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Caio Piza
André Portela Souza

THE CAUSAL IMPACTS OF CHILD LABOR LAW IN BRAZIL: SOME PRELIMINARY FINDINGS

Caio Piza
The World Bank Development Research Group
Contact email: caiopiza@worldbank.org

André Portela Souza
Professor of Economics at Sao Paulo School of Economics (EESP-FGV)
Contact email: andre.portela.souza@fgv.br

Abstract

This paper investigates the causal impact of the change in law of December 1998 that increased the minimum legal age of entry into the labor force from 14 to 16. We used a difference-in-differences (DD) approach to estimate the impact of this law change on labor force participation rates as a whole, as well as for the formal and informal sectors separately. Our results showed that the ban reduced participation rates for boys by 4 percentage points and that this effect was mostly driven by the informal sector. We found no effect on girls.

Keywords: Child labor ban, child labor, participation rates, and treatment effects.

JEL: J08, J22, J23.

INTRODUCTION

Literature on child labor has grown considerably over the last 15 years, and this is not just because of increasing data availability. Child labor has fallen over the years, but the worldwide figures are still alarming. According to the International Labour Organization (ILO), 264 million children aged 5 to 17 participated in the labor market in 2012. Of this, 168 million were child labor, i.e. either under minimum legal age and/or working in hazardous activities.

Due to the negative externalities associated with children's participation in the labor force, it is argued that the public sector could intervene by changing the circumstances that cause parents to send their children to work (see Basu and Van, 1998). In fact, many countries have adopted bans or other mechanisms to break down the 'inter-generational child labor trap' (see Emerson and Souza 2003 and Edmonds 2008 for a survey).

Basu and Van (1998), for instance, argue that a parent's decision to send a child into the labor force might be seen as a rational choice in a poor household facing many constraints. Given a few assumptions, the labor market may have multiple stable equilibria, one characterised by children participating in the labor force and depressed adult wages, and another in which children do not participate in the labor force and adult wages are higher. Because these two equilibria are stable, the authors argue that if children participate in the labor force, a ban could be put in place by the government to shift the economy from this equilibrium to one without child labor.[1]

Although many policies have been put in place to fight child labor, too little is known about the causal impact of such interventions, particularly in developing countries where empirical evidence is almost non-existent.[2] This paper helps fill this gap by delving into the consequences of two recent federal legislations in Brazil aimed at children of a certain age range.[3]

In December 1998, Brazil increased the minimum employment age from 14 to 16. This policy change gave rise to a natural experiment that we will investigate in this paper.[4]

We used a difference-in-differences (DD) approach to estimate the impact of the law passed in December 1998. The results suggest that the change in law reduced labor force participation rates of 14 year-old boys as a whole, but not of girls, who seem to have shifted to the informal sector. The results for boys are interesting as they indicate

[1] The two main assumptions stemming from this result are that the rise in adult wages that results from a ban has to be high enough to compensate for the children's 'forgone' income; and that it allows parents to consume children's leisure. That means: (i) that child and adult labour inputs are perfect substitutes, and (ii) that children's leisure is a normal good for the parents. This theoretical framework therefore suggests that net benefit for households from a child labour ban is ultimately an empirical question.

[2] The only study we are aware of for a developing country is Bharadwaj et al. (2013) for India.

[3] The available evidence for such policies comes almost exclusively from the U.S., mainly the impact of minimum legal age legislations at the beginning of the last century. See, for instance, Moehling (1999), Margo and Finegan (1996), Lleras-Muney (2002), Tyler (2003), and Manacorda (2006).

[4] Ferro and Kassouf (2005) studied the impact of the law by looking at cohorts of 14 and 15 year-old children between 1995 and 2003. Their analysis is akin to a before-and-after analysis without a comparison group.

that a law can be a powerful instrument that affects individuals, including those working informally.[5]

The remaining part of this paper is organized as follows: The second section briefly discusses the change in law and provides the rationale as to how these two laws might affect children's time allocation. The third section presents the data, while the fourth presents the identification strategy. The results are discussed in section 5 and the conclusion highlights the main findings of the paper and outlines some policy recommendations.

