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Work and tax evasion incentive effects of social insurance programs☆ Evidence from an employment-based benefit extension

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ABSTRACT

This article studies how social insurance programs shape individual's incentives to take up registered employment and to report earnings to the tax authorities. The analysis is based on a social insurance reform in Uruguay that extended healthcare coverage to the dependent children of registered private-sector workers. The identification strategy relies on a comparison between individuals with and without dependent children before and after the reform. The reform increased benefit-eligible registered employment by 1.6 percentage points (about 5% above the pre-reform level), mainly due to an increase in labor force participation rather than to movement from unregistered to registered employment. The shift was greater for parents with younger children and for cohabiting adults whose partners' jobs did not provide the couples' children with access to the benefit. Finally, the reform increased the incidence of underreporting of salaried earnings by about 4 percentage points (25% higher than the pre-reform level), mostly for workers employed at small firms. The increase in fiscal revenue from higher levels of registered employment was several orders of magnitude greater than the loss of revenue due to an increase in underreporting.

1. Introduction

A large body of research on labor and public economics has examined the effects of social insurance and social welfare programs on incentives in the labor market. Most of the empirical literature has focused on behavioral responses to social insurance benefits (Krueger and Meyer, 2002) and to in-work benefits (Blundell and MaCurdy, 1999; Moffitt, 2003; Eissa and Hoynes, 2006). In many studies, emphasis has been placed on the financial incentive effects of labor force participation. The effects of responses on additional margins such as registered and unregistered employment and on underreporting of earnings to evade payroll taxes has received less attention (Slemrod and Yitzhaki, 2002; Sandmo, 2012; Slemrod and Weber, 2012). Understanding what leads firms and their employees to register with the formal system and to declare their true income to the government is a first order public finance issue for most economies around the world. For developing and middle-income countries, this is an important issue because of widespread tax non-compliance, poor enforcement of regulations, and low levels of social insurance coverage. Higher levels of registered employment and more reporting of income subject to payroll taxes would result in increased access to social insurance for workers and their families. Moreover, a reduction in tax non-compliance could also help remedy a variety of market distortions that limit economic growth and economic development (La Porta and Shleifer, 2008; Levy, 2008). This issue is also relevant in advanced economies with smaller but still sizeable underground sectors (Schneider and Enste, 2000; Schneider, 2005).

This paper contributes to these discussions by exploring how social insurance programs shape individuals' incentives to take up registered employment and to alter the reporting of earnings to the tax authorities. This question is addressed empirically by examining a large-scale policy change in Uruguay's social insurance system. In this system, most

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benefits are made available only to employees registered with the social insurance administration who pay contributions and payroll taxes. Until 2007, a health insurance benefit was in place according to which the social insurance administration covered the employee's insurance premium from a private healthcare provider. However, the plan did not cover the employee's family. The 2008 reform extended the healthcare coverage funded by the Social Insurance Administration (SIA) to include all registered employees' dependent children under the age of 18. The reform also increased the payroll tax contribution deducted from employees' salaried earnings, although, as described in detail below, the market value of the extended benefit was substantially greater than the increase in the tax liability for most workers with children.

This health insurance benefit extension (HIBEX) reform modified several margins of adjustment for eligible individuals (i.e., adults with dependent children). The reform introduced incentives to move into benefit-eligible employment (i.e., 25 h or more per week at a registered job) to take advantage of the subsidy for dependent children's insurance premiums. The new benefit created an additional incentive for those not working to enter benefit-eligible employment, for those working in unregistered employment to formalize, and for registered workers below the 25-hour cut-off to increase their declared hours of work per week to at least 25.

The identification strategy relies on the fact that childless adults were not eligible for the benefit extension. The reform's impact can thus be estimated using a difference-in-difference approach, comparing the behavioral responses of adults with children to that of childless individuals before and after the reform. The analysis is based on a pool of repeated cross-sections of a nationally representative Uruguayan household survey that covers three years prior to and after the reform. The empirical analysis suggests that the benefit extension had a significant effect on labor market outcomes for eligible individuals. Specifically, the number of individuals in benefit-eligible registered employment increased by 1.6 percentage points with respect to the comparison group, about 5% higher than the pre-reform level. The evidence suggests that this aggregate effect was mostly the result of a reduction in the number of those not working rather than a consequence of increased work hours among registered employees below the threshold, or a movement of unregistered workers to registered employment. The effects were substantially larger for single parents, parents with several children, and parents with younger children, for whom the reform represented a stronger financial incentive. For single parents, a sizeable fraction of the effect can be attributed to movements from unregistered to registered employment. The response from individuals in couples depended on the benefit eligibility of their partners, with larger effects on individuals with a partner whose employment health insurance arrangements did not already offer coverage to the couple's children.

In addition to these responses in labor supply and employment registration, the reform could also have affected an additional margin of non-compliance, i.e., the underreporting of earnings to tax authorities to evade payroll tax and social insurance contributions. The HIBEX may have affected this form of non-compliance with the tax authorities because of the higher health insurance contribution rate (from 3 to 6%) and because those entering registered employment had to face the payroll taxes associated with the whole bundle of social insurance benefits (more than 30% of taxable earnings). The analysis below exploits a unique source of information on underreporting: a household survey in Uruguay asked registered employees whether their salaried earnings were being underreported to the tax authorities. Based on the plausibly exogenous variation introduced by the reform, the results presented below suggest that the extended benefit increased the incidence of underreporting of salaried earnings for individuals with children by about 4 percentage points, almost 25% higher than the pre-reform level. Moreover, this effect was confined to workers employed at small firms.

This paper contributes to the recent literature on labor supply, social insurance programs and tax compliance. First, it provides an analysis of individuals' reactions to social insurance policies in the labor market beyond the labor supply margin typically studied in the empirical literature (Ben-Shalom et al., 2011). The analysis below also covers the effect of social insurance benefits on the registered–unregistered employment margin of adjustment. This is related to recent studies on the effects of work-based benefits such as the Earned Income Tax Credit (EITC) on formal employment (Potter Gunter, 2013). Because the EITC subsidizes only declared work, it induces individuals with low incomes to shift more hours from informal (or unregistered) to registered employment. This paper makes an analogous argument: since the additional benefit is only available through registered employment, the reform introduces an additional incentive to take up jobs that comply with tax regulations.

Second, this paper provides evidence to show that individuals also respond to the policy change through an additional margin of non-compliance, mainly the underreporting of salaried earnings. While the literature on tax evasion has typically concentrated on underreporting of self-employment income, in Uruguay (and in many other developing and middle-income countries) only employers report workers' salaried earnings, and they pay the full amount of taxes due.[2] This analysis adds to the limited evidence currently available on underreporting and the incentives introduced by social insurance programs. The results presented here complement previous findings in at least two ways. On the one hand, rather than rely on indirect evidence such as the comparison between household income and expenditures (Tonin, 2011) or between income distributions from two separate sources (Kumler et al., 2012), the findings are based on direct evidence of underreporting as stated by salaried workers. Moreover, the analysis also breaks down the impact of the reform on underreporting by firm size. This confirms the results of recent theoretical models (Kleven et al., 2009) and reinforces empirical findings found for other countries, including the United States (Chetty et al., 2013). The direct evidence of adjustments to the policy reform in terms of off-the-book employment and underreporting of salaried earnings extends the range of evidence on tax evasion (Slemrod and Weber, 2012) and contributes to the literature that attempts to disentangle tax evasion from real behavioral responses to policies (Chetty, 2009; Saez et al., 2012).

Finally, this paper provides one of the first estimations of the labor market response to social insurance benefits in Latin America. Bosch and Manacorda (2012) survey a relatively large body of empirical literature on the impact of non-contributive programs (such as conditional cash-transfers) on labor supply. They highlight the scarcity of evidence for the region on the effects of social insurance programs linked to registered employment. The findings presented here are in line with the theoretical literature on the effects of benefits tied to registered employment on individual choices between formal and informal employment (Levy, 2008; Galiani and Weinschelbaum, 2012).

This paper is organized as follows. Section 2 describes the institutional background of Uruguay's social insurance system and the 2008 healthcare insurance benefit extension. Section 3 details the work and tax evasion incentive effects introduced by the reform. Section 4 describes the data and the identification strategy to estimate the effects of the reform. Section 5 presents the main results on labor supply, registered employment, and underreporting of salaried earnings. Section 6 discusses a series of robustness and specification checks. Section 7 presents an analysis of the fiscal incidence of the reform accounting for its behavioral effects. Section 8 concludes with a discussion of the implications of these results for the analysis of tax and benefit systems in developing and advanced economies.

[2] The empirical literature on underreporting of salaried earnings is scarce in part due to a lack of data. Moreover, this form of non-compliance is not deemed significant in developed countries because third-party information reporting is effective in inducing compliance, as suggested by the tax law literature (e.g. Shaw et al., 2010) and by empirical studies (e.g., Kleven et al., 2011).

2. Background: Related literature and Uruguay's social insurance system

2.1. Related literature

Uruguay's health insurance benefit extension (HIBEX) is one of a number of social insurance benefits linked to registered employment. The analysis of its effects on the labor market is related to a series of specific studies on benefits, work incentives and several margins of labor supply. With respect to labor force participation, the empirical literature generally finds a reduction in employment from increased social insurance benefits. Most of these studies, however, are related to social insurance benefits for the unemployed (Krueger and Meyer, 2002) or for other persons outside the labor force, including the disabled (Bound and Burkhauser, 1999). The analysis below covers these types of labor supply responses and expands the range of outcomes to include the impact of social insurance benefits on registered (i.e., on-the-book) employment and on misreporting of salaried earnings to the tax authorities.

The benefit–employment linkage also relates this reform to the literature on in-work welfare benefits such as the United States' Earned Income Tax Credits and the Temporary Assistance for Needy Families (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001) and the United Kingdom's Working Families Tax Credits (Francesconi and van der Klaauw, 2007). Studies of these programs typically find positive and significant effects on the extensive margin of labor supply, but the evidence is less conclusive on the intensive margin of response. An exception is the study by Chetty et al. (2013), who explain positive responses to the EITC in the intensive margin by means of an identification strategy that analyzes differences in knowledge about the tax code across neighborhoods. As in the analysis presented in this paper, most of these studies rely on comparisons between individuals with children and childless adults who are not affected by the policies and, hence, used as comparison groups.

This paper is also connected to the literature on the impact of extensions of welfare systems in developing countries, particularly in Latin America (Bosch and Manacorda, 2012). While most of these studies cover non-contributive programs, such as conditional social cash-transfer programs, this study investigates an expansion of a contributive social insurance benefit and its implications for labor supply incentives.

Finally, this study forms part of a growing body of research performed at the intersection between the literature on tax evasion-shadow employment and the literature on labor supply-tax and benefits in developing and developed countries. This type of analysis is based on Sandmo's (1981) and Cowell's (1985) extensions of the Allingham and Sandmo (1972) and Yitzhaki (1974) canonical models of tax evasion. In the context of Latin American labor markets, Levy (2008) and Galiani and Weinschelbaum (2012) model informality (the registered–non-registered employment margin) in terms of tax evasion decisions as a function of fines and probabilities of detection. In a recent study for the United States, Potter Gunter (2013) relies on a quasi-experimental approach based on benefit variation by state to study the effect of the EITC on participation in regular (registered) and informal (unregistered or off-the-book) work among individuals with low incomes. The results indicate that higher levels of benefits in some states induce an increase in registered work and a fall in unregistered hours, with no significant effect on the extensive margin or on total hours worked.

The main outcome of interest in Potter Gunter's study (2013) relates to registered and unregistered self-employment. The analysis below studies unregistered salaried employment. Its innovation lies in examining another dimension of tax evasion, the underreporting of salaried earnings. The literature on tax evasion has typically focused on underreporting of earnings from self-employment because the withholding and reporting requirements of wage and salaried earnings make underreporting difficult (Slemrod and Weber, 2012) and quantitatively small, particularly in the United States (Slemrod, 2007). Most theoretical models also assume no evasion on wage and salary income (Feldman and Slemrod, 2007). Paying a fraction (but not all) of earnings under the table is a potentially relevant margin of adjustment. Kleven et al. (2009) develop a theoretical model where workers and firms may collude to underreport earnings to the tax authority. The model predicts that this type of collusion may be feasible at small firms, but harder to sustain on a larger scale. Tonin (2011) builds a theoretical model where firms and workers collude to hide part or all of the employee's salaried earnings to the fiscal authority "to avoid the payment of taxes and social security contributions." The results in this paper are consistent with these types of models. They are also directly related to recent attempts to empirically test their predictions by analyzing the impact of changes in taxes and benefits on misreporting of social insurance contributions. Tonin (2011) presents supporting evidence based on household income and consumption patterns in Hungary following a hike in the minimum wage in 2001. Consistent with the model, the hike induced higher compliance and reduced disposable income for skilled workers.

