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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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Comments welcome

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Abstract: The impact of the tax and benefit system on work incentives is one of the most salient policy issues in labor and public economics. Despite a concurrent ample literature on tax evasion, there is relatively little analysis of the joint work and evasion incentive effects introduced by social insurance and related programs. This paper contributes to the growing literature that attempts to bridge this gap. Using a quasi-experimental approach, it evaluates the behavioral responses of workers from a large scale expansion of employment-related benefits in the social insurance system of a middle income country, Uruguay. The policy change extended the coverage of a compulsory, in-work and payroll tax financed health insurance program to the dependent children of private sector salaried workers, but only to full-time registered workers – those in jobs complying with payroll tax and social security contribution requirements. The results indicate that individuals that benefited from the reform responded as predicted by economic theory, significantly increasing their labor force participation and hours of work, with most of the increase in registered (or "on" the books) employment. The analysis also presents original evidence on tax evasion within registered salaried employment. Besides the change in off the books employment, the results indicate an adjustment in evasion behavior along the intensive margin, with an increase in the under-reporting of earnings for workers who benefited from the reform. The conclusion illustrates how the consideration of these additional margins that are not contemplated by the canonical model has relevant policy consequences.

Keywords: labor supply, work incentives, tax evasion, social insurance
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1 Introduction

The impact of the tax and benefit system on work incentives is one of the most salient policy issues in labor and public economics. While there is also an ample literature and a wealth of empirical evidence on tax evasion and on the fiscal implications of welfare systems, the joint analysis of the work and evasion incentive effects of taxes and benefits has received less attention in academic and policy analysis. Most studies implicitly assume that work incentives operate within the framework of the law, yet tax and benefit systems can induce behavioral responses in tax registration and on the reporting of earnings alongside the more traditionally studied labor supply margins. The consideration of these additional margins has relevant policy consequences that are not contemplated by the canonical model. For instance, social transfer programs may induce work disincentives, but in a setting with tax evasion they might instead end up encouraging off-the-books employment, with very different fiscal and household welfare implications.

This paper contributes to the growing literature that attempts to bridge this gap and address additional margins of adjustment of labor supply and tax evasion in the context of social insurance and other tax and benefit programs. Using a quasi-experimental approach, it evaluates the behavioral responses of workers from a large scale expansion of employment-related benefits in the social insurance system of a middle income country, Uruguay. The policy change extended the coverage of a compulsory, in-work and payroll tax financed health insurance program to the dependent children of private sector salaried workers. The extended benefit was made available only to full-time registered workers – that is, those in jobs complying with payroll tax and social security contribution requirements. This change potentially affected work incentives, but also the cost-benefit balance of working in a non-registered job, which is a particularly relevant margin in an economy with a sizeable shadow sector (about a quarter of jobs are off-the-books), and with social insurance benefits conditional on registered employment. The results indicate that individuals that benefited from the reform responded as predicted by economic theory, significantly increasing their labor force participation and hours of work, with most of the increase in registered (or "on" the books) employment. The analysis also presents original evidence on tax evasion within registered salaried employment. Besides the change in off the books employment, the results indicate that workers adjusted their evasion behavior along the intensive margin, with an increase in the under-reporting of earnings for those who benefited from the reform.

Understanding the incentive effects of public policies on both labor supply and tax evasion margins is important in developing and middle income countries with widespread levels of tax evasion, non compliance with regulations and weak enforcement. However, as argued below, these issues are also relevant in advanced economies with smaller but still sizeable and growing

underground sectors, and also in the context of increasingly dual labor markets in developed countries (Boeri, 2010). The size of the shadow economy in the early nineties ranged from more than 65 percent of GDP for countries like South Africa, Thailand and Nigeria, to about 30-40 percent for middle income countries (such as Uruguay) and transition economies (Hungary, Russia) and 15-30 percent of GDP for Southern European OECD countries such as Greece, Italy and Spain (Schneider and Enste, 2000). While the figure for this period was below 10 percent for the United States, Schneider (2005) reports that the underground economies of most countries grew between the early 1990s and the early 2000s, with a larger increase in developed countries and those in Central and Eastern Europe. These levels are also reflected in the proportion of the labor force that participates in the shadow economy, which Schneider and Enste (2000) report was in the 10-25 percent range by the late 1990s in some OECD countries (22.5 percent in Denmark, 22 percent in Germany, 32.3 percent in Spain and 12 percent in France). This evidence suggests that the effects of social insurance programs on the tax evasion margin can be quantitatively relevant for most of the world's economies. As in the previous example of cash transfers, policy design should reflect the full range of possible behavioral responses and not be limited to lawful or permitted adjustments – incentives and disincentives matter for work but may also matter for employment registration and payroll tax compliance, with important fiscal and labor market implications.

This paper is related to the substantial literature on the work incentive and broader labor market effects of tax and benefits systems, which range from those covering relatively simple cash and cash-like transfers (Moffitt, 2002) to the assessment of ample social insurance systems (Krueger and Meyer, 2002) and in-work benefits schemes such as the Earned Income Tax Credit (Hotz and Scholz, 2003; Eissa and Hoynes, 2006). The effects of Uruguay's health insurance benefit extension (HIBEX), part of a series of tax-benefit programs linked to employment, are related to a series of specific studies on work incentives and different margins of labor supply. With respect to labor force participation, the empirical literature generally finds a reduction in employment from increased benefits, although most of the previous studies are related to social insurance benefits for the unemployed (Krueger and Meyer, 2002) or for those out of the labor force, such as disability insurance (Bound and Burkhauser, 1999). In the context of mandated employer provision of workplace benefits, Gruber (1994) does not find significant effects on employment of a mandated maternity benefit in the United States, while Buchmueller et al. (2011) similarly fail to find significant effects on employment for a health insurance mandate in Hawaii.

