1995; Brazil 1996; Brazil 1997; Brazil 1998; Brazil 1999; Brazil 2001; Brazil 2002; Brazil 2003; Brazil 2004; Brazil 2005; Brazil 2006; Brazil 2007; Brazil 2008; Brazil 2009; Brazil 2011; Brazil 2012; Cameroon 2001; Cameroon 2007; Colombia 1996; Colombia 1999; Colombia 2001; Colombia 2002; Colombia 2003; Colombia 2004; Colombia 2005; Colombia 2006; Colom-bia 2007; Colombia 2008; Colombia 2009; Colombia 2010; Colombia 2011; Colombia 2012; Costa Rica 2001; Costa Rica 2002; Costa Rica 2003; Costa Rica 2004; Costa Rica 2005; Costa Rica 2006; Costa Rica 2007; Costa Rica 2008; Costa Rica 2009; Dominican Republic 1996; Dominican Republic 1997; Dominican Republic 2000; Dominican Republic 2001; Dominican Republic 2002; Dominican Republic 2003; Dominican Republic 2004; Dominican Republic 2005; Dominican Re-public 2006; Dominican Republic 2007; Dominican Republic 2008; Dominican Republic 2009; Dominican Republic 2010; Dominican Republic 2011; Ecuador 2003; Ecuador 2004; Ecuador 2005; Ecuador 2006; Ecuador 2007; Ecuador 2008; Ecuador 2009; Ecuador 2010; Ecuador 2011; Ecuador 2012; Egypt, Arab Rep. 1998; Egypt, Arab Rep. 2006; Ethiopia 2003; Ethiopia 2004; Ethiopia 2005; Ethiopia 2006; Ethiopia 2009; Ethiopia 2010; Ethiopia 2011; Gabon 2005; Ghana 1998; Ghana 2005; Ghana 2012; Honduras 1995; Honduras 1996; Honduras 1997; Hon-duras 1998; Honduras 1999; Honduras 2001; Honduras 2002; Honduras 2003; Honduras 2004;

Honduras 2005; Honduras 2006; Honduras 2007; Honduras 2008; Honduras 2009; Honduras 2010; Honduras 2011; Haiti 2001; Hungary 2004; Indonesia 1996; Indonesia 1998; Indonesia 1999; Indonesia 2000; Indonesia 2001; Indonesia 2002; Indonesia 2003; Indonesia 2004; Indone-sia 2005; Indonesia 2006; Indonesia 2010; India 1999; India 2007; Jamaica 1996; Jamaica 1999; Jamaica 2001; Jamaica 2002; Jordan 2002; Jordan 2010; Kenya 2005; Kyrgyz Republic 1997; Lao PDR 2002; Lao PDR 2008; Sri Lanka 1996; Sri Lanka 1998; Sri Lanka 1999; Sri Lanka 2000; Sri Lanka 2001; Sri Lanka 2003; Sri Lanka 2004; Sri Lanka 2006; Sri Lanka 2008; Sri Lanka 2009; Latvia 2005; Latvia 2006; Latvia 2007; Latvia 2008; Morocco 1998; Moldova 2005; Madagascar 2001; Mexico 1996; Mexico 1998; Mexico 2000; Mexico 2002; Mexico 2004; Mexico 2005; Mexico 2006; Mexico 2008; Mexico 2010; Mexico 2012; Malta 2009; Malta 2010; Mon-golia 2009; Mongolia 2010; Mongolia 2011; Mozambique 1996; Mozambique 2008; Mauritania 2000; Mauritius 2007; Mauritius 2008; Mauritius 2009; Mauritius 2010; Mauritius 2012; Malawi 2004; Malawi 2010; Niger 2002; Nigeria 2003; Nicaragua 2005; Nicaragua 2009; Nepal 1998; Nepal 2008; Pakistan 1999; Pakistan 2001; Pakistan 2004; Pakistan 2005; Pakistan 2006; Pak-istan 2007; Pakistan 2008; Panama 1995; Panama 1997; Panama 1998; Panama 1999; Panama 2000; Panama 2001; Panama 2002; Panama 2003; Panama 2004; Panama 2005; Panama 2006; Panama 2007; Panama 2008; Panama 2009; Panama 2010; Panama 2011; Panama 2012; Peru 1997; Peru 1998; Peru 1999; Peru 2000; Peru 2001; Peru 2002; Peru 2003; Peru 2004; Peru 2005; Peru 2006; Peru 2007; Peru 2008; Peru 2009; Peru 2010; Peru 2011; Peru 2012; Philip-pines 2001; Philippines 2002; Philippines 2003; Philippines 2004; Philippines 2005; Philippines 2006; Philippines 2007; Philippines 2008; Philippines 2009; Philippines 2010; Philippines 2011; Paraguay 1995; Paraguay 1997; Paraguay 1999; Paraguay 2001; Paraguay 2002; Paraguay 2003; Paraguay 2004; Paraguay 2006; Paraguay 2007; Paraguay 2008; Paraguay 2009; Paraguay 2010; Rwanda 2005; Rwanda 2010; Solomon Islands 2005; El Salvador 1995; El Salvador 1996; El Salvador 1998; El Salvador 1999; El Salvador 2000; El Salvador 2001; El Salvador 2006; El Salvador 2007; Serbia 2008; Chad 2003; Thailand 2000; Thailand 2002; Thailand 2006; Thai-land 2009; Tajikistan 1999; Tajikistan 2003; Tunisia 2000; Turkey 2005; Turkey 2006; Turkey 2007; Turkey 2008; Turkey 2009; Turkey 2010; Tanzania 2006; Uganda 2005; Uruguay 1995; Uruguay 1996; Uruguay 1997; Uruguay 1998; Uruguay 2000; Uruguay 2001; Uruguay 2002; Uruguay 2003; Uruguay 2004; Uruguay 2005; Uruguay 2006; Uruguay 2007; Uruguay 2008; Uruguay 2009; Uruguay 2010; Uruguay 2011; Uruguay 2012; Venezuela, RB 1995; Venezuela, RB 1998; Venezuela, RB 2000; Venezuela, RB 2001; Venezuela, RB 2002; Venezuela, RB 2003; Venezuela, RB 2004; Venezuela, RB 2005; Venezuela, RB 2006; Zambia 2010.