Underreporting of salaried earnings, however, might also be decided solely by the firm rather than pursuant to collusion, especially in developing countries where there are no third-party reporting requirements. This lack of information on reporting[3] implies that the underreporting of salaried earnings may be a quantitatively relevant issue (see Kleven et al., 2011 for a discussion of third-party information reporting). Kumler et al. (2012) study underreporting of earnings in Mexico by examining a pension reform that transitioned from a pay-as-you-go system to a system based on fully funded individual accounts. The reform gave employees an additional incentive to ensure accurate reports of earnings by firms and to monitor their employers' compliance with social insurance contributions. A comparison of income distributions from administrative data and household surveys reveals substantial underreporting of earnings by firms with lower levels of underreporting for larger firms. Moreover, a difference in differences analysis comparing younger cohorts (which benefit more from the reform) to older workers indicates a significant decline in underreporting after the reform.

This paper complements these studies of underreporting in at least two ways. Instead of relying on comparisons between household income and expenditures or between income distributions from two separate sources, the analysis below relies on direct evidence of underreporting of earnings as stated by workers. Moreover, the analysis demonstrates the impact of the reform on underreporting of salaried earnings, breaking down the effect by firm size, as suggested in the recent literature (Chetty et al., 2013; Kleven et al., 2009).

2.2. Uruguay's social insurance system

Uruguay has one of the oldest and most developed social insurance systems in Latin America. The following is a review of only the aspects of that system that are most relevant to this paper; for a more in-depth description, see Ferreira-Coimbra and Forteza (2004). This system follows a European Bismark-type model, where access to most social insurance programs is linked to registered employment and financed through payroll taxes and contributions from registered employees. The *Banco de Previsión Social* (BPS) is the institution in charge of most of its administration (henceforth, the social insurance administration — SIA). It collects payroll taxes and contributions and provides social insurance benefits to most private-sector employees and public-sector workers. These benefits include retirement pension plans and a wide range of other programs, such as health insurance

[3] Strictly speaking, this would be a lack of first-party reporting. The tax authority only has employers' reports for most employees' earnings. The result is the same: the tax authority does not have two sources of information to compare.

(paying premiums to private providers), sickness benefits, unemployment insurance, disability benefits, maternity leave and family allowances (cash transfers paid for each of the worker's dependent children).

Private-sector employers in Uruguay are required to register their employees with the SIA. Both the employer and employee are required to make contributions to finance social insurance programs. Formally, the employer deducts the employee's contributions from her salary and pays both the employee's and the employer's contributions to the SIA. Table 1, Panel B, shows the breakdown of the SIA contributions required by law as a percentage of the taxable salary for private-sector employers and employees. Although the total consists of contributions to different programs, it is transferred in full by the employer to the

Table 1
Summary statistics (Encuesta Continua de Hogares, ECH) 2004–2010 and payroll tax contributions for social insurance benefits.

	Childless individuals				Parents with children < 18				Difference in differences
	Pre-reform		Post-reform		Pre-reform		Post-reform		
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.	
Panel A. Descriptive statistics									
Sample									
N (total = 97,552)	13,402		9,756		45,973		28,421		
Share	0.14		0.10		0.47		0.29		
Individual characteristics									
Age	39.66	9.93	39.62	9.89	38.49	7.62	38.56	7.58	0.117
High school or less	0.72	0.45	0.72	0.45	0.85	0.35	0.86	0.35	0.002
Some college or more	0.28	0.45	0.28	0.45	0.15	0.35	0.14	0.35	−0.002
Male	0.48	0.50	0.49	0.50	0.39	0.49	0.40	0.49	0.005
Head of household	0.63	0.48	0.65	0.48	0.50	0.50	0.53	0.50	0.012
Married	0.65	0.48	0.63	0.48	0.89	0.32	0.87	0.33	0.013*
Number of children aged 0–10	0.00	0.00	0.00	0.00	1.12	0.99	1.06	0.95	−0.061***
Number of children aged 11–17	0.00	0.00	0.00	0.00	0.80	0.83	0.81	0.85	0.013*
Number of children > 18	0.00	0.00	0.00	0.00	0.28	0.60	0.26	0.57	−0.016***
Total number of children	0.00	0.00	0.00	0.00	2.20	1.13	2.14	1.10	−0.064***
Region									
Montevideo	0.71	0.46	0.69	0.46	0.59	0.49	0.60	0.49	0.024***
North	0.06	0.24	0.06	0.24	0.10	0.30	0.09	0.29	−0.007*
Center–North	0.07	0.25	0.08	0.27	0.10	0.30	0.10	0.30	−0.007
Center–South	0.05	0.23	0.06	0.23	0.07	0.26	0.08	0.26	−0.002
South	0.11	0.32	0.11	0.32	0.13	0.34	0.13	0.33	−0.007
Firm size									
<10 employees	0.41	0.49	0.34	0.47	0.44	0.50	0.38	0.49	0.014
10–49 employees	0.22	0.42	0.24	0.43	0.22	0.42	0.23	0.42	−0.009
>49 employees	0.36	0.48	0.43	0.49	0.34	0.47	0.39	0.49	−0.005
Sector									
Agriculture	0.04	0.20	0.04	0.20	0.06	0.24	0.06	0.23	0.001
Industry	0.10	0.30	0.09	0.28	0.11	0.32	0.11	0.31	0.005
Manufacturing	0.07	0.26	0.08	0.28	0.08	0.26	0.07	0.26	−0.010**
Construction	0.06	0.23	0.06	0.25	0.07	0.26	0.09	0.28	0.005
Trade	0.23	0.42	0.23	0.42	0.20	0.40	0.22	0.41	0.016**
Transport/communications	0.07	0.26	0.09	0.28	0.08	0.27	0.08	0.27	−0.012**
Finance/professional	0.11	0.32	0.12	0.32	0.08	0.26	0.07	0.26	−0.009
Education/health	0.19	0.40	0.18	0.39	0.17	0.38	0.17	0.37	0.005
Personal services	0.12	0.32	0.10	0.31	0.15	0.36	0.14	0.34	−0.001
Labor market and tax evasion outcomes									
Benefit-eligible employment	0.58	0.49	0.67	0.47	0.46	0.50	0.57	0.50	0.016*
Not working	0.21	0.41	0.16	0.36	0.33	0.47	0.25	0.43	−0.020***
Unregistered employment	0.16	0.36	0.12	0.32	0.16	0.37	0.13	0.34	0.005
Registered employment	0.63	0.48	0.72	0.45	0.51	0.50	0.62	0.49	0.014*
Benefit-eligible empl.|reg. empl.	0.92	0.26	0.92	0.26	0.91	0.29	0.92	0.28	0.007
Underreporting earnings to SIA	0.10	0.30	0.09	0.28	0.10	0.30	0.10	0.30	0.01
Panel B. Payroll tax contributions to the Social Insurance Administration									
Health insurance benefit	8		9.5		8		11		
Employee	3		4.5		3		6		
Employer	5		5		5		5		
Other social insurance benefits	22.75		22.75		22.75		22.75		
Employee	15.125		15.125		15.125		15.125		
Employer	7.625		7.625		7.625		7.625		
Total	30.75		32.25		30.75		33.75		

Notes: SD denotes the standard deviation. "Pre-reform" refers to the period from the 2nd semester of 2004 to the 2nd semester 2007. "Post-reform" denotes the period from the 1st semester of 2009 to the 2nd semester of 2010. "Difference in differences" refers to the impact coefficient estimated by running a regression as in Eq. (1) (without controls) on each variable in Panel A with Huber–White robust standard errors. "Parents with children < 18" includes all adults in the relevant age range with at least one child under the age of 18 who may or may not have older children. The sample includes individuals aged 25–55 and contemplates the variables of benefit-eligible employment, not working, unregistered and registered employment (97,552 observations); only those working in registered employment are included for benefit-eligible employment conditional on registered employment (56,707 observations). For underreporting of salaried earnings, the sample includes salaried private-sector registered employees aged 25–55 from 2006 onwards (50,669 observations). Means are weighted by ECH sampling weights. The payroll tax contributions correspond to December 2007 ("Pre-reform") and 2008 ("Post-reform"). The "Health insurance benefit" includes contributions for both sickness benefit and health insurance coverage. The change in contributions corresponds to registered employees with salaried earnings higher than 2.5 BPC (threshold level for benefits and contributions — UYU 1775 in 2008). Payroll tax contributions for registered workers with salaried earnings below 2.5 BPC remained unchanged after the reform. "Other social insurance benefits" pension contributions and contributions to the "Fondo de Reconversión Laboral" (labor reallocation fund).

* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

SIA. In many senses, then, it operates like an overall payroll tax (the paper uses contribution and payroll tax interchangeably). This payroll tax amounted to about 31% of taxable wages in 2007, the year before the reform analyzed in this paper. The social insurance benefits are provided as an indivisible bundle, that is, workers or employers cannot opt out of certain programs and pocket the correspondent contribution, although there are minimum work requirements for some benefits as discussed below. Therefore registered employment – that is, employment compliant with social insurance regulations and payroll taxes – grants the worker access to the full bundle of benefits.

As in many middle-income countries, enforcement in Uruguay is relatively weak and there is widespread non-compliance with social insurance regulations and evasion of the payroll tax. This means that a substantial fraction of private-sector salaried employees lack access to employment-related social insurance benefits. Unregistered or off-the-book workers in Uruguay constituted about a quarter of total salaried employees in the decade of 2000 (Gasparini and Tornarolli, 2009) and about 28% of private-sector employees in 2007, as computed with microdata from the *Encuesta Continua de Hogares* described below. This phenomenon is sometimes referred to as labor informality. This paper uses a more precise terminology based on the situation of the worker with respect to the SIA. The discussion refers to an employee's registration status or, alternatively, to the employer and employee's compliance with payroll taxes and social insurance regulations.

2.3. The 2008 reform: The health insurance benefit extension (HIBEX)

Eligibility for the health insurance component of the social insurance system requires workers to be registered with the SIA and to work 25 or more hours per week.[4] Qualifying employees have the right to select a health insurance provider in the private sector, which are structured like HMOs in the United States. The insurance premium is paid by the SIA. This health insurance package covers services for the worker that are similar to those offered in the private healthcare market, usually by the same institutions. These packages include inpatient hospital coverage, emergency room care, medical and surgical services, and maternity care, among other services. By 2007, the health insurance benefit covered 43% of the labor force and approximately 66% of private-sector salaried employees (figures based on the *Encuesta Continua de Hogares*). That year, the SIA spent about UYU 7.4 billion (USD 471 million at the 2010 PPP adjusted exchange rate) on health insurance premiums, which represented 13% of its budget and 1.4% of the Uruguay's gross domestic product (Banco de Previsión Social, 2008). In addition, the health insurance benefit for employees represented about 16% of total healthcare expenditures in Uruguay in 2007 (Ministerio de Salud Pública, 2010a).

In 2007, Uruguay's government embarked on an ambitious overhaul of the healthcare system, with the stated objective of strengthening three areas: healthcare coverage; health management; and healthcare financing. The reform enacted by Uruguay's Parliament (Law 18,211-2007) established a series of new guidelines, regulations and institutions for the healthcare system in order to further equitable access.

Before the 2008 reform, the health insurance benefit was financed by contributions to the SIA amounting to 8% of taxable salaries; 5% represented the employer's contribution and 3% the employee's (Table 1, Panel B). The health insurance benefit, however, covered only registered employees. Individuals not in the labor force, unregistered employees, workers' spouses who were not themselves registered employees, and the dependent children of registered employees had to pay for their own health care, purchase private health insurance, or use the public healthcare system, which is subject to a means test.

The most important provision of the reform was the extension of SIA health insurance benefits to cover the beneficiary's dependent spouses and children.[5] Because of fiscal constraints, this extension was implemented in stages. For the period covered in this paper, the expansion only reached the dependent children of registered employees.[6] The reform was passed in December 2007 and took effect on January 1, 2008. Eligibility for the health insurance benefit extension required that the registered employee have at least one dependent child under the age of 18.[7] The inclusion of these children under the extended benefit was universal, irrespective of pre-existing health conditions and previous health insurance arrangements. As with the previous individual-based benefits, the worker had to be registered with the SIA (compliant with payroll taxes and social insurance regulations), and work at least 25 h per week or earn above a minimum threshold. While the reform covered all salaried workers, the HIBEX was most relevant for private-sector registered employees, since a large fraction of public-sector workers were already entitled to health insurance coverage for their dependents (Ministerio de Salud Pública, 2010b).