By conditioning benefits to employment, the reform discussed below is also related to the notion of the entitlement effect, whereby unemployment benefits induce a move from inactivity to job search (see Mortensen's 1977 seminal contribution and its related literature), although the discussion below focuses on changes in employment. The benefit-employment linkage also related

this reform to the literature on in-work benefits, such as the United States' Earned Income Tax Credits (Hotz and Scholz, 2003; Eissa and Hoynes, 2006) and Temporary Assistance for Needy Families (Moffit, 2003) programs, and the United Kingdom's Working Families Tax Credits (Brewer et al., 2009). These studies tend to find positive and significant program effects on the extensive margin, with larger responses for single mothers (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Francesconi and van der Klauuw, 2007). The evidence on responses along the intensive margin is more limited. Most estimates of hours conditional on working are small (Eissa and Liebman, 1996; Blundell and MacCurdy, 1999). Francesconi and van der Klauuw (2007) find that minimum hours of work requirements in the case of the WFTC induced an increase in employment above the relevant threshold, although the overall effect in the intensive margin is not unambiguous due to the income effect for those already working full time (Gregg et al., 2009). In addition, some studies that focus on labor supply responses of individuals in couples have found evidence on work disincentive effects of income transfer programs, in particular on the couple's secondary worker (Eissa and Hoynes, 2004). As in the analysis presented in this paper, most of these studies rely on comparisons between individuals with children and childless adults, which are not affected by the policies and are thus used as control groups.

There is a growing series of studies at the intersection of the tax evasion-shadow employment and labor supply responses to tax and benefit systems. De la Rica and Lemieux (1994) discuss the impact of public health insurance on the underground economy, comparing the cases of Spain and the United States in the early 1990s. This discussion is especially relevant for Uruguay, since access to health insurance is also linked to employment and payroll taxes as in the case of Spain and thus imposes similar constraints on workers. De la Rica and Lemieux (1994) find that despite the differences between the Spanish and American systems, their labor market outcomes consequences are similar when one assimilates firms not providing insurance (in the United States) with those not complying with social security taxes (in Spain). Lemieux et al. (1994) study the effects of Canada's broader tax system on labor supply in the underground economy. They find that for an average worker, the tax and transfer system does not distort significantly the allocation of hours from the regular to the underground economy, although the effect seems to be more important for specific groups eligible for welfare programs. While Lemieux et al. (1994) follow a structural approach, the analysis below relies on a quasi-experimental study of a specific reform. Potter Gunter (2010) also relies on a quasi-experimental approach based on benefit variation by state for her study of the effect of the EITC on participation on regular (registered) and informal (off-the-books) work among low income individuals. This type of analysis is based on Sandmo's (1981) and Cowell's (1985) extension of the Allingham and Sandmo's (1972) and Yitzhaki (1974) canonical model of tax evasion. Potter Gunter (2010) finds that the higher levels of benefits in some states induces an

increase in registered work and a fall in off-the-books hours, with no significant effect on the extensive margin nor on total hours supplied. A further relevant antecedent is Boeri and Garibaldi's (2007) search model which incorporates firm's and worker's decisions at the shadow employment, legal employment and unemployment margins simultaneously. Comparative statics within their general equilibrium framework indicate that increased registered employment conditional benefits induces higher participation in the legal sector – akin to a "registered" employment entitlement effect on labor supply and tax evasion.

Besides off-the-books work, another dimension of tax evasion analyzed below is the under-reporting of earnings. The tax evasion literature has typically concentrated on under-reporting of self-employment earnings (Hurst et al., 2011), since the withholding and reporting requirements of wage and salaried earnings make under-reporting difficult (Slemrod and Weber, 2010) and thus quantitatively small, particularly in the United States (Slemrod, 2007) – the typical theoretical models in fact assume no evasion on wage and salary income (Feldman and Slemrod, 2007). However, this literature has overlooked the potential of collusion between employers and employees to deceive the tax authorities and settle a fraction (but not all) of earnings under the table (Sandmo, 2011), which would result in under-reporting even with token compliance with withholding and reporting requirements. This issue is particularly relevant when there are benefits associated with registered employment for the worker that bear no direct cost to the employer, as in the case of the HIBEX reform in Uruguay. Tonin (2011) builds a theoretical model where firms and workers can collude and fail to report part or all of the employee's salaried earnings to the fiscal authority "to avoid the payment of taxes and social security contributions". While the model incorporates a penalty for being caught evading, it does not contemplate employment-related benefits linked to registered work. Tonin (2011) concentrates on the impact of introducing a minimum wage in this economy. The predictions of the model are consistent with the empirical evidence derived from the analysis of a large increase in the minimum wage in Hungary in 2001. This indirect evidence, based on household's consumption patterns, indicates that the minimum wage induced higher compliance and thus lowered disposable income for low and mid skilled workers.

This paper is also related to recent contributions to the literature on the informal sector, and more specifically, on labor informality defined as jobs without access to social insurance benefits. The analysis below takes into account worker's preferences with respect to these employment-related benefits, as in Levy (2008) and Galiani and Weinschelbaum (2011), although it does so in the context of a simple labor supply framework augmented with tax evasion, related to the tax evasion literature discussed above and to Fortin et al.'s (1997) evasion dualism. The effects of the HIBEX reform are concentrated in the upper part of the earnings distribution, as stressed by Fields (1990). The analysis and the results are also related to recent work on informality, evasion and contract

enforcement (Basu et al., 2011; Basu et al., 2012), and complement Fugazza and Jacques' (2003) considerations on the balance between deterrence and the increase of the "individual benefits of participating in the regular sector". Finally, the impact of the HIBEX reform on wages can also be interpreted in the light of compensating wage differentials for employment related benefits (Levy, 2008) and of the literature on non-pecuniary benefits (Gruber, 2000), specifically in terms of the negative correlation between wages and job-related amenities (Powell and Shan, 2012).