SUPPLEMENTAL ONLINE APPENDIX

Appendix Table A3.1. Effects of MW Ratio on Share of Self-employed (First Wave Only)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.284***	0.284***	0.284***	0.085	0.324***	0.222***
	(0.065)	(0.065)	(0.065)	(0.065)	(0.092)	(0.050)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	405	405	405	405	405	385
No. Countries	61	61	61	61	61	59
R-sqr overall	0.116	0.116	0.116	0.133	0.158	0.294
Observations	918936	918936	918936	918936	918936	837385

The table reports the effect of an increase in the minimum wage ratio on the share of high educated (secondary or post-secondary education) self-employed when only the first wave per each country is considered. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.2. Effects of MW Ratio on Share of Self-employed (Cohorts with more than 200 observations)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.236***	0.249***	0.292***	0.280***	0.279***	0.176***
	(0.020)	(0.020)	(0.020)	(0.028)	(0.019)	(0.015)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	2084	2084	2084	2084	2084	1990
No. Countries	58	58	58	58	58	56
R-sqr overall	0.072	0.072	0.080	0.106	0.122	0.232
Observations	9958551	9958551	9958551	9958551	9958551	9463195

The table reports the effect of an increase in the minimum wage ratio on the share of self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 200 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.3. Effects of MW Ratio on Share of Self-employed (Variable Sector <10% missing)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.214***	0.227***	0.257***	0.262***	0.264***	0.214***
	(0.018)	(0.019)	(0.018)	(0.027)	(0.019)	(0.017)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	1909	1909	1909	1909	1909	1802
No. Countries	40	40	40	40	40	38
R-sqr overall	0.077	0.078	0.084	0.113	0.132	0.239
Observations	6786867	6786867	6786867	6786867	6786867	6364718