HIBEX beneficiaries could choose a health insurance provider in the private sector or rely on the public healthcare system for their children. Regardless of the option chosen, the SIA reimbursed each child's premium to the corresponding institutions. Registered employees, however, opted overwhelmingly for the private sector, with only about 5% of all children enrolled in the extended benefit using the public system in 2008 (Banco de Previsión Social, 2009).

The HIBEX represented a large-scale policy change. It had a substantial impact on the health insurance coverage of its target population and substantially increased privately provided insurance for children under the age of 18. Fig. 1 depicts the trend in the percentage of dependent children of private-sector employees aged 0–17 (i.e., eligible for HIBEX) who used private-sector healthcare providers for the period 2004–2010; from 2008 onwards, it depicts the trend in the proportion of those who accessed private-sector health care through the HIBEX. The figure also plots the same series for individuals aged 18 and older — i.e., those not covered by the benefit extension.[8] In the period from 2004 to 2007, the evolution of private healthcare coverage for both groups was similar. Following the 2008 reform, private healthcare coverage for individuals aged 0–17 increased by 30 percentage points, and it continued to grow (albeit slightly) until 2010. This change was driven by the HIBEX reform, as indicated by the series depicting the percentage of children 0–17 who accessed private health care through the new program. In contrast, there was no significant change in the trend in private healthcare coverage for individuals aged 18 or older. By 2008, 450,000 children under 18 years old were insured through the HIBEX, or about 48% of all children of this age group in Uruguay (Banco de Previsión Social, 2009) out of a total population of about 3.3 million individuals (Uruguayan National Bureau of Statistics, projection for 2008).

[4] Very low-income workers are also excluded from the health insurance benefit. The threshold is defined as those earning below 1.25 times the *Base de Prestaciones y Contribuciones* (BPC). Created in 2004 (Law 17,856), the BPC index replaced the minimum wage as the indexing basis for all government benefits. The baseline was the value of the minimum wage in force on December 20, 2004. It was UYU 1636 in 2007, or about USD 103 per month at the 2010 PPP adjusted exchange rate.

[5] Borgia (2008) and Bergolo and Cruces (2013) provide more details about the healthcare system reform, including the benefit expansion discussed here. Cruces and Bergolo (2013) describe the series of recent reforms to Uruguay's social insurance system, including health insurance, family allowances and pensions.

[6] The extended benefit also reached children under the age of 18 with retired parents eligible for a SIA old-age pension after the reform. It covered as well the dependent children of those retired before the reform but who belonged to the low-income group of the old-age pension system. By 2008, children in this group represented fewer than 1% of all children enrolled (Banco de Previsión Social, 2009). This small number of individuals is excluded from the analysis.

[7] For the purpose of the SIA benefit, a dependent child is defined as an offspring, a step-child or a foster child of the registered employee under the age of 18. Disabled children above this age threshold also qualify for the HIBEX. The reform also established that dependent children enrolled in the HIBEX could retain coverage until the age of 21 if their parents reimbursed the value of the child's insurance premium to the SIA.

[8] Computed with microdata from *the Encuesta Continua de Hogares*, described below. Similar trends are found in administrative records from the Ministry for Public Health.

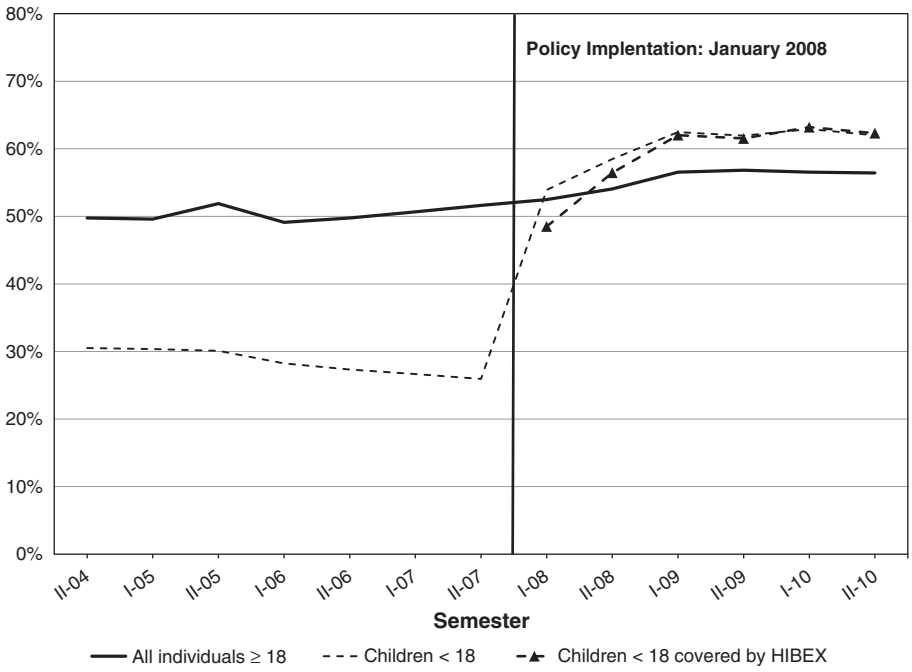

Fig. 1. Healthcare coverage in the private market before and after the reform (2004–2010). Note: The sample corresponds to the Encuesta Continua de Hogares from the 2nd semester of 2004 to the 2nd semester of 2010.

To finance this extension of coverage, the legislation specified an increase in payroll contributions for most workers, with a crucial distinction between parents and childless workers. Employer contributions remained unchanged by the reform at 5% of taxable earnings as one of the reform's stated intentions was to leave firms' employment costs unaffected. The employee's contribution to the health insurance benefit, however, increased from 3 to 6% of taxable earnings for registered private-sector workers with dependent children under the age of 18, regardless of the number of qualifying children. The reform also increased the contribution for childless registered employees and for those whose children did not qualify (those over the age of 18) from 3 to 4.5% of their taxable earnings. The payroll tax remained unchanged at the 3% level for low-wage workers, defined as those with monthly earnings below 2.5 times the value of the BPC (UYU 1775 per month in 2008, USD 112), irrespective of whether they had children who qualified for the HIBEX or not. The reform thus increased the overall payroll tax rate from 30.75 to 32.25% for most childless registered workers, and from 30.75 to 33.75% for most registered workers with young dependent children, with no change in contributions for very low-wage workers. Table 1, Panel B, summarizes the changes in the contributions and the total payroll taxes introduced by the HIBEX reform.

The reform implied a potential net benefit for most adults with eligible children. The market value of the extended benefit exceeded the increase in the tax liability for most parents in registered employment. For instance, the monthly health insurance premium for a child in the private market in 2007 was more than two times the 3% increase in the contribution of the average monthly taxable earnings of a registered private-sector worker.[9] Moreover, the average number of eligible children per adult in the sample in 2007 was 1.92 (Table 1). However, this comparison must be qualified. For high earners with few children, the cost of the increase in the payroll tax may have ended up being more than the new benefit, although only a small number of workers at the top of the income distribution faced this potential net loss.[10] A further qualification stems from the fact that childless workers faced an increase in the cost of registered employment (a total of 1.5% of salaried earnings) without any additional benefit. This may have yielded a reduction in the level of employment for this group of workers. However, the available empirical evidence for Latin America indicates that even sweeping changes in labor taxes had negligible effects on employment (Gruber, 1997; Cruces et al., 2010). An increase of 1.5 percentage points in the social insurance contributions of childless registered workers can, then, be expected to have had a negligible effect. In any case, after the reform, only about 8% of children benefiting from the HIBEX used the public healthcare sector rather than private health insurance providers (Fig. 1). The very high take up of the HIBEX suggests that it represented a net benefit for a majority of those eligible. Thus, the empirical analysis in this paper is based on a comparison of labor market outcomes for adults with and without eligible children. Section 4.2 below addresses some of the caveats with this comparison, the identification strategy and some robustness tests to validate it.

3. Work and tax evasion incentives' effects of the health insurance benefit extension

The health insurance benefit extension represents an increase in the benefits associated with registered employment for individuals who work 25 h or more per week (the minimum hour cut-off for eligibility) and have children under the age of 18. This policy change yields a number of predictions on labor supply and tax evasion responses.[11] Given these new incentives, the main expected effect of the reform would be an increase in the probability of moving into benefit-eligible employment (i.e., working 25 h or more per week in registered employment) for parents of children in the 0–17 age range. There are three

[9] The average monthly taxable earnings for a private-sector registered employee in 2007 was UYU 12,305 (USD 778 per month at the 2010 PPP adjusted exchange rate), and thus the 3 percentage point increase in the contribution rate represented UYU 369 (USD 23). In contrast, the health insurance premium for a child in the private market in 2007 was on average UYU 859 (USD 54) per month (Ministerio de Salud Pública, 2010a).

[10] In 2007, the reform implied a net loss in income only for those registered workers with one child and taxable earnings above UYU 28,633, and for those with two children and earnings above UYU 57,267. These values correspond to the 94th and 99th percentiles of the distribution of monthly labor earnings for registered workers in 2007.

[11] The structure of the discussion in this section was suggested by an anonymous referee.

possible channels through which this change could operate. First, the reform introduced an incentive to work in registered employment with 25 or more hours per week for those not working. Second, those in unregistered employment faced a similar incentive: either to move into a benefit-eligible job or to formalize their existing employment arrangement (i.e., getting "on"-the-books with 25 declared hours at their current job). Third, the reform provided an incentive for those in registered employment working less than 25 h per week to increase their declared hours of work to at least 25.

The main expected effect yields the prediction that (1) the number of individuals with children in registered employment working 25 h or more per week should increase after the reform. The different channels through which this main effect operates imply some of the following additional predictions: (2) the number of those not working should decrease, (3) the number of individuals with unregistered employment should fall, (4) the number of individuals in registered employment (irrespective of hours worked) should increase, and more specifically (5) conditioned on registered employment, the number of those working 25 h or more per week should increase.[12]

While the discussion has thus far referred to overall effects, the benefits derived from the reform (and thus the behavioral reactions to the new incentives) may have varied in magnitude for different subgroups defined by the demographic characteristics of an individual's household. First, since the increase in the payroll tax was independent of the number of children, the reform implied a larger financial incentive for individuals with more children and for those with younger children (since the benefit covers children up to the age of 18, implying a longer potential duration of the benefit).

Second, the rules of the benefit extension imply that the HIBEX may have induced a differential response for single parents compared to parents living with their partners for at least two reasons.[13] On the one hand, the rules of the reform implied a potential double taxation for couples, since if the two parents were registered employees, both had to pay the increase in the payroll tax (if both worked more than 25 h a week) for the same level of benefit. Thus, individuals with partners in benefit-eligible employment did not face an additional incentive to move to benefit-eligible employment themselves after the reform, since the couple's children would be covered through their partners (in fact, they faced an incentive to reduce participation in benefit-eligible employment). In turn, individuals whose partners were not in benefit-eligible employment faced an additional incentive to take up (or remain in) benefit-eligible employment themselves to warrant coverage for the couple's children. On the other hand, the HIBEX represented an unambiguous incentive for single parents, in contrast to married individuals, to take up benefit-eligible employment, since they would not face any double taxation or have to take another individual's labor market outcomes into account when making their own decisions.

Finally, changes in tax rates can also affect the incentives for reporting earnings to the tax authorities. Given the increased costs associated with registered employment in the form of higher payroll taxes, the reform may also have affected the intensive margin of tax evasion by inducing more underreporting of salaried earnings. This additional incentive may affect both the firm (which faces higher payroll tax liabilities) and the worker (who faces lower after-tax wages). It may result in higher rates of underreporting on the part of the firm or pursuant to collusion of employers and employees. In any case, the literature reviewed above indicates that underreporting is more likely at small firms. An additional prediction on the effects of the reform is thus that (6) the number of registered employees with eligible children stating that their earnings were underreported should increase, especially among those working at small firms.

4. Data and empirical strategy

4.1. Sample and outcomes of interest

The empirical analysis in this paper is based on cross-sectional microdata from the *Encuesta Continua de Hogares* (ECH) survey carried out by Uruguay's National Institute of Statistics (*Instituto Nacional de Estadística*, INE) since 1990. The ECH is a nationally representative household survey conducted according to international standards. It combines elements of living standards and labor force surveys, and it is the main source for socioeconomic, labor and demographic indicators in Uruguay. The microdata and supporting documents, such as questionnaires and details on sample selection and stratification, are all publicly available.

The analysis in this paper relies on two waves of the survey corresponding to the first and second semesters of the calendar year. These repeated cross-sections cover the period from the 2nd semester of 2004 to the 2nd semester of 2010, that is, three and a half years before the reform and three years after it. The sample for the analysis includes men and women aged 25 through 55 who are either heads of households or spouses of heads of households. The sample excludes individuals who reside in rural areas or in urban areas with less than 5000 inhabitants, since the ECH only extended its coverage to such areas in 2006.[14] The analysis is carried out on a sample of private-sector salaried employees (excluding the self-employed), as well as the unemployed and individuals outside the labor force in the relevant age range. The sample does not include government employees because a large proportion was entitled to health insurance for their dependents before the reform. The sample also excludes retired and disabled individuals and those who were studying full time during the survey's reference period. Taking all of these restrictions into account and pooling all the survey waves yield a sample of 97,552 individuals for the 2004–2010 period.