To evaluate the labor supply and tax evasion margins of response to social insurance programs introduced by the linkage of benefits to registered work, this paper exploits a policy change in the Uruguayan social insurance system. The reform extended the coverage of a compulsory, employment-based health insurance program to the dependent children of full-time salaried workers in private sector who are registered with the social security administration. The identification strategy relies on the fact that childless adults were not eligible for this benefit extension, and thus the impact of this policy can be estimated using a difference-in-difference methodology by comparing the labor market behavioral responses of adults with children with that of childless individuals before and after the reform. This quasi-experimental setup is applied to repeated cross sections of a nationally-representative household survey which covers the period before and after the reform. The reform also increased the rate of contributions deducted from workers' wages, although as described in detail below the extended benefit had a market value far greater than the increase in the payroll tax liability for the typical worker.

The results indicate that, as predicted, both work incentives and tax evasion in the labor market are relevant margins of response to changes in tax and benefit systems. In the presence of new entitlements that make registered work more valuable, individuals increase their labor force participation and adjust their hours of work to take advantage of the new situation, although the extensive margin is the most relevant. At the same time, workers adjust their behavior along the extensive and intensive margins of tax evasion (Sandmo, 2011), by increasing the probability of registered work while simultaneously increasing the under-reporting of earnings, respectively. More specifically, the results indicate that the policy change led to a substantial increase in the employment rate of individuals with children, driven by an increase in registered work, and to a small but significant positive effect on the intensive margin of their labor supply, as predicted, given the full time eligibility requirement. These labor supply responses were highly heterogeneous, with effects concentrated among younger, highly educated workers, and among lone female parents. There is also evidence that secondary workers in the household reacted strategically to this extension of coverage, which only required one registered worker within the household, by reducing registered employment.

The results indicate a complex pattern of tax evasion responses in the labor market beyond changes in registered (or "on the books") employment. Exploiting a unique feature of the data, the analysis uncovers direct evidence of under reporting of salaried earnings to the tax authority for those affected by the reform, which coupled with a small but significant decline in registered workers' earnings suggests some form of negotiation and collusion between firms and employees.

This paper makes a series of contributions to the labor supply and tax evasion literatures. It first presents quasi-experimental evidence from a policy reform on several margins of labor supply responses to changes in employment-related benefits, extending the results of the existing literature to the case of a middle income country. Moreover, the analysis relies on an extended tax and benefits-labor supply framework that accommodates incentives to tax evasion along its extensive and intensive margins, providing evidence adjustments to policy reforms in terms of off-the-books employment and under-reporting of earnings. The paper thus uncovers both tax evasion and "real" behavioral responses to the policy change (Saez, Slemrod and Giertz, 2009). Finally, besides more standard results on the effect of policy on outright tax evasion (non-registered employment), the paper also presents original and direct evidence on the under-reporting of salary and wage earnings, potentially extending the range of "evidence on the invisible" tax evasion (Slemrod and Weber, 2010).

The conclusion of the paper discusses the implications of these findings for middle income and advanced economies. In this setting, the relevant trade-offs for workers and governments become more complex. In terms of tax evasion, the decision is not simply between off-the-books versus registered work, but between full non-compliance, full compliance, and "on-the-books" salaried employment with under the table payments. The latter case is directly related to recent contributions on the elasticity of taxable income for high earnings individuals (Chetty, 2009; Piketty, Saez and Stantcheva, 2011). The results imply that lower income workers might also engage in tax avoidance and compensation bargaining when facing policy changes. Finally, some of the results may apply directly not only to the shadow economies of developed countries, but also to their dual labor markets. For instance, recent work indicates that Hawaii's employers rely on part-time workers because they are not covered by the health insurance mandate (Buchmueller et al., 2011), which is consistent with the evidence uncovered here.

This paper is organized as follows. Section 2 describes the institutional background of social insurance programs in Uruguay, and describes the 2008 reform. Section 3 details the work and tax evasion incentive effects introduced by the reform. Section 4 describes the data and the identification strategy for the estimates of the effects of the reform. Section 5 presents the main results on labor supply, registered employment, wages and under-reporting of salaried earnings. Section 6 in turn discusses a series of robustness and specification checks. Section 7 concludes with a discussion of

the implications of these results for the analysis of tax and benefit systems in developing and advanced economies.

2 Institutional background

2.1 Social insurance programs in Uruguay

Uruguay has one of the oldest, most developed social security systems in Latin America. The following is only a partial review of the relevant aspects for this paper – for more details, see Ferreira-Coimbra and Forteza (2004). This social security system follows mainly a European Bismark-type of model, were access to most social insurance programs is linked to employment. The *Banco de Previsión Social* (BPS) is the institution in charge of most of the social security administration (henceforth, it is referred as the SSA). It collects payroll taxes and contributions, and provides social insurance benefits for 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, sickness benefits, unemployment insurance, disability benefits, maternity leave, and family allowances (a cash transfer benefit paid for each of the worker's dependent children).[1]

By law, private sector employers in Uruguay are required to register their employees with the SSA. Both the employer and employee are required to make social security contributions to finance social insurance programs. Formally, the employer deducts the employee's contributions from her wages, and pays both the employee's and the employer's contributions to the SSA. This payroll tax amounted to about 31 percent of taxable wages in 2007, the year before the reform studied in this paper. Table 1 shows the breakdown of the SSA contributions required by law as a percentage of the taxable salary for private sector employers and employees. Although the total payroll tax consists of contributions to different programs, it is transferred in full by the employer to the SSA and thus usually likened to an overall payroll tax. Moreover, the social insurance benefits are provided as an indivisible bundle – workers or employers cannot op out of certain programs and pocket the correspondent contribution, although there are some minimum work requirements for some benefits as discussed below. Registered work (compliant with social security regulations and payroll taxes) thus grants the worker access to the full bundle of benefits. Although the SSA also covers the self-employed and public sector employees, these groups of workers are subject to a different set of rules and programs.