The table reports the effect of an increase in the minimum wage ratio on the share of self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations, where the minimum wage is below the 70th percentile wage and we exclude waves only surveys where the variable sector has more than 10% missing values . The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.4. Effects of MW Ratio on Share of Self-employed (Non-linear Effects - Female)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW 2nd decile wages	0.025**	0.030**	0.054***	0.050***	0.033***	0.018***
	(0.012)	(0.012)	(0.012)	(0.010)	(0.005)	(0.005)
MW 3rd decile wages	0.044***	0.050***	0.077***	0.086***	0.072***	0.040***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.006)	(0.006)
MW 4th decile wages	0.087***	0.098***	0.150***	0.118***	0.092***	0.045***
	(0.017)	(0.018)	(0.017)	(0.016)	(0.008)	(0.008)
MW 5th decile wages	0.113***	0.121***	0.153***	0.147***	0.097***	0.055***
	(0.022)	(0.021)	(0.021)	(0.021)	(0.010)	(0.010)
MW 6th decile wages	0.167***	0.174***	0.227***	0.208***	0.093***	0.055***
	(0.026)	(0.025)	(0.023)	(0.021)	(0.011)	(0.011)
MW 7th decile wages	0.221***	0.231***	0.279***	0.230***	0.090***	0.051***
	(0.020)	(0.020)	(0.018)	(0.022)	(0.012)	(0.011)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	1027	1027	1027	1027	1027	978
No. Countries	55	55	55	55	55	53
R-sqr overall	0.105	0.106	0.119	0.150	0.162	0.321
Observations	3652341	3652341	3652341	3652341	3652341	3435857

The table reports the effect of an increase in the minimum wage ratio on the share of female self-employed only. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. *MW 2nd decile wages* is a dummy equal to 1 if the minimum wage is between the 10th and 20th percentile of the cohort wage distribution of (possibly full-time) wage workers outside of agriculture and the public sector. *MW 3rd decile wages* is a dummy equal to 1 if the minimum wage is between the 20th and 30th percentile of the cohort wage distribution of (possibly full-time) wage workers outside agriculture and the public sector, etc. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old and a dummy for individuals who are 30-50 years old. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.5. Effects of MW Ratio on Share of Self-employed (Non-linear Effects - Male)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW 2nd decile wages	0.039***	0.042***	0.068***	0.044***	0.027***	0.014***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.004)	(0.003)
MW 3rd decile wages	0.073***	0.076***	0.112***	0.076***	0.038***	0.021***
	(0.011)	(0.011)	(0.012)	(0.010)	(0.006)	(0.005)
MW 4th decile wages	0.087***	0.093***	0.127***	0.093***	0.051***	0.027***
	(0.017)	(0.015)	(0.015)	(0.014)	(0.008)	(0.007)
MW 5th decile wages	0.093***	0.100***	0.148***	0.096***	0.045***	0.023***
	(0.014)	(0.014)	(0.014)	(0.013)	(0.009)	(0.008)
MW 6th decile wages	0.108***	0.114***	0.158***	0.108***	0.041***	0.019*
	(0.015)	(0.015)	(0.018)	(0.015)	(0.012)	(0.011)
MW 7th decile wages	0.117***	0.120***	0.170***	0.110***	0.045***	0.027**
	(0.021)	(0.020)	(0.017)	(0.016)	(0.012)	(0.011)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	1456	1456	1456	1456	1456	1398
No. Countries	61	61	61	61	61	59
R-sqr overall	0.069	0.070	0.077	0.095	0.104	0.188
Observations	6443052	6443052	6443052	6443052	6443052	6150392

The table reports the effect of an increase in the minimum wage ratio on the share of male self-employed only. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. *MW 2nd decile wages* is a dummy equal to 1 if the minimum wage is between the 10th and 20th percentile of the cohort wage distribution of (possibly full-time) wage workers outside of agriculture and the public sector. *MW 3rd decile wages* is a dummy equal to 1 if the minimum wage is between the 20th and 30th percentile of the cohort wage distribution of (possibly full-time) wage workers outside agriculture and the public sector, etc. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old and a dummy for individuals who are 30-50 years old. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.6. Effects of MW Ratio on Share of Self-employed (Non-linear Effects - Young)