The ECH collects information on all the relevant outcomes discussed in the previous section: employment and labor force participation, hours of work, the SIA registration status of the employee, salaried earnings and whether there is underreporting of such earnings. The registration status of an employee is derived from a specific self-reported variable that indicates whether contributions (payroll taxes) are paid to the SIA for her current job.[15] The ECH questionnaire also contains a unique feature for detecting the presence of underreporting of earnings for registered salaried workers. Since 2006, registered private-sector employees have been asked directly whether their salaried earnings are being underreported to the tax and social security authorities. The specific question is: "Are your contributions based on the total amount of your earnings from this job?" ("¿Aporta por la totalidad del salario en esa ocupación?"). The explicit aim of the ques-

[12] These are all quantitatively relevant margins of adjustment. In 2007, before the reform, about 54% of individuals with children in the sample were not eligible for the extended benefit. Those not working represented 33% of all individuals with children, 16% were unregistered employees, and about 5% were registered employees working below the 25 weekly hour threshold (see Table 1, Panel A).

[13] The benefit extension covered the children of legally married and cohabiting couples. The text sometimes refers to individuals in both types of arrangements as married for the sake of brevity.

[14] While this implies a loss of information, there is a trade-off between a more representative sample and a longer pre-reform period with a comparable sample. The latter option was chosen since more than 80% of the population of Uruguay resides in urban areas with more than 5000 inhabitants.

[15] The specific question in the ECH is: "Are you contributing to a retirement benefit through this job?" ("¿Aporta a una caja de jubilaciones por este trabajo?"). This is a standardized question in household surveys in Latin America; it is used to define registered or formal work in most of the recent literature for the region — see, for instance, Gasparini and Tornarolli (2009) and Galiani and Weinschelbaum (2012).

tion is to establish if tax evasion is taking place, as stated in the ECH enumerator's manual which specifies that this question attempts to "capture underreporting of salaried earnings for the computation of social security contributions".[16] Finally, all of the estimates presented below are computed using the corresponding ECH sampling weights.[17]

4.2. Identification assumptions, econometric strategy and outcomes of interest

The empirical work in this paper attempts to identify the causal effect of the introduction of the 2008 health reform on labor market outcomes and tax evasion and to provide evidence on the direction and magnitude of the incentive effects discussed in the previous section. The strategy used to evaluate the policy's effects is a difference-in-differences analysis (Angrist and Krueger, 1999). This framework compares the evolution of the outcomes of interest for a group of individuals exposed to a policy change (the treatment group) to the change in the same outcomes for individuals unaffected by the reform (the comparison group). The latter group is assumed to capture the counterfactual trend for the treatment group which would have been observed in the absence of the policy change.

The treatment group consists of individuals aged 25 to 55 with at least one dependent child under the age of 18, — that is, those potentially eligible for the HIBEX. The comparison group consists of childless individuals in the same age range who were not directly affected by the benefit extension.[18] The empirical analysis therefore compares adults with at least one dependent child under the age of 18 to individuals in the same age group with no dependent children. The following is the basic difference-in-difference specification with controls on which most of the estimates in this paper are based:

$$Y_{ijt} = \alpha + \delta AnyChildren_{ijt} + \beta AnyChildren_{ijt} * PostReform_t + X'_{ijt}\gamma + \theta_t + \varphi_j + \tau_{jt} + \epsilon_{ijt} \quad (1)$$

where i indexes individuals, j state and t time. The variable Y_{ijt} is one of the outcomes of interest (described in more detail below); $AnyChildren_{ijt}$ is an indicator variable for individuals in the treatment group, coded as one if the individual has at least one child under the age of 18 and zero otherwise; $PostReform_t$ is a dummy equal to one in the post-policy period (from January 2008 onwards) and zero otherwise; and $AnyChildren_{ijt} * PostReform_t$ is the interaction term between the two variables, which captures the difference-in-difference treatment effect. The X_{it} matrix contains individual-specific variables to condition the differences in trends to observable characteristics. The individual controls are age (in 6 categories), gender, head of the household indicator, marital status, education level (in 6 categories), the number children in the household, the number of children under the age of 6, and the number of children and age and education interactions (both in 6 categories). The estimations also include controls for firm size and industry (5 and 9 categories, respectively) for the outcomes which are defined for the labor force. θ_t and φ_j are a full set of semester and state fixed effects (Uruguay is administratively divided into nineteen states or "departamentos"), which account for aggregate systematic shocks to local labor markets correlated with, but not caused by, the HIBEX. Finally, τ_{jt} are state-by-semester effects, introduced to control for state-specific shocks over the period that might be independent of national economic conditions.

The analysis is based on six outcomes of interest directly related to the expected effects discussed in Section 3. The first is benefit-eligible employment, which is an indicator coded as one for salaried employees who report 25 h or more per week at a registered job and zero otherwise. This minimum hour cut-off is determined by the eligibility requirement for health insurance coverage. The second outcome of interest is registered employment, which is an indicator variable coded as one for registered salaried employees and zero otherwise (irrespective of the number of hours worked). The third outcome is unregistered employment, an indicator coded as one for off-the-book salaried employees and zero otherwise. The fourth variable of interest is benefit-eligible employment defined only for registered employees, which is coded as one for registered employment at or above 25 h per week and zero for those in registered employment working 24 h or less. A fifth outcome of interest is an indicator variable equal to one for those not working and zero for those working (either as registered or unregistered employees). These variables are defined for all individuals in the sample for the whole 2004–2010 period, with the exception of conditional benefit-eligible employment which is defined only for the subsample of registered employees (56,707 observations). Finally, a sixth outcome of interest, underreporting of salaried earnings, is captured by a measure coded as one if the individual reports her salaried earnings as underreported to the SIA and zero otherwise. The analysis is based on the subsample of registered employees for the years 2006–2010 (50,669 observations).

The estimations rely on linear probability models (OLS) for the binary dependent variables and all the regression results report Huber–White robust standard errors. The main results are based on the 2004–2010 period excluding both semesters of 2008, since the policy change was effectively implemented throughout that year.[19]

The difference-in-differences identification strategy in this setting requires two main assumptions. First, in the absence of the reform, the underlying trends in labor market outcomes (conditional on observable characteristics X) would have been similar for the treatment and comparison groups. This assumption would be violated if there were unobservable shocks that affected the trends in the outcomes of interest for both groups differently — shocks correlated with the policy change and with the group-specific trends in outcomes. Section 5 discusses the results from an extended version of the model of Eq. (1) which includes controls for time trends interacted with the treatment group indicator. Section 6 also presents some additional specification checks to explore this possibility. An additional concern would arise if, besides the health insurance benefit extension, some other contemporaneous changes affected individuals with and without children. The introduction of HIBEX was accompanied and preceded by the implementation of a series of new programs and reforms in other existing schemes as part of an active government policy to expand the safety net in Uruguay,[20] but the main results of this paper are not affected by the introduction of controls for participation in other programs.[21]

The second identification assumption is that the composition of each group remained constant over the period under study.[22] This

[16] This question refers specifically to underreporting, and it does not reflect a cap on contributions, which are voluntary for earnings above a relatively high threshold (monthly earnings above UYU 74,000, about USD 3800). This is confirmed by the fact that individuals self-report as underreporting earnings along the whole income distribution. For instance, for the sample before the reform, 15.1% of individuals in the bottom quintile of the distribution of monthly income declared underreporting of salaried earnings, with 11.6, 10.9, 9.9 and 6.0% in the following quintiles.

[17] The main results do not vary when the regressions are estimated without the ECH sampling weights.

[18] The empirical analysis below also relies on an alternative comparison between individuals with and without children within the group of low-earning registered employees (those earning below 2.5 BPC). These individuals benefited from the extended coverage if they had children, but did not face the increase in SIA contributions (from 3 to 6% for those with children and from 3 to 4.5% for the childless). While this constitutes a potentially viable comparison group, the resulting sample size is small and the results are underpowered. These results are presented as part of the robustness tests in Table 7 below.

[19] The main results are qualitatively similar when observations for 2008 are included (Table 3).

[20] For a detailed description of those programs and reforms, see Cruces and Bergolo (2013), Amarante et al. (2011) and Manacorda et al. (2011).

[21] A subsample of those in the treatment group (low-income individuals with children) participated in a means tested transfer program for families with at least one dependent child ("asignaciones familiares"). As a disincentive to work, this would result in an attenuation bias for the estimates of the effects of the HIBEX. Specification checks (available upon request) indicate that this benefit did not affect the main HIBEX estimates.

[22] Abadie (2005) and Athey and Imbens (2006) discuss the implications of compositional changes in the difference-in-differences framework.

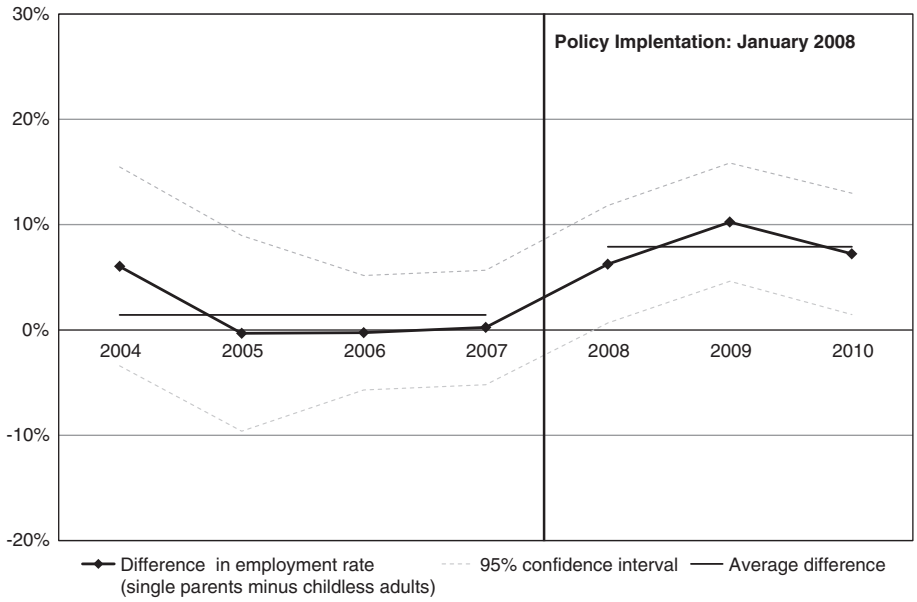

Fig. 2. Difference in benefit-eligible employment rates (registered and working 25 h or more per week) by year with demographic controls. Single parents with children under the age of 18 and childless adults. Note: The sample contains individuals aged 25 to 55 and corresponds to the Encuesta Continua de Hogares (ECH) from the 2nd semester of 2004 to the 2nd semester of 2010. The set of controls includes the individual's age, gender, head of household status, education level, number of children, head of household status and marital status.

assumption may be incorrect, for instance, if the treatment group expands over time and incorporates individuals with different characteristics. Although Eq. (1) includes controls for a broad set of individual characteristics, this may not be enough to control for potential differences in group-specific compositional changes over time. Section 6 discusses in more detail these concerns and presents some sensitivity checks to test the validity of the identification strategy.

5. Empirical results

5.1. Descriptive analysis

Table 1 presents summary statistics by treatment status before and after the policy change (from the second semester of 2004 to the second semester of 2007 and from the first semester of 2009 to the second semester of 2010, respectively) for individuals in the sample with and without eligible children. The statistics in Panel A indicate that individuals in the two groups were reasonably similar in terms of age, education and other main socioeconomic characteristics. Most importantly, the last column in the table presents the difference between the pre- and post-reform changes for the two groups (effectively, an unconditional difference-in-differences analysis), which indicates only a few statistically significant changes in the average characteristics of the two groups. This evidence suggests that, at least in terms of observed characteristics, the main results discussed below are not driven by compositional effects.

The bottom rows of Panel A in Table 1 present similar descriptive statistics for the main labor market outcomes defined in Section 4.2. The average level of benefit-eligible employment before and after the reform was about 58 and 67% respectively for the comparison group and about 46 and 57% respectively for the treatment group. These differences in levels between the two groups are also reflected by the level of the not-working indicator, ranging from 21 to 16% for the comparison group and from 33 to 25% for the treatment group. On the contrary, the levels of benefit-eligible employment and of underreporting of earnings (both conditional on registered employment) were similar between the two groups for the two periods: about 91–92% of registered workers were eligible for the HIBEX and about 10% stated that their earnings were being underreported.