[1] This could also be referred to as the social protection or social security system. The social insurance terminology is better suited to avoid confusion with the (mostly) pension-related definition of social security in the United States. Krueger and Meyer (2002) provide a more detailed definition and an in depth discussion of social insurance programs.

As in many middle income countries, enforcement in Uruguay is relatively weak and there is widespread non-compliance with social security regulations and evasion of the payroll tax, which results in a substantial fraction of private sector salaried workers who lack access to the employment-related social insurance benefits. This phenomenon is sometimes referred to as labor informality, although the term has suffered from conceptual stretching and this paper prefers to use more precise terminology based on the situation of the worker with respect to the SSA – thus the rest of this paper refers to an employee's registration status, or alternatively to a job's compliance with payroll taxes and social security regulations.[2] Unregistered (or off-the-books) workers amounted to about a quarter of total wage employment in the decade of 2000 (Gasparini and Tornarolli, 2009; Heckman and Pagés, 2004), and represented about 28 percent of private-sector workers in 2007, as computed with microdata from the *Encuesta Continua de Hogares*, described below.

2.2 The health insurance benefit

Eligibility for the health insurance component of the social insurance system has some additional requirements. Besides being registered with the SSA, employees must work 25 or more hours per week[3] – referred to as part time and full time workers from here onwards. Before the 2008 reform, the health insurance benefit was financed by contributions to the SSA amounting to eight percent of taxable salaries, with five percent from corresponding to the employer's contribution and three percent to the employee's (Table 1).

Eligible workers are entitled to select a health insurance provider in the private sector, similar to HMOs in the United States. The insurance premium is paid for by the SSA. This health insurance package covers services for the worker which are similar to those offered (usually by the same institutions) in the private health care market. These packages include inpatient hospital coverage, emergency room care, medical and surgical services, and maternity care, among others.

By 2007, the health insurance benefit covered 43 percent of the labor force, and approximately 66 percent of private sector salaried workers (figures based on the *Encuesta Continua de Hogares*). In that year, the SSA spent about UYU 8 billion (USD 514 million at the 2010 PPP adjusted exchange rate) in health insurance premiums, which represented 13 percent of its budget and 1.5 percent of the Uruguay's gross domestic product (GDP) (Banco de Previsión Social, 2008). In

[2] For a discussion about the definition of underground economic activities see Schneider and Enste (2000). See also Gerxhani (2003) for a extensive literature survey on the informal economy in developing countries.

[3] 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* (BPS). The BPC is an index created by in 2004 (Law 17,856), which replaced the minimum wage as the indexing basis for all benefits. The baseline took the value of the minimum wage in force on 20 December 2004. It was UYU 1,636 on 2007 – about USD 103 at the 2010 PPP adjusted exchange rate.

addition, the health insurance benefit for employees represented about 16 percent of total health care expenditures in Uruguay in 2007 (Ministerio de Salud Pública, 2010a).

Until 2007, the health insurance benefit covered only the registered worker. Non-registered workers, workers' spouses who were not themselves in a registered job and their dependent children had to pay for their own health care expenditures, purchase private health insurance, or use the public health care system, which is subject to a means test.

2.3 The 2008 reform and the health insurance benefit extension (HIBEX)

The Uruguayan government embarked in 2007 in an ambitious overhaul of the health care system, with the stated objective of strengthening three areas: health care coverage; health management; and health care financing. The resulting reform proposal was enacted by Uruguay's Parliament (Law 18,211/2007), which established a series of new guidelines, regulations and institutions for the health care system aimed at furthering equitable access.

The most important provision of the reform[4] was the extension of the health insurance benefit provided by the SSA, which only covered the registered worker, to her dependents. The target were the workers' spouses and dependent children, but for fiscal reasons this extension was implemented in stages. For the period covered in this paper, the expansion only reached the dependent children of registered workers.[5] The reform was passed in December 2007 and it took effect on January 1, 2008.

Eligibility for the health insurance benefit extension (henceforth, HIBEX) required that the registered worker should have at least one dependent child younger than 18 years old.[6] The reform established that the inclusion of these children under the benefit was universal, irrespective of pre-existing health issues and of previous health insurance arrangements. As with the previous individual-based benefit, the worker had to be registered with the SSA (and thus in a job complying payroll taxes and social security regulations), and work more than 25 hours or earn above a minimum threshold. While it covered all salaried employees, the HIBEX was most relevant for private sector registered workers, since a large fraction of public sector workers were already entitled to health insurance coverage for their dependents (Ministerio de Salud Pública, 2010a).

[4] Borgia (2008) provides more details of the health care system reform, including the benefit expansion discussed here.

[5] The extended benefit also reached children younger than 18 whose parents both became retired and were eligible for an SSA old-age pension after the reform. It also covered 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 percent of all children enrolled in the health insurance benefit by their parents (Banco de Previsión Social, 2009)

[6] A dependent child is defined for the purpose of the SSA benefit as an offspring, a stepchild or a foster child of the registered worker, younger than 18 years old. Disabled children above this age threshold also qualify for the HIBEX. The reform also established that the dependent children enrolled in the HIBEX could remain covered when reaching 18 years old until age 21 if their parents reimbursed the value of the child's insurance premium to the SSA.