	(1)	(2)	(3)	(4)	(5)	(6)
	Self-employed Share	Self-employed Share	Self-employed Share	Self-employed Share	Self-employed Share	Self-employed Share
MW 2nd decile wages	0.013	0.016	0.036***	-0.015	0.009	0.003
	(0.010)	(0.010)	(0.011)	(0.012)	(0.007)	(0.007)
MW 3rd decile wages	0.044***	0.050***	0.089***	-0.010	0.022**	0.018*
	(0.013)	(0.013)	(0.013)	(0.016)	(0.010)	(0.011)
MW 4th decile wages	0.048***	0.058***	0.117***	0.005	0.034***	0.029**
	(0.014)	(0.015)	(0.017)	(0.021)	(0.013)	(0.013)
MW 5th decile wages	0.052***	0.061***	0.138***	-0.007	0.046***	0.045***
	(0.014)	(0.014)	(0.015)	(0.023)	(0.014)	(0.014)
MW 6th decile wages	0.077***	0.090***	0.189***	0.021	0.071***	0.071***
	(0.018)	(0.018)	(0.017)	(0.025)	(0.016)	(0.018)
MW 7th decile wages	0.141***	0.153***	0.222***	0.041	0.085***	0.092***
	(0.020)	(0.020)	(0.022)	(0.029)	(0.018)	(0.019)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	880	880	880	880	880	849
No. Countries	54	54	54	54	54	52
R-sqr overall	0.098	0.099	0.111	0.114	0.127	0.192
Observations	3172997	3172997	3172997	3172997	3172997	2981582

The table reports the effect of an increase in the minimum wage ratio on the share of 18-29 years old self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. *MW 2nd decile wages* is a dummy equal to 1 if the minimum wage is between the 10th and 20th percentile of the cohort wage distribution of (possibly full-time) wage workers outside of agriculture and the public sector. *MW 3rd decile wages* is a dummy equal to 1 if the minimum wage is between the 20th and 30th percentile of the cohort wage distribution of (possibly full-time) wage workers outside agriculture and the public sector, etc. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling) and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.7. Effects of MW Ratio on Share of Self-employed (Non-linear Effects - Prime Age)

	(1)	(2)	(3)	(4)	(5)	(6)
	Self-employed Share	Self-employed Share	Self-employed Share	Self-employed Share	Self-employed Share	Self-employed Share
MW 2nd decile wages	0.081***	0.085***	0.127***	0.064***	0.023***	0.023***
	(0.014)	(0.014)	(0.012)	(0.014)	(0.006)	(0.005)
MW 3rd decile wages	0.122***	0.129***	0.193***	0.104***	0.044***	0.039***
	(0.014)	(0.014)	(0.013)	(0.018)	(0.008)	(0.007)
MW 4th decile wages	0.151***	0.163***	0.248***	0.125***	0.056***	0.048***
	(0.018)	(0.020)	(0.015)	(0.021)	(0.009)	(0.009)
MW 5th decile wages	0.201***	0.211***	0.278***	0.139***	0.098***	0.074***
	(0.025)	(0.025)	(0.025)	(0.030)	(0.012)	(0.010)
MW 6th decile wages	0.261***	0.269***	0.355***	0.190***	0.131***	0.092***
	(0.029)	(0.025)	(0.021)	(0.032)	(0.014)	(0.011)
MW 7th decile wages	0.305***	0.312***	0.382***	0.222***	0.151***	0.103***
	(0.019)	(0.019)	(0.015)	(0.026)	(0.012)	(0.010)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	980	980	980	980	980	942
No. Countries	61	61	61	61	61	59
R-sqr overall	0.096	0.097	0.115	0.118	0.139	0.264
Observations	5560550	5560550	5560550	5560550	5560550	5305033

The table reports the effect of an increase in the minimum wage ratio on the share of 30-50 years old self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. *MW 2nd decile wages* is a dummy equal to 1 if the minimum wage is between the 10th and 20th percentile of the cohort wage distribution of (possibly full-time) wage workers outside of agriculture and the public sector. *MW 3rd decile wages* is a dummy equal to 1 if the minimum wage is between the 20th and 30th percentile of the cohort wage distribution of (possibly full-time) wage workers outside agriculture and the public sector, etc. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling) and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.8. Effects of MW Ratio on Share of Self-employed (Non-linear Effects - Old)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW 2nd decile wages	0.040***	0.057***	0.096***	0.058***	0.024***	0.026***
	(0.011)	(0.010)	(0.010)	(0.011)	(0.006)	(0.006)
MW 3rd decile wages	0.112***	0.121***	0.169***	0.113***	0.039***	0.048***
	(0.014)	(0.013)	(0.011)	(0.013)	(0.008)	(0.008)
MW 4th decile wages	0.164***	0.186***	0.208***	0.135***	0.031***	0.042***
	(0.014)	(0.014)	(0.014)	(0.017)	(0.010)	(0.010)
MW 5th decile wages	0.231***	0.256***	0.309***	0.210***	0.087***	0.082***
	(0.016)	(0.017)	(0.013)	(0.019)	(0.013)	(0.013)
MW 6th decile wages	0.234***	0.254***	0.323***	0.224***	0.105***	0.099***
	(0.019)	(0.018)	(0.014)	(0.021)	(0.016)	(0.015)
MW 7th decile wages	0.226***	0.248***	0.322***	0.222***	0.096***	0.094***
	(0.026)	(0.024)	(0.016)	(0.022)	(0.015)	(0.014)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	622	622	622	622	622	584
No. Countries	39	39	39	39	39	37
R-sqr overall	0.082	0.084	0.093	0.094	0.102	0.264
Observations	1361846	1361846	1361846	1361846	1361846	1299634