Fig. 2, in turn, illustrates the essence of the basic results for the outcomes of interest presented in Table 1. The figure plots the conditional difference (with basic demographic controls) in the annual predicted benefit-eligible employment rates between 2004 and 2010 for the comparison group (childless adults) and for a subset of the treatment group, single parents with children under 18 years.[23] Following the severe economic crisis that affected Uruguay in 2003–2004, the level of benefit-eligible employment increased substantially over the whole sample period for all groups. The difference in this rate between childless adults and single parents was relatively small (about 1.4 percentage points on average) and stable for the pre-reform period 2004–2007. After the reform, benefit-eligible employment grew markedly faster for single parents, which is reflected in the upward trend in the series and in the substantially larger average difference of 7.9 percentage point for 2008–2010. This simple evidence is further reinforced by the unconditional difference-in-differences estimates of the reform's effect presented in Panel A of Table 1 for the overall treatment group, which indicate an increase in benefit-eligible registered employment of about 1.6 percentage points, as well as a fall in the proportion of those not working of about 2 percentage points, with no significant differences for underreporting of earnings or for the conditional benefit-eligible variable. This preliminary evidence on the impact of the HIBEX is further developed below in terms of the full conditional difference-in-differences framework given by Eq. (1).

5.2. Baseline results: Labor supply and registered employment

Table 2 presents the baseline estimates of the effects of the 2008 health insurance benefit extension on the labor market outcomes of interest discussed in Section 3. Each column reports the OLS estimates

[23] The underlying specification for Fig. 2 is analogous to that of Table 5, with a limited set of demographic controls and yearly treatment dummies. The sample is the same as in Table 5, panel A. This subgroup is better suited for a visual presentation of the main results because, as described below, the effects of the policy for single parents are substantially larger than for the overall treatment group.

Table 2
Effect of the health insurance benefit extension on labor market outcomes. Difference in differences estimates.

	(1)	(2)	(3)	(4)	(5)
	Benefit-eligible employment	Not working	Unregistered employment	Registered employment	Benefit-eligible employment | reg. empl.
AnyChildren * *PostReform*	0.0162**	−0.0162**	0.001	0.0152**	0.0107**
	[0.0075]	[0.0063]	[0.0058]	[0.0073]	[0.0052]
AnyChildren	0.0239***	−0.0292***	−0.0052	0.0344***	−0.0132***
	[0.0066]	[0.0059]	[0.0055]	[0.0066]	[0.0048]
Observations	97,552	97,552	97,552	97,552	56,707
R^2	0.26	0.24	0.05	0.25	0.17
Dependent variable mean	0.46	0.33	0.16	0.51	0.91

Notes: The sample includes individuals aged 25–55 (all individuals for columns 1–4, conditional on registered employment for column 5). The data corresponds to the Encuesta Continua de Hogares (ECH) from the 2nd semester of 2004 to the 2nd semester 2007 and from the 1st semester of 2009 to the 2nd semester of 2010. "Benefit-eligible employment" refers to registered employment with 25 h or more per week. The "*AnyChildren*" variable equals 1 if the individual has at least one child under the age of 18 and 0 otherwise. The "Post-reform" variable equals 1 for the period 2009–2010 and 0 otherwise. The estimated coefficients correspond to the regression in Eq. (1). The regressions are estimated as linear probability models. The controls include the number of children; the number of children aged 0–5; dummy variables for: age (6 categories), gender, head of the household status, marital status, educational level (6 categories), region of residence (19 categories), firm size (5 categories), industry (9 categories) (the final two are only included in the benefit-eligible registered employment conditional on registered employment regression), and semester (11 categories). The controls also include interactions between age and education groups and number of children, and time and region of residence. Regressions are weighted by ECH sampling weights. The reported means for the dependent variables correspond to the pre-reform period (2004–2007).
Huber–White robust standard errors in brackets.
** Significant at 5%.
*** Significant at 1%.

of Eq. (1) for the main outcomes: benefit-eligible employment, not working, registered and unregistered employment, and benefit-employment conditional on registered work. The first row in Table 2 presents the estimates and standard errors of the interaction coefficient β in Eq. (1), which captures the impact of the HIBEX. The second row displays the estimates of the coefficient of the treatment variable (*AnyChildren* in Eq. (1)). The last row in Table 2 reports the average of each column's dependent variable for the period before the implementation of the HIBEX (2004–2007). The discussion focuses on the specifications with the full set of controls in Eq. (1); the estimates with no controls are similar in size but less precisely estimated (see Table 1, Panel A).

The estimates in column 1 of Table 2 correspond to the main outcome of interest, that is, benefit-eligible employment. The result indicates a statically significant increase in registered employment above the 25 weekly-hour cut-off for parents of young children relative to childless individuals of 1.62 percentage points. In terms of the pre-intervention average, this effect represents an increase of 3.5%. While small, this effect is still economically significant when compared to the trend in this variable over the period under study.

Columns 2 to 5 present the results from the additional predictions corresponding to the alternative channels of response outlined in Section 3. The negative and significant coefficient of −1.62 for the *not working* indicator in column 2 indicates that the HIBEX induced unemployed individuals and/or those previously out of the labor force to join the labor market (a 4.9% increase relative to the pre-reform average). The reform could also have increased benefit-eligible employment through the formalization of unregistered employees. The positive and significant coefficient of 1.52 for registered employment in column 3 (very close to those in columns 1 and 2) and the virtually null and insignificant coefficient for the effect of the HIBEX on unregistered employment (column 4) indicate that the increase in benefit-eligible employment was mainly channeled through an increase in registered employment for those not previously working, rather than an increase in the registration of those previously in unregistered employment. The results indicate that the HIBEX did not produce, on average, a decline in unregistered employment. The distinction between single and married individuals below, however, indicates some additional effects for those facing greater changes in incentives after the reform.

Finally, the results in column 5 indicate that part of the overall effect on benefit-eligible employment was due to an increase in the hours of work above the 25 weekly-hour cut-off point for those who were registered employees.[24] This is consistent with the discussion in Section 3, since working right below the eligibility threshold becomes even less beneficial. While statistically significant at the 5% level, the 1.1 percentage point effect of the HIBEX for this variable is relatively small: it only implies a 1.2% increase relative to the pre-reform average, since about 90% of registered workers were above the cut-off before the policy change.

The discussion in Section 3 also implies a series of predictions about empirical results that would be incompatible with the postulated incentive effects of the HIBEX. For instance, there should be no positive effect of the reform on registered work below the hour-eligibility threshold. This placebo test can be implemented by defining the outcome as an indicator equal to one for those registered employees working less than 25 h per week and zero for all other individuals. A regression similar to those in Table 2 yields a coefficient for the effect of the reform on this outcome of −0.001, with a standard error of 0.0037. Moreover, a further placebo test implies that the HIBEX should not have an impact on unregistered hours of work above or below 25 weekly hours — a significant difference-in-differences in this variable between the comparison and treatment groups would indicate some underlying group-specific trend and would cast doubt on the identification strategy. Conditioning the previous estimate on unregistered workers, the coefficient of the reform variable is −0.0097 and statistically insignificant at standard levels (standard error of 0.0204).

The estimates presented in Table 2 would be biased if the treatment and comparison groups had different underlying trends in labor market outcomes. Inspection of Fig. 2 did not reveal major differences in the pre-reform trends for the benefit-eligible employment variable, but this can be tested formally by means of a flexible version of the basic difference-in-differences model. Table 3 presents the results from estimating Eq. (1) for the full 2004–2010 period. The table reports

[24] A detailed analysis of hours of work for different groups indicates a substantial shift among working parents from 24 weekly hours or less to more than 25 weekly hours. Since only about 10% of registered workers with eligible children worked less than 25 h and about 7% worked 25 to 34 h during the pre-reform period, the sample size is too small to conduct an analysis of bunching around this threshold.

Table 3
Effect of the health insurance benefit extension on labor market outcomes. Dynamic specification of the difference in differences model.

	(1)	(2)	(3)	(4)	(5)
	Benefit-eligible employment	Not working	Unregistered employment	Registered employment	Benefit-eligible employment|reg. empl.
AnyChildren ∗ *Year dummies*					
2010	0.0249**	−0.0222**	0.0053	0.017	0.0158**
	[0.0114]	[0.0095]	[0.0084]	[0.0110]	[0.0077]
2009	0.0260**	−0.0064	−0.0135	0.0199*	0.0106
	[0.0114]	[0.0094]	[0.0084]	[0.0110]	[0.0076]
2007	0.0012	0.0009	0.0024	−0.0033	0.0053
	[0.0110]	[0.0091]	[0.0083]	[0.0107]	[0.0075]
2006	0.0087	−0.0061	−0.0014	0.0075	−0.0047
	[0.0111]	[0.0094]	[0.0083]	[0.0109]	[0.0076]
2005	0.0173	0.0065	−0.0083	0.0018	0.0154
	[0.0147]	[0.0128]	[0.0122]	[0.0145]	[0.0109]
2004	0.0211	0.0222	−0.0339**	0.0118	0.0039
	[0.0194]	[0.0168]	[0.0166]	[0.0192]	[0.0139]
Observations	117,001	117,001	117,001	117,001	68,515

Notes: The sample includes individuals aged 25–55 (all individuals for columns 1–4, conditional on registered employment for column 5). The data corresponds to the Encuesta Continua de Hogares (ECH) from the 2nd semester of 2004 to the 2nd semester of 2010. Unlike all the other tables in the paper, the estimates presented here include the two semesters of 2008, the year of the reform. The omitted year category is 2008. "Benefit-eligible employment" refers to registered employment with 25 h or more per week. The "*AnyChildren*" variable equals 1 if the individual has at least one child under the age of 18 years and 0 otherwise. The "Post-reform" variable equals 1 for the period 2009–2010 and 0 otherwise. The estimated coefficients correspond to the regression in Eq. (1). The regressions are estimated as linear probability models. The controls include the number of children; the number of children aged 0–5; dummy variables for: age (6 categories), gender, head of the household status, marital status, education (6 categories), region of residence (19 categories), firm size (5 categories), industry (9 categories) (the final two are only included in the benefit-eligible registered employment conditional on registered employment regression), and semester (11 categories). The controls also include interactions between age and education groups and number of children, time and region of residence, and *AnyChildren* and year dummies. Regressions are weighted by ECH sampling weights. The reported means for the dependent variables correspond to the pre-reform period (2004–2007).
Huber–White robust standard errors in brackets.
* Significant at 10%.
** Significant at 5%.

that the coefficients of the year dummies interacted with the eligible children indicators.[25] The omitted period is 2008 (the year of the policy implementation), so all coefficients are measured relative to that year. Each row of the table reports the coefficients for the year-eligibility interaction for each of the dependent variables, which include the five outcomes in Table 2.

The results in Table 3 confirm the robustness of the results in Table 2 and reinforce the soundness of the identification strategy. First, only one of the 20 pre-reform year coefficients for the 5 regressions is statistically significant at the standard levels (unregistered employment in 2004), which confirms the lack of differential trends between the treatment and comparison groups in the pre-reform period. Second, the coefficient for benefit-eligible employment is large (2.49–2.6 percentage points) and statistically significant for the two years after the implementation of the policy, while that of registered employment is also relatively large (1.7–1.99 percentage points), but only significant at standard levels for 2009 (although the size of the coefficient is similar for 2010 with a p-value of about 15%, which suggests lack of statistical power rather than lack of effect). Third, the effect on the not-working indicator and the benefit-eligible employment conditional on registered work are only statistically significant for 2010. The lack of evidence of pre-existing differential trends and the lack of effects on the placebo outcomes support the causal interpretation of the main results in Table 2. Section 6 presents additional evidence on their robustness.

Taken together, the pattern of results in Table 2 indicates that eligible individuals responded to the health insurance benefit extension as predicted in Section 3. The bulk of the response to the additional benefit appears to have occurred due to an increase in participation rather than due to changes in hours of registered workers or shifts from off-the-book to on-the-book employment. The following section breaks the results down by subgroups facing differential incentives.

[25] The year dummies are also included independently, as described in Eq. (1). The estimates in Table 3, and other tables, are based on annual rather than semester dummies because the latter are too imprecisely estimated.

5.3. Differential incentives: Estimates by age and number of children

As discussed in Sections 2.3 and 3, the reform's financial incentives varied as a function of the age and number of an individual's dependent children. The higher cost associated with the extended benefit represented a fixed percentage of taxable earnings irrespective of the number of children who gained access to health insurance (the SIA covered the premiums of all dependent children), and thus implied a larger financial incentive for employees with more children. Moreover, younger children are associated with higher insurance and out-of-pocket health expenditures and with a longer duration of the benefit. Younger children should increase the implicit value of the HIBEX for their parents.