HIBEX beneficiaries could choose a health insurance provider in the private sector, or rely on the pubic health care system for their children – irrespective of this choice, the SSA reimbursed the each individual's premium to the corresponding institutions. Registered workers, however, opted overwhelmingly for the private sector, with only about 5 percent of all children enrolled in the extended benefit using the public system in 2008 (Banco de Previsión Social, 2009). This figure reflects the high valuation that individuals placed on the health care packages provided in the private sector with respect to the publicly-provided services.

To finance this massive extension, the legislation specified an increase in almost all payroll contributions for workers. The employee's contribution for the health insurance benefit increased from 3 to 6 percent of taxable earnings for private-sector workers with dependent children under 18 years old, irrespective of the number of qualifying children. The reform also increased the contribution for childless workers and for those whose children did not qualify (those older than 18) from 3 to 4.5 percent of their taxable earnings. The payroll tax remained unchanged at the 3 percent level for low-wage workers, defined as those with monthly earnings 2.5 times or less the current value of the BPC (UYU 1,775 in 2008, USD 112), irrespective of whether they had children qualifying for the HIBEX or not. Employer contributions, on the other hand, remained unchanged by the reform at 5 percent of taxable earnings – the reform's designers intention was not leave firms' employment costs unaffected. Table 1 summarizes the changes in the contributions and the total payroll taxes introduced by the HIBEX reform.

As a part of an effort to improve the efficiency of the health care system and contain the cost of increased coverage, the reform modified several aspects of the benefit's resource allocation system. For instance, the SSA's reimbursements policy adopted a capitated formula based on the beneficiary's health-risk group. The SSA expected that the increase in efficiency from this change would result in significant reductions in cost per patient. This measure and the increase in payroll taxes should cover most of the cost of the extension of coverage.

The HIBEX represented a very large scale policy change with a substantial impact on the health insurance coverage of its target population. By 2008, 450 thousand children under 18 years old were insured through the HIBEX – about 48 percent of all children in this age-group in Uruguay (Banco de Previsión Social, 2009), out a total population of about 3.3 million individuals (Uruguayan National Bureau of Statistics, projection for 2008). The design of the program, which amounted to a health insurance subsidy for registered workers' children, and the public's preference for private sector providers implies that its introduction resulted in an increase in privately-provided insurance for children younger than 18. The effect, however, should be concentrated on children whose parents were employed in registered private sector jobs; the reform should not have affected the coverage of individuals above 18, because they were mostly unaffected by the policy change. Figure 1 depicts the

trend in the percentage of dependent children of private sector workers aged 0-17 (i.e., eligible for HIBEX) who used private-sector health care providers for the period 2004-2010, and, from 2008 onwards, 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.[7] In the period from 2004 to 2007, before the policy change, the evolution of private health care coverage for both groups was similar. Following the 2008 reform, the rate of private health care coverage for individuals aged 0-17 increased by 30 percentage points, and continued to grow (albeit slightly) until 2010. This radical change was driven by the HIBEX reform, as indicated by the series depicting the percentage of children 0-17 who accessed privately health care through the new program. In contrast, there was no significant change in the trend of private sector coverage for individuals aged 18 or older.

The HIBEX thus substantially modified the pattern of access to health insurance for a large proportion of workers with dependent children in Uruguay. The empirical results in this paper isolate the impact of this policy on individuals' labor market behavior by relying on the exogenous introduction of this reform and exploiting the differential work and tax evasion incentive effects for individuals with and without children. The following section briefly describes these potential incentive effects.

3 Work incentive effects of the health insurance benefit extension in the context of tax evasion

The HIBEX modified a benefit linked to registered employment and could thus induce a series of behavioral responses along the extensive and intensive margins of labor supply and tax evasion. The reform also increased the contribution (payroll tax) rate for most private sector employees by 1.5 percentage points, with an additional 1.5 levied on workers benefiting from the HIBEX. The reform did not affect employers, at least not directly – the additional tax was explicitly levied on workers to avoid increasing employment costs,[8] and the new benefit did not alter the legal requirements for registering workers with the SSA nor the penalties for failing to do so. Since the market value of the extended benefit greatly exceeded the increase in the tax, a straightforward way to analyze the effects of the program is to liken it to a government transfer conditional on registered work and targeted to individuals with children.[9] For a registered private sector salaried worker in 2007, the 3 percent

[7] Calculation using micro data from *Encuesta Continua de Hogares*, described below. Similar trends are found with administrative records from the Ministry of Public Health.

[8] While the consensus in labor economics indicates that the labeling of different components of payroll taxes is irrelevant and that only the aggregate rate matters, the explicit purpose of the policy makers might constitute an important signal in the context of the evidence on bargaining over wages and the collusion between workers and employers discussed below.

[9] The 1.5 percentage point increase in the social security contributions for all workers that accompanied the HIBEX is only marginally covered in this discussion, because of the far greater size of the reform's implicit transfer and because

payroll tax contribution represented about UYU 369 (USD 23) of the average taxable earnings of UYU 12,305 (USD 778) for this type of worker, while the value of the health insurance premium for a child in the private market in 2007 was on average UYU 859 per month (Ministerio de Salud Pública, 2010a), or about USD 54 at the 2010 PPP adjusted exchange rate.

This policy change yields several reduced-form predictions on labor supply and tax evasion responses, since the HIBEX introduction altered the net compensation of registered work and the cost-benefit balance between off the books and on the books employment. Figure 2 depicts a simplified budget constraint for an average private sector worker before and after the reform, in order to assess the financial incentive on both the extensive (employment) and intensive (hours conditional on work) margins of labor supply associated with the HIBEX. To highlight the main work incentives effects of the reform, the figure focuses on the case of a single parent. The labor earnings are based on the average hourly wage in private sector registered jobs – those that entitle individuals to the HIBEX.[10] The reform generates a discontinuous increase in the budget set at 25 hours, since HIBEX beneficiaries receive a cash-like subsidy to cover the insurance premium of their dependent children. The HIBEX represents a considerable increase in the disposable income of participating workers, since even for the case of one child the insurance premium in the private sector is substantially lower than the 3 percent tax increase. The implicit transfer is in fact increasing in the number of children, and this is exploited in the empirical section below.