2. THE CHANGE IN LAW OF DECEMBER 1998

The Brazilian Constitution of 1988 set the minimum legal age of entry into the labor market at 14. In 1990, a federal rule named 'The Statute of Children and Adolescents'[6] established children and youth rights. Complementary to the Constitution of 1988, the statute is considered the legal framework for children and youth in the labor market.[7] From 1988 to November 1998, the minimum legal working age in Brazil was 14 and individuals below age 17 were prohibited from working in hazardous conditions.

As a consequence of comprehensive modifications approved for the pension system on December 15, 1998, Constitutional Amendment No. 20 increased the minimum legal age for entry into the labor market from 14 to 16.[8] This change in law did not affect children who turned 14 before the law was passed; but those who turned 14 after the change were banned from the formal labor force.

The relatively large informal sector in Brazil can cast doubts on the effectiveness of the law. However, the effect of this intervention on participation rates of the treatment group depends on its enforceability and also on the size of the problem it is trying to fix. If some of the children participating in the formal sector simply shifted to the informal sector after the ban, the effect of the law on children's participation rates would be negligible or even positive. But, if some employers decided to no longer employ children below age 16 to avoid legal consequences (such as paying fines), the law would probably reduce participation rates in the informal sector as well.

[5] We are working on regression discontinuity regressions and looking at a wider range of outcomes. Some preliminary results on participation rates are aligned with the main findings discussed here. However, we also find that the ban affected school attendance of girls and had heterogeneous long-term effects on white and non-white males.

[6] *Lei do Estatuto e do Adolescente*, Law No.8069 from July 13, 1990. Complementary to the Constitution of 1988, the statute is considered the legal framework for children and youth in the labour market.

[7] ILO considers an individual 17 years or younger as a child. In this paper, the terms children, teenagers, and youth are used interchangeably.

[8] The law was passed on December 15, 1998 and came into effect the following day.

3. EMPIRICAL STRATEGY

To estimate the effect of the law of 1998, children who turned age 14 after the law change of December 1998 (treatment) were compared with those who turned age 14 before the change in law (comparison). As differences in labor force participation can arise from differences in ages or cohorts, we applied two DD estimates as control for these potential confounding effects. One estimate uses as counterfactual the difference in participation rates between those who turned 14 after December 1997 and those who turned 14 before December 1997. This is the different-cohorts-and-same-ages DD. In the second estimate, the counterfactual is the difference in participation rates between those who turned 13 after December 1997 and those who turned 13 on or before December 1997. This is the different-ages-and-same-cohorts DD. Table 1 shows how the 'treatment' and 'comparison' groups are defined for these two estimates.

Since we controlled for age (in weeks) linearly, the first difference in the DD exercise can be interpreted as a parametric regression discontinuity design.

Table 1 – Definition of the Eligible and Comparison Groups for the DD Estimates

	PNAD 1998	PNAD 1999
	Different cohorts and same ages	
Treatment Group	Turned 14 between January and June, 1998	Turned 14 between January and June, 1999
Comparison Group	Turned 14 between July and December, 1997	Turned 14 between July and December, 1998
	Different ages and same cohorts	
Treatment Group	Turned 13 between January and June, 1998	Turned 14 between January and June, 1999
Comparison Group	Turned 13 between July and December, 1997	Turned 14 between July and December, 1998

The identification strategy for the DD depends on two assumptions: (1) any difference in labor force participation between the treatment and control groups exists in level but not in difference. That means the groups would show a common trend in the absence of the changes in law; and (2) all unobserved characteristics that can be correlated with the eligibility status of the individual or other covariates are additive and time-invariant. In our first regression model, we assumed that any differences in participation rates between treated and comparison groups that are due to age effects, is constant over time. Whereas in the second regression model, we assumed a cohort effect that is fixed over time (see Meyer 1995).