To explore the potential heterogeneity introduced by these varying financial incentives, the basic model in Eq. (1) is extended to allow for different treatment effects according to the number of dependent children and the age range of those children. The resulting specification is:

$$Y_{ijt} = \alpha + \sum_k \delta^{(k)} AnyChildren_{ijt}^{(k)} + \sum_k \beta^{(k)} AnyChildren_{ijt}^{(k)} * PostReform_t + X'_{ijt}\gamma + \theta_t + \varphi_j + \tau_{jt} + \epsilon_{ijt} \quad (2)$$

where $AnyChildren_{ijt}^{(k)}$ is an indicator equal to one if individual i in state j at time t has children in group k, where k represents one of the following four mutually exclusive groups: one child aged 0–10; one child aged 11–17; two or more children where the youngest is aged 0–10; and two or more children where the youngest is aged 11–18. The matrix X_{ijt} includes the same covariates as Eq. (1), and the additional terms represent the same set of state and time controls. The results from regressions based on Eq. (2) are presented in Table 4. The rows in this table report the estimates of the HIBEX's impact for each number of children/age range group, $\beta^{(k)}$.

Column 1 in Table 4 indicates an increase of about 2.5 percentage points in benefit-eligible employment for parents with one child aged 0–10, and of 1.8 percentage points for parents with two or more children where the youngest is aged 0–10. The coefficients for the indicators of one child and two children aged 11–17, however, are not

Table 4
Effect of the health insurance benefit extension on labor market outcomes by age and number of children. Difference in differences estimates.

	(1)	(2)	(3)	(4)	(5)
	Benefit-eligible employment	Not working	Unregistered employment	Registered employment	Benefit-eligible employment | reg. empl.
*Child group dummies * PostReform*					
One child only, aged 0–10	0.0246**	−0.0126	−0.0119	0.0245**	0.0066
	[0.0100]	[0.0085]	[0.0075]	[0.0098]	[0.0069]
One child only, aged 11–17	0.0084	−0.012	0.0026	0.0095	0.0113
	[0.0104]	[0.0092]	[0.0081]	[0.0103]	[0.0078]
Two children or more, youngest aged 0–10	0.0183**	−0.0184**	0.0031	0.0153*	0.0116*
	[0.0083]	[0.0073]	[0.0066]	[0.0082]	[0.0060]
Two children or more, youngest aged 11–17	0.0023	−0.0212*	0.0174	0.0038	0.0123
	[0.0133]	[0.0119]	[0.0110]	[0.0132]	[0.0103]
Observations	97,552	97,552	97,552	97,552	56,707
Dependent variable mean	0.46	0.33	0.16	0.51	0.91

Notes: The sample includes individuals aged 25–55 (all individuals for columns 1–4, conditional on registered employment for column 5). The data corresponds to the Encuesta Continua de Hogares (ECH) from the 2nd semester of 2004 to the 2nd semester 2007 and from the 1st semester of 2009 to the 2nd semester of 2010. "Benefit-eligible employment" refers to registered employment with 25 h or more per week. The dependent variables and controls are the same as those detailed in the notes to Table 2. The regressions are estimated as linear probability models. The rows report the coefficients estimated for each number–by–age range of children group from regressions as in Eq. (2). Regressions are weighted by ECH sampling weights. The reported means for the dependent variables correspond to the pre-reform period (2004–2007).
Huber–White robust standard errors in brackets.
* Significant at 10%.
** Significant at 5%.

significantly different from zero. In keeping with the literature, the greater effects in the case of parents of young children are in line with the evidence on larger employment responses to financial incentives for this group, particularly among single women (see Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001, for the EITC, and Francesconi and van der Klaauw, 2007, for the WTFC).

The pattern of results for the other labor market outcomes in Table 4 is similar: the effects are mainly driven by the indicators for having at least one child in the 0–10 age group (for registered employment, for instance), while the negative effect on the not-working indicator is mainly driven by the dummies corresponding to those with more than one child (irrespective of the age of the youngest child). Together, these results confirm the prediction of a larger reaction to the HIBEX among those with more children and among those with a longer horizon of benefits for their children, as suggested in the discussion in Section 3.

5.4. Differential incentives: Estimates by marital status

The HIBEX also introduced differential incentives for different household configurations. The incentives were stronger for single parents since, by definition, the coverage of their dependent children did not depend on the employment status of a partner and since they did not face any potential double taxation. The incentives faced by married partners, in turn, varied according to their partner's employment and benefit-eligibility status.

The results in Table 5 broadly confirm these expected differential reactions to the reform. Panel A presents the results for the same five labor market outcomes in previous tables conditioning the sample on single parents (about 17,000 individuals from a total of more than 97,000 in the whole sample). The pattern of results is similar to the baseline estimates of Table 2, but with much larger and more significant effects, even with a substantially smaller number of observations. With respect to single childless individuals, single parents increased their level of benefit-eligible employment by 5.3 percentage points (significant at the one percent level), which represents a 14% increase with respect to the pre-reform average. This effect is about four times greater than the effect reported for the entire sample as shown in Table 2. The impact of the reform reduced the proportion of the not-working indicator by about 2.1 percentage points (column 2, significant at the 10% level) and, as such, it was similar to the effect for the entire sample. However, the estimated impact on unregistered employment (column 3) for single parents is negative and highly significant statistically, indicating a decline of about 4 percentage points in this labor market outcome indicator with respect to the comparison group. The impact of the reform on this indicator for the entire sample was virtually zero. The reductions in the number of single parents not working and of those working in unregistered employment are reflected in the sizeable (6.2%) increase in the number of those in registered employment (column 4, significant at the one percent level). The small and not statistically significant coefficient in column 5 (benefit-eligible employment conditional on being a registered employee) indicates that the increase in the number of single parents in benefit-eligible employment (column 1) was mainly attributable to those switching from unregistered to registered employment, with a smaller contribution from those entering the labor force and no discernible impact from registered employees increasing their hours to meet the benefit cut-off. Together, the results suggest that the positive response in the level of benefit-eligible employment for single parents after the policy change was driven by transitions from unregistered to on-the-book employment for those who were working with a smaller contribution from those entering the labor force. The smaller baseline effects for the entire sample, on the other hand, corresponded mostly to the latter factor.

Panel B in Table 5 presents estimates of the same models for the sample of married and cohabiting individuals. The estimates are substantially smaller than those for singles and in line, albeit less significant, than those for the entire sample (Table 2). Only the coefficient of the variable in column 5 (benefit-eligible employment conditional on being a registered employee) is statistically different from zero (and only at the 10% level). However, further testing indicates that this effect cannot be distinguished from the effect of the reform on the same variable for singles (p-value of 0.49).

The fact that the HIBEX had no impact on the sample of married parents, however, does not mean that there were no responses at the household level. Since the extended health coverage of a couple's children only required one of the parents to hold benefit-eligible employment, an individual's response should depend on the employment status of her partner. These household-level responses can be tested by means of an extended version of the model given by Eq. (1) which includes additional indicators on whether or not the fact that an individual's partner holds a benefit-eligible job interacted with the variable capturing the HIBEX's effect (*AnyChildren * PostReform*). It

Table 5
Effect of the health insurance benefit extension on labor market outcomes of individuals by marital status and partner's employment status. Difference in differences estimates.

	(1)	(2)	(3)	(4)	(5)
	Benefit-eligible employment	Not working	Unregistered employment	Registered employment	Benefit-eligible employment | reg. empl.
Panel A. Singles					
*AnyChildren * PostReform*	0.0530***	−0.0207*	−0.0417***	0.0624***	0.0017
	[0.0158]	[0.0124]	[0.0137]	[0.0153]	[0.0127]
Observations	17,340	17,340	17,340	17,340	10,387
Dependent variable mean	0.38	0.27	0.29	0.44	0.86
Panel B. Individuals with partners					
*AnyChildren * PostReform*	0.0102	−0.0112	0.0047	0.0064	0.0114*
	[0.0088]	[0.0078]	[0.0065]	[0.0086]	[0.0062]
Observations	80,212	80,212	80,212	80,212	46,320
Dependent variable mean	0.47	0.33	0.15	0.52	0.92
P-value singles = couples	0.0177**	0.5173	0.0020***	0.0014***	0.4991
Panel C. By partner's employment status					
Reform's effect on those with ineligible partners	0.0168*	−0.0082	−0.0035	0.0117	0.0125*
	[0.0093]	[0.0082]	[0.0070]	[0.0091]	[0.0065]
Reform's effect on those with eligible partners	−0.0012	−0.0155*	0.0182**	−0.0027	0.0090
	[0.0099]	[0.009]	[0.0071]	[0.0097]	[0.0071]
Observations	80,212	80,212	80,212	80,212	46,320
Dependent variable mean	0.47	0.33	0.15	0.52	0.92

Notes: The sample includes individuals aged 25–55 (all individuals for columns 1–4, conditional on registered employment for column 5). The data corresponds to the Encuesta Continua de Hogares (ECH) from the 2nd semester of 2004 to the 2nd semester of 2007 and from the 1st semester of 2009 to the 2nd semester of 2010. "Benefit-eligible employment" refers to registered employment with 25 h or more per week. The dependent variables and controls are the same as those detailed in the notes to Table 2. The regressions are estimated as linear probability models. The specification is similar to that of Eq. (1) with two exceptions: it includes an indicator for the employment status of the individual's partner and its interaction in order to capture the reform's impact (*AnyChildren * PostReform*). Panels A and B report estimates from a regression as Eq. (1) for single individuals in the sample and for partnered individuals, respectively. Panel C reports the estimates of the HIBEX's impact on partnered individuals conditioning on their partner's employment status, that is, holding a benefit-eligible registered job ("those with eligible partners") or not ("those with ineligible partners"). Regressions are weighted by ECH sampling weights. The reported means for the dependent variables correspond to the pre-reform period (2004–2007).
Huber–White robust standard errors in brackets.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

should be stressed that the identification of these effects arises from the assumption that the eligibility status of the spouse is exogenously given.

The estimates of these coefficients presented in Panel C in Table 5 distinguish between the two cases, labeled "Eligible partners" and "Ineligible partners." Two sets of results emerge from the estimates presented in this panel. For individuals whose partners were not in benefit-eligible employment, the HIBEX induces a statistically significant increase in this type of employment of about 1.7 percentage points (significant at the 10% level). This change seems to be driven mostly by an increase in registered employment, although this coefficient is not significant (column 4, coefficient 1.2). To a lesser extent, the overall change is also due to a small increase (with respect to the 0.92 pre-reform level) in benefit-eligible employment for those in registered employment (column 5). On the other hand, and as expected, the HIBEX had virtually zero effect on the benefit-eligible employment (column 1) in the case of individuals whose partners received access to the extended benefit through employment. These individuals did not have the additional incentive of gaining access to the extended benefit. In fact, the reform seems to have induced a statistically significant increase in unregistered employment for this group of about 1.8 percentage points (column 4, significant at the five percent level). However, the source of this effect seems to have been a reduction in the number of those not working (column 2, coefficient of −1.5 percentage points, significant at the 10% level) rather than through a transition from registered to unregistered employment (column 4, coefficient of 0.27 percentage points, not statistically significant), which would have been more consistent with the mechanisms highlighted in Section 3.

To sum up, the response patterns for married and cohabiting individuals are in line with the predicted effects of the reform. They are also consistent with the existing evidence on the labor supply responses to financial incentives for households (Eissa and Hoynes, 2004), and they extend this evidence to an additional margin of choice between registered and unregistered employment. These results also confirm the relevance of social insurance benefits to the observed patterns of informal employment for heads of households and their partners in Latin American countries (Galiani and Weinschelbaum, 2012).

5.5. Underreporting of salaried earnings

The results indicate that workers reacted to the reform along the registered–unregistered employment margin, with more individuals working at jobs complying with payroll taxes and contributions after the reform. This can be interpreted as an extensive margin of tax evasion — i.e., a decision of the employer and/or the employee about whether or not to evade the full amount of employment-related taxes. As the discussion in Section 3 highlights, however, changes in taxes and benefits could also have an impact on the intensive margin of evasion of employment-related taxes. Employers and/or employees might decide to pay taxes for less than the full amount due, and this type of evasion would take the form of underreporting of earnings.

The HIBEX could affect this form of non-compliance with the tax authorities and the social insurance administration in several ways. The previous results indicate that a significant number of individuals moved to benefit-eligible registered employment following the reform. To access the extended health insurance coverage for their children, these workers and their employers had to incur the full cost of payroll taxes and contributions, which amounted to more than 30% of taxable earnings, since the additional benefit could not be granted independently of other social insurance benefits. Moreover, for those who already held benefit-eligible registered employment, the higher contribution rate implied an incentive to increase underreporting to avoid a reduction in net earnings for workers and to avoid higher total payroll tax payments for employers.