The table reports the effect of an increase in the minimum wage ratio on the share of 51-65 years old self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. *MW 2nd decile wages* is a dummy equal to 1 if the minimum wage is between the 10th and 20th percentile of the cohort wage distribution of (possibly full-time) wage workers outside of agriculture and the public sector. *MW 3rd decile wages* is a dummy equal to 1 if the minimum wage is between the 20th and 30th percentile of the cohort wage distribution of (possibly full-time) wage workers outside agriculture and the public sector, etc. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling) and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.9. Effects of MW Ratio on Share of Self-employed (Non-linear Effects - Low Education)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW 2nd decile wages	-0.006	-0.002	0.015	0.011	0.017**	0.009
	(0.018)	(0.018)	(0.021)	(0.017)	(0.007)	(0.006)
MW 3rd decile wages	-0.011	-0.011	0.006	0.001	0.042***	0.024***
	(0.018)	(0.019)	(0.022)	(0.020)	(0.009)	(0.007)
MW 4th decile wages	0.001	0.008	0.036	-0.001	0.043***	0.024***
	(0.018)	(0.020)	(0.025)	(0.023)	(0.010)	(0.009)
MW 5th decile wages	0.027	0.033	0.060**	0.059**	0.058***	0.033***
	(0.020)	(0.022)	(0.026)	(0.026)	(0.012)	(0.010)
MW 6th decile wages	0.037	0.043*	0.080***	0.072***	0.062***	0.033***
	(0.024)	(0.024)	(0.028)	(0.027)	(0.014)	(0.012)
MW 7th decile wages	0.061***	0.066***	0.102***	0.074**	0.058***	0.027**
	(0.022)	(0.023)	(0.027)	(0.030)	(0.014)	(0.012)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	1023	1023	1023	1023	1023	977
No. Countries	53	53	53	53	53	51
R-sqr overall	0.092	0.092	0.099	0.118	0.136	0.237
Observations	3699524	3699524	3699524	3699524	3699524	3514383

The table reports the effect of an increase in the minimum wage ratio on the share of low educated (primary education or no schooling) self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. *MW 2nd decile wages* is a dummy equal to 1 if the minimum wage is between the 10th and 20th percentile of the cohort wage distribution of (possibly full-time) wage workers outside of agriculture and the public sector. *MW 3rd decile wages* is a dummy equal to 1 if the minimum wage is between the 20th and 30th percentile of the cohort wage distribution of (possibly full-time) wage workers outside agriculture and the public sector, etc. The Z dummies included are: a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.10. Effects of MW Ratio on Share of Self-employed (Non-linear Effects - High Education)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW 2nd decile wages	-0.006	-0.011*	-0.010	0.025***	0.006	0.002
	(0.006)	(0.006)	(0.007)	(0.007)	(0.004)	(0.003)
MW 3rd decile wages	-0.026***	-0.032***	-0.037***	0.029***	0.001	-0.003
	(0.008)	(0.008)	(0.009)	(0.011)	(0.005)	(0.005)
MW 4th decile wages	-0.018	-0.024*	-0.020	0.070***	0.000	-0.011
	(0.013)	(0.014)	(0.013)	(0.015)	(0.007)	(0.007)
MW 5th decile wages	-0.052***	-0.061***	-0.075***	0.029	-0.001	-0.011
	(0.014)	(0.013)	(0.013)	(0.021)	(0.009)	(0.009)
MW 6th decile wages	-0.062***	-0.072***	-0.107***	0.021	-0.008	-0.016
	(0.015)	(0.015)	(0.014)	(0.018)	(0.013)	(0.012)
MW 7th decile wages	-0.016	-0.027	-0.080***	0.062**	0.002	0.010
	(0.031)	(0.029)	(0.028)	(0.027)	(0.014)	(0.013)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	1460	1460	1460	1460	1460	1399
No. Countries	59	59	59	59	59	57
R-sqr overall	0.055	0.055	0.062	0.067	0.081	0.213
Observations	6395869	6395869	6395869	6395869	6395869	6071866