Since the new benefit is only available in the positive range of hours of work, standard labor supply theory predicts an increase in employment. For individuals out of the labor force before the reform, the HIBEX induces an increase in participation akin to the "making work pay" mechanisms of in-work benefits. It can also be thought of as inducing an entitlement effect, although in this case the benefit accrues while holding the job rather than after the employment is terminated, as in the case of unemployment insurance. Moreover, HIBEX eligibility requires a minimum of 25 hours worked per week, and thus the reform's financial incentive is binding over the range of full time registered employment.

The overall effect of the HIBEX on hours of work for those employed before the reform is theoretically ambiguous. Those working part-time (less than 25 hours per week) have a clear incentive to may move to the high-hours range of the budget constraint and qualify for HIBEX for their children. On the other hand, full time employees face a minor substitution effect to reduce hours

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the available empirical evidence for Latin America indicates that even large changes in labor taxes had negligible effects on employment (Gruber, 1997; Cruces and Galliani, 2011).

[10] This budget constraint corresponds to that of a lone parent with one or two dependent children younger than 18 years old, earning an hourly wage rate of UYU 78.6 (the average for this category). This simplified version of the budget constraint incorporates the discontinuity introduced by the minimum hours requirement, but it ignores mostly non binding minimum earnings requirement of 1.25 BPC, and a new provision of the reform which implied that those earning between 1.25 and 2.5 BPC were entitled to the HIBEX but were not required to pay the increase in the payroll tax.

from the small increase in the payroll tax, and an ambiguously signed income effect from the HIBEX's implicit transfer. As stressed in the welfare literature, however, these hours effects tend to be second order to those in the extensive margin (Blundell and MaCurdy, 1999; Eissa and Hoynes, 2006).

The previous paragraphs, however, abstracted from the issue of registered and off-the-books employment. The linkage between the benefit and registered employment implies that the reform should also affect this additional margin at the intersection of labor supply and tax evasion. The simplest possible setting[11] is one of free occupational choice for utility maximizing workers, who compare the value of registered jobs (with payroll taxes and the bundle of social insurance benefits) and off-the-books jobs, which evade the social security contributions and do not provide entitlement to benefits. In equilibrium, wages are determined such that workers are indifferent between the types of job. The introduction of the HIBEX thus modifies the cost-benefit analysis between employment in registered and non-registered jobs.[12] As discussed above, for individuals with children and outside the labor force, or for those working part-time in a registered job, the reform introduces a net positive incentive to participate full time and in a registered job. For individuals working full time in registered jobs before the reform, the HIBEX increases the pay-off of this choice, although there might be some minor effects in hours worked. Finally, those working off-the-books before the reform and close to the margin of choice (at current wages) or hours, there is an incentive to move to a registered job, or to negotiate alternative employment conditions (i.e., getting "on" the books) with their employers within their current job to benefit from benefit extension.

The reform should thus increase total employment and modify the composition of jobs, with an increase in registered employment with respect to unregistered employment. In addition, the reform should also increase the proportion of those working full-time, especially in registered jobs, as well as increasing overall hours of work since registered jobs are characterized by higher hours with respect to off-the-books jobs, as depicted by the respective distributions in Figure 3.

The discussion so far only distinguished between individuals with and without children. However, the labor supply literature offers some guidance with respect to heterogeneity of these effects among the population. On the one hand, since the increase in the payroll tax is independent of the number of children, the reform creates greater financial incentives for individuals with more children. Moreover, the HIBEX might affect high-skill workers disproportionally, since they tend to have higher valuations of social insurance benefits and they exhibit higher rates of compliance with

[11] This process can be thought of in the context of Rosen's (1986) model of 'compensating differentials', as in Gruber's (2000) and Gruber and Currie's (2002) discussion of employer-provided health insurance and it implications for labor mobility in the United States.

[12] Even in the context of dual or segmented labor markets, it is still possible that workers at the margin between the two will react to changes in incentinves.

social security regulations (Levy, 2008). The HIBEX might also have a greater impact on single mothers, since this group has been found to be more responsive to financial incentives in the labor supply literature.

The expected impact of the reform on couples with children is ambiguous. Couples with both members out of the labor force or in off-the-books employment face an increased incentive to have at least one member in a registered job. However, there may be a disincentive effect on participation for secondary workers, because the HIBEX requires only one of the children's parents to be registered to warrant coverage. In fact, when the two parents are in registered jobs the reform implies a double taxation for the same level of the benefit, since the two are required to pay the higher contribution rate. The income effect from the transfer and the double taxation might thus reduce labor supply or induce a shift to unregistered work for secondary earners.

The discussion so far covered responses along the extensive and intensive margins of labor supply and the extensive margin of tax evasion in the form of off-the-books employment. There are two additional potentially relevant margins of adjustment. On the one hand, although the discussion so far has adopted a reduced form partial equilibrium approach, a stronger linkage of benefits to registered work might affect the relative remuneration of off and on the books employment. As discussed by Powell and Shan (2012), there might be a wage-amenity trade off and workers might be willing to forego earnings to benefit from the HIBEX if the new package is worth more than the fall in net income. On the other hand, as discussed above in the context of the new incentives faced by off-the-books workers, bargaining for improved work conditions in the form of registered employment might also result in collusion between the employer and the employee to report less than the full amount of salaried earnings to the SSA authorities. While in one scenario the worker implicitly pays for the additional benefit in the form of lower wages, in the underreporting case part of the additional cost (in the form of increased payroll tax contributions) is shifted to the government as tax evasion. A higher rate of registered employment could thus also be accompanied by more evasion of wage earnings, introducing a trade-off between off the books employment and partial under the table payments.