Either as the result of collusion between employers and employees or pursuant to employers' unilateral decisions to evade taxes, the reform

Table 6
Effect of the health insurance benefit extension on underreporting of salaried earnings, overall and by firm size. Difference in differences estimates.

	(1)	(2)	(3)	(4)
	All registered employees	Small firms (<10 employees)	Medium firms (10–49 employees)	Large firms (50+ employees)
Panel A. All registered employees				
*AnyChildren * PostReform*	0.0073	0.0342**	−0.0052	−0.0031
	[0.0061]	[0.0152]	[0.0131]	[0.0060]
AnyChildren	−0.0083	−0.0213	−0.0034	−0.0033
	[0.0059]	[0.0138]	[0.0125]	[0.0060]
Observations	50,669	15,151	13,035	22,483
R^2	0.06	0.04	0.03	0.02
Dependent variable mean	0.10	0.18	0.11	0.04
Panel B. Registered employees working > 25 h				
*AnyChildren * PostReform*	0.0093	0.0409**	−0.0012	−0.0049
	[0.0064]	[0.0166]	[0.0136]	[0.0061]
AnyChildren	−0.0105*	−0.0262*	−0.0042	−0.004
	[0.0062]	[0.0151]	[0.0130]	[0.0062]
Observations	46,222	12,681	12,175	21,366
R^2	0.06	0.04	0.04	0.02
Dependent variable mean	0.10	0.17	0.11	0.04
Panel C. Registered employees working < 25 h				
*AnyChildren * PostReform*	−0.0128	−0.0051	−0.0799	0.0471
	[0.0224]	[0.0380]	[0.0498]	[0.0287]
AnyChildren	0.0183	0.0143	0.0266	0.0202
	[0.0218]	[0.0346]	[0.0512]	[0.0307]
Observations	4,447	2,470	860	1,117
R^2	0.14	0.15	0.15	0.24
Dependent variable mean	0.15	0.22	0.09	0.03

Notes: The sample includes employed individuals aged 25–55. The data corresponds to the Encuesta Continua de Hogares (ECH) from the 1st semester of 2006 to the 2nd semester 2007 and from the 1st semester of 2009 to the 2nd semester of 2010. The dependent variable is the underreporting of salaried earnings as reported by the individual herself. The controls are the same as those detailed in the notes to Table 2. The regressions are estimated as linear probability models. Regressions are weighted by ECH sampling weights. The reported means for the dependent variables correspond to the pre-reform period (2004–2007).
Huber–White robust standard errors in brackets.
* Significant at 10%.
** Significant at 5%.

may have increased underreporting of salaried earnings for registered employment. As discussed in Section 4.1, the analysis of this margin of adjustment to the reform relies on workers' responses to a specific question in the survey, one that enquires about whether the contributions to the SIA for earnings from a registered job were based on the full amount of taxable earnings. Table 6, column 1, presents the difference-in-differences estimates of the HIBEX's impact on underreporting of salaried earnings to the SIA for the subsample of registered employees. As expected, the sign of the coefficient is positive, but it is not significantly different from zero. However, as discussed in Section 3, there may be heterogeneous effects on underreporting as a function of firm size. As highlighted in recent studies, tax evasion by employers and employees is more difficult for large firms to sustain, whereas it is easier for small firms to misrepresent salaried earnings (Kleven et al., 2009). The average level of underreporting of 10% in Table 1 varies significantly in the sample under study: in 2007, 18.5% of workers at firms with less than 10 employees declared that their earnings were underreported, but this figure is only 11.2% for those employed at firms with 10 to 49 workers and only 4.1% at firms with more than 50 employees.[26] These potential heterogeneous effects are addressed by re-estimating the same regression, but conditioning it on firm size. These results are presented in columns 2 to 4 in Table 6. Consistent with the previous discussion, the HIBEX has a positive and significant effect on underreporting only for employees at firms with less than 10 workers. With respect to those in the comparison group, the reform implied a 3.4 percentage point increase in underreporting of salaried earnings after the reform, which represents a sizeable increase (about 19%) in the rate of underreporting. On the other hand, the estimated effects of the reform on workers at firms with 10 to 49 workers (column 3) and on workers at firms with over 50 employees (column 4) are substantially smaller in absolute terms (−0.52 and −0.31 percentage points, respectively); in neither case are they significantly different from zero.

Finally, the reform should only increase underreporting in benefit-eligible registered employment and not in ineligible-registered employment (for those working less than the 25 weekly-hour cut-off). As an additional robustness test of these results, and to rule out the impact of other contemporaneous changes, Panels B and C in Table 6 re-estimate the same models for all workers and by firm size, but restrict the sample to those in benefit-eligible registered employment (Panel B) and to those in benefit-ineligible registered employment (Panel C) — i.e., distinguishing between those working more and less than 25 h per week. The only significant coefficient in the two panels is that of the effect of the reform on workers in benefit-eligible employment at small firms. In that case, the coefficient (4.1 percentage points, about 25% more than the pre-reform level) is similar in size to the one reported in Panel A, whereas the additional placebo test in Panel C indicates that the reform had no discernible effect (coefficient of 0.5 percentage points) on benefit-ineligible workers at small firms. The patterns of significance of the coefficients reported in these two panels confirm the previous findings: the HIBEX's impact on underreporting was confined to workers at small firms.

In sum, these results are consistent with recent empirical literature that finds evidence that changes in tax and benefit systems affect the (unilateral or collusive) decisions of firms and employees to misreport

[26] Evasion by means of unregistered employment follows a similar pattern. In 2007, the pre-reform year, 41.1% of employees at firms with less than 10 workers were not registered with the SIA, whereas only 9.5% of employees at medium-sized firms (10 to 49 employees) were not registered, and only 3.3% at firms with more than 50 workers.

Table 7
Effect of the health insurance benefit extension on labor market outcomes. Robustness and specification checks.

	(1)	(2)	(3)	(4)	(5)	(6)
	Benefit-eligible employment	Not working	Unregistered employment	Registered employment	Benefit-eligible employment|reg. empl.	Underreporting of earnings (small firms)
Panel A. With time-cohort effects						
*AnyChildren * PostReform*	0.0144*	−0.0175***	0.0022	0.0153**	0.0085	0.0342**
	[0.0077]	[0.0065]	[0.0060]	[0.0075]	[0.0054]	[0.0152]
Observations	97,552	97,552	97,552	97,552	56,707	15,151
Panel B. With AnyChildren/covariates interactions						
*AnyChildren * PostReform*	0.0132*	−0.0122*	−0.0004	0.0126*	0.0111**	0.0332**
	[0.0075]	[0.0063]	[0.0058]	[0.0073]	[0.0053]	[0.0153]
Observations	97,552	97,552	97,552	97,552	56,707	15,151
Panel C. With PostReform/covariates interactions						
*AnyChildren * PostReform*	0.0173**	−0.0165**	−0.0017	0.0182**	0.0072	0.0398**
	[0.0081]	[0.0070]	[0.0063]	[0.0079]	[0.0059]	[0.0165]
Observations	97,552	97,552	97,552	97,552	56,707	15,151
Panel D. Individuals with unchanged payroll tax						
*AnyChildren * PostReform*	0.0168*	−0.0118	−0.0039	0.0157	0.0107	0.0915**
	[0.0100]	[0.0143]	[0.0119]	[0.0116]	[0.0272]	[0.0403]
Observations	40,803	40,803	40,803	40,803	7,023	3,127

Notes: The sample includes individuals aged 25–55 (all individuals for columns 1–4, conditional on registered employment for columns 5 and 6). The data corresponds to the Encuesta Continua de Hogares (ECH) for the 2nd semester of 2004 to the 2nd semester of 2007 and from the 1st semester of 2009 to the 2nd semester of 2010. For the regressions in Panel A, the data corresponds to the period that spans from the 2nd semester of 2004 to the 2nd semester of 2007. The data for the regressions in column 6 starts in the 1st semester of 2006. "Benefit-eligible employment" refers to registered employment with 25 h or more per week. The dependent variables and controls are the same as those detailed in the notes to Table 2. The regressions are estimated as linear probability models. Panel A reports results with a full set of birth cohort-semester interactions. Panel B reports results with the *AnyChildren* indicator interacted with demographic controls. Panel C reports the results from a specification with the PostReform indicator interacted with demographic controls. Panel D reports results by restricting the sample to individuals with salaried (nominal) earnings below 2.5 BPC (the regression sample includes those with zero earnings). The reported means for the dependent variables correspond to the pre-reform period (2004–2007).
Huber–White robust standard errors in brackets.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

social insurance contributions and payroll taxes (Tonin, 2011; Kumler et al., 2012). They are also in line with the prediction that this behavior is feasible at small firms but less likely on larger scales (Chetty et al., 2013; Kumler et al., 2012).

6. Robustness and specification tests

The following pages present a series of robustness and specification tests on the difference-in-differences estimates presented in the previous section. These exercises are based on variations in the model of Eq. (1), with full controls for individual characteristics, semester and state-fixed effects, and state-by-semester interactions effects, as in the previous analysis.

The interpretation of the difference-in-differences estimates of the HIBEX's impact as a causal effect relies on a series of assumptions. The main identifying assumption is that the changes in outcomes would have been similar for the treatment and comparison group in the absence of the reform. While these counterfactuals are inherently non-testable, it is possible to evaluate auxiliary hypotheses consistent with the identifying assumption. One option is to consider the pre-reform period and to establish whether there were common trends in the outcomes of interest, such that the reform represented a departure from the previous parallel changes.[27] The estimates on the year-by-year and treatment interaction terms presented in Table 3 provide a first test of the empirical strategy and establish that the difference-in-differences estimates were not simply capturing long-run differential trends between individuals with and without children.

A further concern for the identification strategy is that the treatment and comparison groups may have changed over the period under study, confounding treatment with composition effects. The summary statistics and the unconditional difference-in-differences estimates in Table 1 indicate that the main demographic characteristics of both groups did not change substantially before and after the policy change, but these cross-sectional differences in means might still conceal distributional variation across groups. For instance, since the age distribution of individuals with and without eligible children differs, the trends in their labor supply may have differed by cohort. Panel A in Table 7 presents the results of a specification that adds a full set of birth-cohort/semester interactions based on 8-year cohorts. The estimated coefficients for the main outcomes are somewhat smaller than the baseline results in Table 2, but they remain significant at the usual confidence levels, with the exception of the benefit-eligible employment conditional on registered employment outcome in column 5. The regressions were also estimated including interaction terms between the *AnyChildren* indicator and the full set of demographic covariates in order to allow control variables to enter separately for the treatment and the comparison groups. Results in Panel B indicate that the main estimates are also robust to this alternative. Another specification tests includes interactions of all demographic controls with the post-reform indicator to explore if other covariates could explain the responses in labor market outcomes for individuals with children relative to those without children after the HIBEX. The results in Panel C indicate that the main results are robust to the inclusion of these additional controls, with the exception of the conditional benefit-eligible employment outcome in column 5.

Finally, Panel D in Table 7 presents the estimates of the main outcomes of interest for the subsample of individuals for which there was no change in the payroll tax (those with earnings below the 2.5 BPC eligibility cut-off — see Section 2.3). As discussed above, the

[27] For instance, Uruguay's economy grew steadily during the period under study. Individuals with and without children might respond differently (even conditional on their observable characteristics) to episodes of growth.

Table 8
Net impact of the health insurance benefit extension on the government budget: Mechanical effects and behavioral responses.