The table reports the effect of an increase in the minimum wage ratio on the share of high educated (secondary or post-secondary education) self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. *MW 2nd decile wages* is a dummy equal to 1 if the minimum wage is between the 10th and 20th percentile of the cohort wage distribution of (possibly full-time) wage workers outside of agriculture and the public sector. *MW 3rd decile wages* is a dummy equal to 1 if the minimum wage is between the 20th and 30th percentile of the cohort wage distribution of (possibly full-time) wage workers outside agriculture and the public sector, etc. The Z dummies included are: a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.11. Effects of MW Ratio on Share of Self-employed (Non-linear Effects - Cohorts with more than 200 observations)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW 2nd decile wages	0.036***	0.041***	0.067***	0.061***	0.040***	0.022***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.005)	(0.004)
MW 3rd decile wages	0.054***	0.059***	0.092***	0.078***	0.059***	0.036***
	(0.010)	(0.010)	(0.010)	(0.009)	(0.006)	(0.005)
MW 4th decile wages	0.085***	0.095***	0.128***	0.109***	0.082***	0.049***
	(0.013)	(0.013)	(0.012)	(0.012)	(0.008)	(0.007)
MW 5th decile wages	0.109***	0.118***	0.158***	0.144***	0.105***	0.065***
	(0.014)	(0.014)	(0.015)	(0.015)	(0.009)	(0.008)
MW 6th decile wages	0.152***	0.161***	0.202***	0.179***	0.125***	0.082***
	(0.021)	(0.020)	(0.019)	(0.019)	(0.012)	(0.010)
MW 7th decile wages	0.207***	0.213***	0.255***	0.214***	0.144***	0.100***
	(0.018)	(0.017)	(0.016)	(0.018)	(0.012)	(0.010)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	2084	2084	2084	2084	2084	1990
No. Countries	58	58	58	58	58	56
R-sqr overall	0.073	0.074	0.082	0.107	0.122	0.232
Observations	9958551	9958551	9958551	9958551	9958551	9463195

The table reports the non-linear effects of an increase in the minimum wage ratio on the share of self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 200 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. *MW 2nd decile wages* is a dummy equal to 1 if the minimum wage is between the 10th and 20th percentile of the cohort wage distribution of (possibly full-time) wage workers outside of agriculture and the public sector. *MW 3rd decile wages* is a dummy equal to 1 if the minimum wage is between the 20th and 30th percentile of the cohort wage distribution of (possibly full-time) wage workers outside agriculture and the public sector, etc. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.12. Effects of MW Ratio on Share of Self-employed (Non-linear Effects - Variable Sector <10% missing)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW 2nd decile wages	0.035***	0.041***	0.062***	0.050***	0.037***	0.024***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.005)	(0.005)
MW 3rd decile wages	0.044***	0.050***	0.080***	0.073***	0.057***	0.046***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.006)	(0.006)
MW 4th decile wages	0.082***	0.091***	0.122***	0.104***	0.077***	0.059***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.008)	(0.007)
MW 5th decile wages	0.115***	0.125***	0.160***	0.145***	0.098***	0.080***
	(0.016)	(0.017)	(0.016)	(0.016)	(0.010)	(0.008)
MW 6th decile wages	0.135***	0.143***	0.187***	0.162***	0.107***	0.090***
	(0.017)	(0.017)	(0.017)	(0.017)	(0.011)	(0.010)
MW 7th decile wages	0.192***	0.202***	0.246***	0.194***	0.127***	0.113***
	(0.019)	(0.019)	(0.018)	(0.020)	(0.012)	(0.010)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	1909	1909	1909	1909	1909	1802
No. Countries	40	40	40	40	40	38
R-sqr overall	0.078	0.079	0.086	0.114	0.132	0.239
Observations	6786867	6786867	6786867	6786867	6786867	6364718