The estimation of the impact of the law of 1998 on the outcomes of interest is conducted through the linear probability regression model:

$$Y_{it} = \beta_0 + X'_{it}\beta_1 + \beta_2 T_i + \beta_3 D_{99} + \delta T_i D_{99} + u_{it}$$

where Y_{it} is the outcome variable of individual i in time t, X_{it} is the vector of observed characteristics of individual i in time t. The vector includes dummies gender and ethnicity, the years of schooling of the household head, the age of the household head, the gender of the household head, and dummy variables for regions and the metropolitan region. In our first regression, T_i is a dummy variable that equals 1 if individual i turned aged 14 in the first semester of 1998 or 1999, and 0 if individual i turned 14 in the second semester of 1997 or 1998, D_{99} is a year dummy that takes value

1 in 1999 (after the law was passed) and zero for 1998 (before the law was passed), and u_{it} denotes an idiosyncratic error term. For this regression, we also include as control the individual´s age (in weeks). In our second regression, T_i takes the value of 1 if individual *i* in cohort c turned age 14 in the first semester of 1999 and 0 if (s)he belongs to the cohort that turned 14 in the second half of 1998. Table 1 shows the groups under comparison in each case. Because the data provides exact dates of birth, we run the regressions using a 26 week-bandwidth (6-month) as can be inferred from table 1.

In both regression models, the parameter of interest, δ , provides the intent-to-treat (ITT) effect. In other words, the impact of the ban on the cohort hindered from participating in the formal labor force at age 14.

4. DATA & RESULTS

The sample used in this paper is drawn from the Brazilian household surveys (*Pesquisa Nacional por Amostra de Domicilios* – PNAD) of 1998 and 1999. The PNAD is an annual household survey that covers around 100,000 households and about 320,000 individuals. It is a major source of microdata in Brazil, and is a nationally representative survey that contains detailed information on each household's socio-economic characteristics, demographic data, household income, and labor force status. The Brazilian Bureau of Statistics (IBGE) provides the exact reference date for when data collection is carried out and we used that information to identify the treatment and comparison groups.[9] Our results are exclusive for urban areas.

We used three measures of child labor: labor force participation rates as a whole without distinguishing between sectors, participation in the formal labor force, and participation rates in the informal labor force.[10]

Table 2 shows ITT estimates on child labor incidence and reports the mean for the comparison group before the change in law. As can be seen, participation rates among boys are much higher compared to girls, and predominantly informal. We could therefore expect a higher impact of the change in law on boys. The first block of table 2 compares different cohorts keeping the age differential constant, whereas the second block compares the same cohort before and after the change in law. The estimates are very similar and point to a reduction in child labor of 4 percentage points for boys. Compared to a different cohort, the fall in participation rates was of 36 percent over the comparison group. Instead, if we used participation rates of the comparison group one year before, the drop in participation rates approached 80 percent. In both cases, the effect is almost fully driven by a reduction in participation rates in the informal sector. We found no impact on girls.

CONCLUSION

This paper contributes to the literature on the impact of child labor laws by delving into the short-term impacts of the Brazilian Constitutional Amendment of

[9] According to IBGE, the reference date is the last week of September. There can be some variability in the exact day, but it is usually September 26 or 27.

[10] The dummy variable for labour force participation rates takes the value of 1 if the individual is either (1) employed or looking for a job or was an active worker in the week of reference but was prevented from working due to external causes in the week of reference, or (2) worked in the last 12 months, and zero otherwise. A formal worker is someone working with a work permit (*carteira assinada*) issued by the Brazilian Ministry of Labour and zero otherwise, whereas an informal worker is someone who works without such permission. This definition does not include domestic servants. In Brazil, domestic servants are covered by a separate legislation.

December 1998, which increased the minimum legal age of entry into the labor force from 14 to 16. Unlike other estimates available in the literature, we present estimates for both formal and informal sectors separately. DD estimates show that the ban reduced labor force participation rate for boys only. The impact is almost fully explained by the fall in participation rates in the informal sector. We found no impact on girls.