The exogenous introduction of the HIBEX in 2008 is exploited below in a natural experiment setting to unearth evidence on the potential behavioral responses along the intensive and extensive margins of both labor supply and tax evasion.

4 Data and empirical strategy

4.1 Sample construction and descriptive statistics

The empirical analysis in this paper is based on cross-sectional microdata from the *Encuesta Continua de Hogares* (ECH) survey carried out by the Uruguayan National Institute of Statistics (*Instituto Nacional de Estadística,* INE) since 1990. The ECH is the main source for socioeconomic, labor and demographic indicators in Uruguay. The survey is conducted according to international standards and it is routinely used by researchers and international organizations. The microdata and supporting documents, such as questionnaires and details on sample selection and stratification are all publicly available.

The ECH is a nationally representative household survey, which combines elements of living standards and labor force surveys. The analysis in this paper relies on two waves of the survey corresponding to the first and second semester of the calendar year. These repeated cross-sections cover the period from the 2nd semester of 2004 to 2nd semester of 2010 – that is, three years and a half before the reform and three years after the HIBEX. The sample for the analysis includes males and females aged 25 through 55 who are either the heads of household or the heads' spouses. 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 these areas in 2006.[13] The analysis is carried out on a sample of private sector salaried workers (excluding thus the self-employed), as well as unemployed and out of the labor force individuals in the relevant age range. The sample does not include government employees because a large proportion of them were entitled to health insurance for their dependents before the reform. The sample also excludes retired and disabled individuals, and those who were in full time education during the reference period of the survey. Taking all of these restrictions into account and after pooling all the survey waves results in a sample of 97,552 observations.

The ECH collects detailed information on the type of health insurance and the health care provider for each individual in the household. Since the 2008 health reform was a major policy innovation, the statistical institute took great care to accurately reflect the ensuing changes in the ECH questionnaires. The ECH also collects information on all the relevant outcomes discussed in the previous section – employment and labor force participation, hours of work, the SSA registration status of the employee, wages and whether there is underreporting of salaried earnings. The employment variable is an indicator coded as one for salaried employees who reports working

[13] 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 percent of the population in Uruguay resides in urban areas with more than 5000 inhabitants.

positive weekly hours in their primary job, and zero otherwise. The analysis also relies on a similarly constructed indicator variable equal to one when the individual works 25 hours or more per week, and zero otherwise. This part time/full time cut-off is determined by the eligibility requirement for health insurance coverage. The hours variable corresponds to the hours worked by private sector employees in their primary job during the week before the interview. The hourly wage variable is obtained from the reported earnings of salaried workers in their primary jobs during the month before the interview, divided by their monthly hours of work (weekly hours multiplied by 4.3). The hourly wages are deflated using the official consumer price index for all urban consumers, and correspond to local currency units (UYU) at constant 2010 prices. The registration status of a worker is derived from a specific self-reported variable which indicates whether contributions (payroll taxes) are paid to the SSA for the current job.[14] As discussed above, social insurance benefits are provided as an indivisible bundle, so the fact that a specific job does not comply with compulsory payroll taxes implies that the job is off the books and not registered with the SSA. Finally, the ECH questionnaire contains a unique feature for detecting the presence of illegal arrangements between employers and employees in the context of registered jobs. Since 2006, registered private sector workers are asked directly whether their salaried earnings are being underreported to the tax and social security authorities.[15]

The employment analysis, based on the working positive hours and working 25 hours and more indicators, includes all of the 97,552 individuals in the final sample. For the analysis of log hours per week and job registration, the sample is limited to the 71,147 private sector salaried workers in the sample. For wages, the sample is limited to those with positive log hourly wages, 68,374 observations. The analysis of underreporting of earnings is done for the 50,669 observations of private sector workers for the years 2006-2010. Finally, all of the estimates presented below are weighted with the corresponding ECH sampling weights, although as shown in the robustness section the results are very similar when based on un-weighted data (Table 9, Panel F).

[14] The specific question in the ECH is: "Are you contributing for 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, and 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 (2011).

[15] The specific question included in the ECH since 2006 is: "Are your contributions based on the total amount of your earnings in this job?" ("¿Aporta por la totalidad del salario en esa ocupación?").

4.2 Econometric strategy and identification assumptions

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 thus provide evidence on the direction and magnitude of the incentive effects discussed in the previous section.

The strategy to evaluate the effect of this policy impact consists of a difference-in-differences analysis (Angrist and Krueger, 1999; Bertrand, Duflo and Mullainathan, 2004) of a policy (or natural) experiment. 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 control group). The comparison group is assumed to capture the counterfactual trend for the treatment group which would have been observed in the absence of the policy change.

In this particular setting, the treatment group consists of individuals aged 25 to 55 with at least one dependent child younger than 18 years old – that is, those potentially eligible for the HIBEX. The control group consists of individuals in the same age group with no dependent children.[16] The control group consists of childless individuals in the same age range, since they were not directly affected by the reform – by definition, they had no dependent children to insure with the extended coverage and thus they should not have modified their labor market and related outcomes because of the reform.[17] Similar estimation strategies relying on differential effects by demographic groups have been applied to evaluate the effect of social insurance programs on labor supply (see Eissa and Liebman, 1996; Fransesconi and van der Klaauw, 2007, Boyle and Lahey, 2010) and wages (Gruber, 1994), among other outcomes.