The table reports the non-linear effects of an increase in the minimum wage ratio on the share of self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations, where the minimum wage is below the 70th percentile wage and we exclude waves only surveys where the variable sector has more than 10% missing values . The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. *MW 2nd decile wages* is a dummy equal to 1 if the minimum wage is between the 10th and 20th percentile of the cohort wage distribution of (possibly full-time) wage workers outside of agriculture and the public sector. *MW 3rd decile wages* is a dummy equal to 1 if the minimum wage is between the 20th and 30th percentile of the cohort wage distribution of (possibly full-time) wage workers outside agriculture and the public sector, etc. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.13. Effects of MW Ratio on Share of Self-employed (Self-employed Only)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.152***	0.156***	0.188***	0.168***	0.177***	0.105***
	(0.018)	(0.018)	(0.019)	(0.024)	(0.016)	(0.016)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	2484	2484	2484	2484	2484	2377
No. Countries	61	61	61	61	61	59
R-sqr overall	0.060	0.061	0.068	0.101	0.119	0.203
Observations	10095393	10095393	10095393	10095393	10095393	9586249

The table reports the effect of an increase in the minimum wage ratio on the share of self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of self-employed only outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.14. Effects of MW Ratio on Share of Self-employed (Low Educated Self-employed Only)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.251***	0.260***	0.309***	0.219***	0.195***	0.133***
	(0.018)	(0.019)	(0.020)	(0.024)	(0.016)	(0.015)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	2484	2484	2484	2484	2484	2377
No. Countries	61	61	61	61	61	59
R-sqr overall	0.065	0.066	0.075	0.118	0.136	0.219
Observations	10095393	10095393	10095393	10095393	10095393	9586249

The table reports the effect of an increase in the minimum wage ratio on the share of self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of low educated (primary education or no schooling) self-employed only outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.15. Effects of MW Ratio on Share of Self-employed (Low Educated Self-employed and Family Workers Only)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW ratio	0.330***	0.345***	0.409***	0.282***	0.271***	0.193***
	(0.020)	(0.020)	(0.020)	(0.026)	(0.020)	(0.015)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	2484	2484	2484	2484	2484	2377
No. Countries	61	61	61	61	61	59
R-sqr overall	0.083	0.085	0.097	0.132	0.153	0.261
Observations	10095393	10095393	10095393	10095393	10095393	9586249

The table reports the effect of an increase in the minimum wage ratio on the share of self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the 70th percentile wage. The share of self-employed is the share of low educated (primary education or no schooling) self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Appendix Table A3.16. Effects of MW Ratio on Share of Self-employed (MW Ratio Based on Median Wages)

	(1) Self-employed Share	(2) Self-employed Share	(3) Self-employed Share	(4) Self-employed Share	(5) Self-employed Share	(6) Self-employed Share
MW Ratio-median	0.189***	0.196***	0.238***	0.223***	0.204***	0.129***
	(0.016)	(0.017)	(0.017)	(0.021)	(0.013)	(0.011)
CountryFE	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	No	Yes	Yes	Yes	Yes	Yes
Country × YearFE	No	No	Yes	Yes	Yes	Yes
Z dummies	No	No	No	Yes	Yes	Yes
Z dummies × CountryFE	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	Yes
Degrees of freedom	2484	2484	2484	2484	2484	2377
No. Countries	61	61	61	61	61	59
R-sqr overall	0.076	0.077	0.087	0.112	0.131	0.245
Observations	10095393	10095393	10095393	10095393	10095393	9586249

The table reports the effect of an increase in the minimum wage ratio on the share of self-employed. The sample comes from merging the I2D2 dataset and the ILO Global Wage Database 2012. We keep household surveys of developing countries where the minimum wage exists; we further limit our sample to labor market groups (cohorts) formed by more than 100 observations and where the minimum wage is below the median wage. The share of self-employed is the share of self-employed and non-paid employees (i.e. family worker) outside of agriculture. Only full-time self-employed are considered when they can be identified; both full- and part-time self-employed are considered when full-time self-employed cannot be identified. The minimum wage ratio is calculated as the minimum wage over the 70th percentile wage of (full-time if possible) wage workers outside of agriculture and the public sector; wages are weighted with survey weights. The Z dummies included are: a dummy signaling if the individual has a low level of education (primary education or no schooling), a dummy for individuals who are 18-29 years old, a dummy for individuals who are 30-50 years old and a dummy for being male. The controls included in the 6th Column are dummies for industry (agriculture, manufacturing, etc.), urban/rural, whether the individual is the head of the household/spouse/other and the household size. In the estimations equal weights are given to each labor market group. Standard errors are clustered at cohort level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

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