I. Tax revenue	Payroll tax (2007)	Change in payroll tax	Individuals (2007)	Change in number of individuals	Monthly avg. salary (2007 UYU)	Total annual change (million UYU)
A — Mechanical effect						
A. 1 — Childless individuals		0.015	67,122		12,889	155.7
A. 2 — Parents		0.03	159,250		12,689	727.5
B — Behavioral effect						
B. 1 — Parents	0.08	0.03		5574	12,689	93.4
B. 2 — Underreporting (small firms)	0.08	0.03	7,657	332	8,231	−2.3
C — Total (A. 1 + A.2 + B. 1 + B.2)						974
II. Expenditure			**Individuals (2007)**	**Change in number of individuals**	**Expenditure per individual (2007 UYU)**	**Total annual change (million UYU)**
D — Mechanical effect						
D.1 — Children acquiring the benefit			242,072		603	1750.7
D.2 — Reduced public sector exp. on children			162,632		−350	−683.1
E — Behavioral effect						
E. 1 — Parents entering benefit-eligible empl.				5,574	809	54.1
E. 2 — Children of parents entering empl.				8,473	603	61.3
E. 3 — Parents + children leaving the public sector				10,232	−350	−43.0
G — Total (D. 1 + D.2 + E. 1 + E. 2 + E.3)						1140.1
Budget deficit, mechanical (D1 + D2 − A1 − A2)						184.5
Net behavioral effect (E1 + E2 + E3 − B1 − B2)						−18.7
Total effect (G − C)						165.8
Budget deficit/government expenditures on health insurance (2007)						0.284%
Budget deficit/GDP (2007)						0.030%

Notes: The mechanical effects correspond to the changes in revenue due to the changes in payroll tax contribution rates after the reform and to changes in expenditure due to the payment of health insurance premiums and the fall in the use of the public health system by those who enrolled in the new benefit after the reform. The behavioral effects take into account the impact of the reform on labor market and tax evasion outcomes. See Section 7 for more details.

comparison between eligible and ineligible individuals in this group has the advantage that, while eligible employees were potential beneficiaries of the benefit extension, neither group faced an increase in payroll tax contributions. The estimated coefficients of interest are similar in magnitude to those from the baseline estimates in Table 2, although only the coefficient of the reform on the benefit-eligible employment outcome is statistically significant at the 10% level; the outcome on underreporting is significant at the 5% level. Since this group did not see an increase in payroll tax, we should expect a larger effect, and this is in fact the case: relative to the pre-treatment dependent variable mean of 0.46, the effect on eligible employment for the whole sample of 0.0162 (Table 2, column 1) represents an increase of about 3.5%. The corresponding pre-treatment mean is 0.11, so the 0.0168 increase (Table 7, panel D, column 1) implies a much greater effect in relative terms (about 15.3% greater). The fact that the estimates are still robust for this subgroup reinforces the identification strategy, indicating that the increase in payroll tax for the control group is not a concern. The analysis for this subgroup, however, lacks statistical power: the samples for these alternative comparison and treatment groups are substantially smaller and, unlike the analysis for the full sample in Tables 4 and 5, this one does not allow for a meaningful analysis by subgroup.

In general, these robustness and specification tests suggest that changes in the composition of the treatment and comparison groups did not introduce a spurious correlation between changes in the outcomes before and after the reform.

7. Financial implications and discussion

The HIBEX was designed to increase the scope of health insurance coverage for the dependent children of registered employees. As depicted in Fig. 1, take-up of the benefit was very high, triggering a large increase in government expenditure in the form of private health insurance premiums for newly eligible children. Part of this expense was offset by a reduction in the costs of the public health system. In terms of revenues, the reform had several implications. On the one hand, tax collection increased. An increase in contributions for most registered workers generated higher payroll tax receipts. On the other hand, in addition to this direct or mechanical effect, the reform also resulted in an increase in registered employment. This behavioral effect generated both higher tax collection and higher expenditures. Finally, the results discussed above also indicate that the reform had a dual effect on tax evasion in the labor market: registered employment increased, but underreporting of salaried earnings increased as well. The following pages discuss the overall impact of the reform on government finance due to each of these effects, which are summarized in Table 8. This estimate of the fiscal incidence of the reform is restricted to individuals in the survey sample, and it relies on a series of assumptions.[28]

The first step for computing the fiscal incidence of the reform is to estimate the mechanical changes in tax revenue and expenditure. Childless registered workers did not imply any additional expenditure after the reform, but they represented higher revenues. The fraction of the payroll tax allocated to health insurance for these workers increased by 1.5 percentage points. Based on an average monthly salary of UYU 12,889.2 for the 67,122 childless registered employees working 25 h or more per week in 2007, annual government revenue increased by UYU 155.7 million from this source (row A.1, column 6 — all values in monthly UYU are multiplied by 12 to obtain the results in annual terms).[29]

The second mechanical effect follows the change in payroll taxes and healthcare insurance benefits for the 159,250 individuals with children in benefit-eligible employment in the pre-reform year, 2007. In terms of revenue, based on the average taxable earning of UYU 12,689.2 per month in 2007 for this group, the 3 percentage point increase in

[28] The values of all parameters correspond to the year 2007, except when indicated otherwise (for instance, the parameters that changed with the reform (contributions) or new parameters, such as the insurance premiums paid by the SIA, which are based on figures from 2008). Information on expenditure per individual, such as health insurance premiums, is taken from administrative sources (Banco de Previsión Social, 2008, 2009; Ministerio de Salud Pública, 2010a). All monetary values in UYU are expressed in 2007 prices.

[29] As a further simplifying assumption, the analysis does not take into account the level of underreporting for those already in registered employment before the reform (rows A.1 and A.2), which stayed roughly constant. This would complicate the analysis and require further assumptions, which would, in turn, result in somewhat lower figures for tax revenue in column 6 of rows A.1 and A.2. The analysis highlights instead the reform-induced behavioral changes in underreporting of earnings for workers at small firms (row B.2).

health insurance contributions resulted in an increase in revenue of UYU 727.5 million (row A.2, column 6). On the expenditure side, the reform generated an additional expenditure in the form of health insurance premiums paid by the SIA for the children of these workers in 2008 at UYU 603 per child per month (in 2007 prices). The total number of children covered by the reform was 242,072, which resulted in an additional expenditure of UYU 1750.7 million (row D.1, column 6). However, a substantial fraction of the children were covered by the public health sector in 2007 at an average cost of UYU 350 per child per month, so this shift in their health insurance arrangements from public to private amounted to savings of UYU 683.1 million (row D.2, column 6) and resulted in a substantially lower net increase in expenditures.

The changes discussed thus far are limited to mechanical effects. The evidence in this paper, however, indicates the presence of behavioral effects due to the HIBEX, which has further consequences for government expenditure and tax revenues. The estimates from Table 2 show that, on average, the HIBEX increased benefit-eligible employment by 1.62 percentage points, a 3.5% increase with respect to the pre-reform levels of 2007. This represented an increase of 5574 (0.035 × 159,250) in the number of workers in benefit-eligible employment and 8473 newly eligible children (average of 1.52 children per registered employee in 2007). Computing the annual premiums for adults (UYU 809) and children (UYU 604) paid by the SIA in 2008, the additional annual expenditure in health insurance attributable to this behavioral effect is UYU 115.4 million (rows E.1 and E.2, column 6). However, 4450 of the newly covered adults and 5692 of their children were covered by the public sector before the reform (based on the 2007 coverage of the public healthcare system, 67% for parents and 81.5% for children), so this shift in healthcare insurance arrangements from public to private reduced public-sector expenditure by UYU 350 per individual per month for a total of UYU 43 million (row E.3, column 6).

In terms of revenue, the increase of 5574 in benefit-eligible registered workers implied higher contributions to the health insurance fund for an annual total of UYU 93.4 (row B.1), yielded by an average monthly earning per worker of UYU 12,689.2, the additional pre-reform employer contributions (5% of earnings), the additional pre-reform employee contribution (3%), and the increase in the payroll tax rate due to the reform for this group (an additional 3% of earnings).

Finally, also in terms of revenue, an additional factor is the increase in underreporting of salaried earnings for registered employees at small firms. It can be assumed that earnings were underreported by 25%, which represents the middle point between average earnings for registered employees at small firms and the minimum wage. Table 6, Panel B, indicates that the reform increased underreporting by 4.09 percentage points for benefit-eligible employees at small firms, which represented an increase of 24% in the incidence of underreporting with respect to the pre-reform period. Of a total of 45,042 registered employees with children working at small firms in 2007, 7657 (17%) declared underreported earnings, and the average monthly earning for this group in 2007 was UYU 8231. Taking into account the 3% increase in contributions, tax revenue from this effect fell by UYU 1.4 million. A similar analysis applies to the subset of parents who entered benefit-eligible employment in small firms after the reform (about 28% of the original 5574 workers). A total of 332 of these 1576 new workers underreported earnings. Including the original health insurance contribution of 8% and the additional 3% due to the reform, this implied an additional fall in (potential) tax collection of UYU 0.9 million. Overall, tax collection fell by UYU 2.3 million due to the behavioral response of underreporting salaried earnings (row B.2, column 6).[30]

To sum up, on the basis of these assumptions, the direct or mechanical effect of the reform implies an increase in the budget deficit of ([1750.7 − 683.1] − [155.7 + 727.5]) = UYU 184.5 million. Furthermore, the behavioral impact of the reform actually implied an additional total effect of ([54.1 + 61.3 − 43.0] − [93.4 − 2.3]) = UYU −18.7 million, i.e., a reduction in the budget deficit. This implies that the increase in tax collection through the increase in the number of registered workers more than compensated for the additional public expenditure incurred by covering the health insurance premiums of those individuals and their families. Another relevant conclusion is that the additional revenue created by the increase in registered employment following the reform, UYU 93.4 million, exceeded by a factor of about 40 the loss of revenue of UYU 2.3 million due to the higher levels of underreporting induced by the HIBEX; the increase in the intensive margin of payroll tax evasion was of a second order when compared to the gains from the reduction in its extensive margin.

Taken together, the results indicate that the HIBEX increased the budget deficit by (1140.1 − 974) = UYU 165.8 million or USD 10.5 million at the 2010 PPP adjusted exchange rate, which represents 0.030% of Uruguay's GDP in 2007 and 0.28% of total government expenditure on health care for that year. This additional 0.030% of GDP more than tripled the coverage of explicit health insurance for children.[31] While the specific figures depend on a series of assumptions, they provide suggestive evidence on the relative magnitude of the different effects at play. There are probably other important positive effects in terms of children's health and other spillovers from registered employment (access to pensions and other benefits), which should be included in a full cost–benefit analysis which is beyond the scope of this paper.

8. Conclusions

This paper studied the impact of a reform in Uruguay's social insurance system that extended the coverage of a health insurance benefit from registered employees to their dependent children. This extended benefit introduced a new incentive for parents to take up registered employment. The evidence confirms that individuals reacted as predicted, with a substantial increase in benefit-eligible employment for parents as compared to childless individuals. Also as predicted, single parents, parents with several children, and parents with younger children entered registered employment at a higher rate. For individuals in couples, responses to the reform depended on their partner's employment's benefit-eligibility. The increase in benefit-eligible employment was driven by both reductions in the number of those not working and (for single parents) by shifts from unregistered to registered employment. The results indicate a complex pattern of tax-evasion responses in the labor market following the policy change. The reform also induced reactions along a further margin of adjustment in tax compliance, with higher levels of underreporting of taxable earnings for benefit-eligible registered employees working at small firms. A fiscal incidence analysis of the reform indicates that the payroll tax increases associated with the benefit extension managed to cover most but not all of the increase in expenditure. Moreover, the reform's behavioral effect amounted to a small, but relevant, fraction of the total change in the budget deficit. Finally, the increase in revenue from the reform's positive effects on registered employment far outweighed the loss in revenue from increased underreporting of salaried earnings.

[30] This total is yielded by the UYU 1.4 million (0.24 × 7657 × 0.25 × 8231 × 0.03) and the 332 (1572 × 0.17 × [1 + 0.24]) new underreporters for UYU 0.9 million (332 × 0.25 × 8231 × 0.11).

[31] The total number of children enrolled in the program after the reform in 2008 was 250,545 (Table 8, D.1 + E.2). A first part of this total corresponds to the 79,440 children of registered employees who were already enrolled in private health insurance because their parents paid for it before the reform. The second component corresponds to the mechanical effect for children of registered employees. Before the reform, there were 162,632 children of registered employees who used the public health sector, and who then enrolled in private health insurance because of the reform (row D.2). The third component is the behavioral response. Parents who entered benefit eligible employment and enrolled their eligible children implied 8473 newly covered children (row E.1). Compared to the baseline coverage of 79,440 children, the behavioral response represented an increase of about 10.7% in the coverage of explicit health insurance.

The analysis indicates that additional tax evasion margins of response mean complex trade-offs for workers and governments in the context of tax and benefit systems. In terms of tax evasion, the decision is not simply between off-the-book versus registered employment, but between full non-compliance, full compliance, and on-the-book salaried employment with under-the-table payments. These results confirm the insight that work incentives do not necessarily operate within the framework of the law and that the design of social insurance systems should take into account these additional dimensions.

The implications of this analysis are relevant not only for developing economies with large informal labor forces. The policy consequences are also relevant for OECD countries, most of which have experienced an increase in their shadow economies over the past two decades (Schneider and Enste, 2000; Schneider, 2005); they may well face trade-offs similar to those described for Uruguay, at least for part of their labor forces. Moreover, it is necessary to understand the channels through which the tax and benefit system yields changes in evasion behavior in the context of salaried employment. For instance, it would be important to distinguish between situations where the employer and the employee collude to evade taxes from settings where the firm engages in unilateral evasion. Third-party information reporting (which has been highlighted as an effective measure to induce compliance) and other specifics of tax law and implementation details may also play an important role in the reduction of unregistered employment and of the informal sector in developing countries.

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