These preliminary findings suggest that the law was biding and general measures are more effective when the incidence is somewhat pervasive, as in the case of boys. One may conjecture the reasons why there is a strong effect among informal workers. It is possible that the child labor ban reduced verification costs by local authorities and increased chances of firms getting caught for breaking the law. Before the change in law, a child aged 14 could work formally. The cost of verification of a worker hired formally or informally might be greater than the simpler verification of whether a child is working at all. The firms seem to have responded to the change in the law by reducing the number of working children. Policymakers thus should have a broader perspective when they pass such laws, and take into account their heterogeneous effects on time allocation of children.

Table 2 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on Extensive Margin of Labor Supply

	Labor Force Participation Rates			Participation Rates – Formal Labor Force			Participation Rates – Informal Labor Force		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
				Different cohorts and same ages					
Eligible*D_{99} (*DD*)	-0.019**	-0.040***	0.0035	-0.0048	-0.0074	-0.0016	-0.014*	-0.032**	0.0051
	(-2.40)	(-2.89)	(0.45)	(-1.42)	(-1.27)	(-0.47)	(-1.94)	(-2.55)	(0.73)
Eligible	-0.017***	-0.024***	-0.011**	-0.0091***	-0.016***	-0.0032	-0.0076*	-0.0089	-0.0076*
	(-3.46)	(-2.87)	(-2.26)	(-4.41)	(-4.26)	(-1.58)	(-1.73)	(-1.14)	(-1.75)
D_{99} (1998=0, 1999=1)	0.040***	0.068***	0.012**	0.0078***	0.011**	0.0046	0.032***	0.057***	0.0077
	(6.53)	(6.25)	(2.05)	(2.59)	(2.10)	(1.58)	(5.88)	(5.81)	(1.45)
Mean of the dependent variable									
Comparison Group in 1998	*0.076*	*0.112*	*0.040*	*0.021*	*0.032*	*0.010*	*0.055*	*0.08*	*0.030*
Observations	14641	7223	7418	14641	7223	7418	14641	7223	7418
Adjusted R2	0.035	0.026	0.008	0.010	0.013	0.001	0.030	0.024	0.005
				Different ages and same cohorts					
Eligible*D_{99} (*DD*)	-0.025***	-0.042***	-0.0067	-0.0039*	-0.0056	-0.0021	-0.020**	-0.035**	-0.0037
	(-2.77)	(-2.66)	(-0.78)	(-1.86)	(-1.53)	(-0.97)	(-2.29)	(-2.33)	(-0.45)
Eligible	-0.0032	-0.0098	0.0021	-0.0010	-0.0022	0.0001	-0.0033	-0.0088	0.0011
	(-0.63)	(-1.04)	(0.46)	(-1.37)	(-1.41)	(0.0001)	(-0.66)	(-0.95)	(0.25)
D_{99} (1998=0, 1999=1)	0.036***	0.052***	0.020***	0.0050**	0.0067*	0.0032*	0.032***	0.047***	0.017***
	(5.28)	(4.22)	(3.21)	(2.53)	(1.92)	(1.73)	(4.83)	(3.94)	(2.82)
Mean of the dependent variable									
Comparison Group in 1998	*0.031*	*0.053*	*0.010*	*0.002*	*0.004*	*0.001*	*0.028*	*0.048*	*0.01*
Observations	7354	3597	3757	7354	3597	3757	7354	3597	3757
Adjusted R2	0.027	0.024	0.005	0.003	0.005	0.000	0.026	0.024	0.004

Note: Robust T-statistics in parentheses. *, **, *** Statistically significant at 10 percent 5 percent, and 1 percent respectively. The controls include age (in weeks), dummy variables for gender (male), ethnicity (white), head's years of schooling, age, and gender (=1 if male), dummy for states and metropolitan area.

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