The empirical analysis therefore compares adults with at least one dependent child younger than 18 years old with individuals in the same age group with no dependent children. The following is the basic difference-in-difference (DD) specification with controls on which most of the estimates in this paper are based:

$$Y_{ijt} = \alpha + \delta AnyChildren_{ijt} + \beta AnyChildren_{ijt} * Postreform_t + X_{ijt}'\gamma + \delta_t + \varphi_j + \tau_{jt} + \varepsilon_{ijt} \quad (1)$$

where i index individuals, j state and t time. The variable Y_{ijt} is the outcome of interest (such as individual employment status or hours worked per week); $AnyChildren_{ijt}$ is an indicator variable for individuals in the treatment group, coded as 1 if the individual has at least one child younger than 18

[16] Another potential control group is that of adults with dependent children aged 18 or more. However, as detailed above, this group had the option of paying the subsidized health insurance premium for their children aged 18 to 21, and thus their labor supply behavior might have been affected by the HIBEX. The sample size for parents with children above 21 is very small.

[17] While the reform increased the overall payroll tax rate for this group from 30.75 to 32.25 percent, the reform can be interpreted as imposing a 1.5 percentage point increase for all private sector worker's payroll tax rate, with an additional 1.5 percentage points for those with dependent children.

years old and zero otherwise; *Postreform* is a dummy equal to 1 in the post-policy period (from January 2008 onwards) and zero otherwise; and $AnyChildren_{ijt}$ *$Postreform$ is the interaction term between those 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, marital status and gender interaction, education (in 6 categories) and the number and age of children in the household (in 3 categories). The estimations also include controls for firm size and industry (5 and 9 categories, respectively) unless otherwise indicated, with the exception of the employment and full time work outcomes which are defined for those out of the labor force. δ_t and φ_j are a full set of semester and state fixed effects (Uruguay is administratively divided in nineteen states or "departamentos"), which account for any aggregate systematic shock to local labor markets correlated with, but not caused by, the HIBEX. Finally, τ_{jt} are state-by-semester effects to control for state-specific shocks over the period which might be independent of the national economic conditions.[18]

The estimations rely on a linear probability model (OLS) for the binary dependant variables.[19] The effects on hours of work and hourly wages are estimated as log-linear regressions. With respect to inference, all tables report Huber-White robust standard errors clustered within *AnyChildren* and semester group. The main results are based on the 2004-2010 period excluding the two semesters of 2008, since the policy change was effectively implemented throughout that year.[20]

The difference in difference 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 control groups. This assumption would be violated if there were unobservable shocks that affected differently the trends in the outcomes of interest for both groups – shocks correlated with the policy change and with the group-specific trends in outcomes. Section 6 presents a falsification exercise which relies on the pre-reform period to test for the presence of systematic pre-existing differences in the trends in outcomes between the two groups. Section 6 also presents some additional specification checks to explore this possibility. An additional concern would arise if some other contemporaneous

[18] The main results are robust to changes in the included controls. Regressions with different specifications or additional controls are presented in Appendix A, Table A3, Panel A. Additionally, the main results are also robust to the inclusion of the state unemployment rate instead of state-by-semester fixed effects (Appendix A, Table A.3, Panel B).

[19] Angrist and Pischke (2009) argue that linear probability model estimates do not differ substantially from those obtained by probit and logit, with the advantage that the DD estimate of β has a straightforward causal interpretation. The main results remain unchanged when computing probit estimates – marginal effects from selected estimations are reported in Appendix A, Table A3, Panel C.

[20] The main results remain unchanged when including observations from for 2008, with the exception of the hourly wage equation where the DD coefficient of interest is not statistically significant at the usual levels in some specifications. These robustness checks are reported Table 7, Panel F.

change affects individuals with and without children beside the health insurance benefit extension. 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.[21] The most important of these changes consisted of an expansion of a means tested transfer program for families with at least one dependent child, the family allowance benefit (AFAM). These changes affected the subsample of the treatment group (low-income individuals with children), presumably inducing a disincentive to work. However, this policy by definition should not have affected the relevant outcomes for the control group in the natural experiment discussed here. The AFAM would then only result in an attenuation bias for the estimates of the effects of HIBEX. While disentangling the effect of both the AFAM and HIBEX programs is beyond the scope of this paper, the empirical analysis considers the potential heterogeneity of effects by education groups, and implements some specification checks to test whether the AFAM benefit expansion affects the main HIBEX estimates (see Section 6). The second identification assumption is that the composition of each group remains constant over the period.[22] This assumption may be implausible, for instance, if the treatment group expands over time by including more diverse individuals. Although Equation (1) includes 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 assumption.

Table 2 presents summary statistics for some key indicators in the sample by treatment status before (2nd semester of 2004 to 2nd semester of 2007) and after (1rst semester of 2009 to 2nd semester of 2010) the policy change. There seem to be similar trends in most of these observable characteristics over the entire period, and some substantial changes in the outcomes of interest, such as labor force participation and registered employment. This is tested formally in the following two sections.

5 Empirical results

5.1 Baseline results: Labor supply and job registration

Table 3 reports the baseline estimates of the labor supply responses to the 2008 health insurance benefit extension in Uruguay. Each column contains the OLS estimates of Equation (1) fir the outcomes of interest. Two specifications are presented for each outcome – the odd columns report regressions estimates without controlling for state-specific shocks, and the even columns use

[21] For a detailed description of those programs and reforms, see Amarante et al. (2011), Borraz and González Pampillón (2011) and Manacorda et al. (2011).

[22] Abadie (2005) and Athey and Imbens (2006) provide precise definitions and further discussions of compositional changes in the difference-in-difference approach.

state-by-semester fixed effects. The dependent variable in columns (1) – (2) is an indicator of employment status, equal to one if the individual worked positive hours during the last month and zero otherwise.[23] In columns (3) – (4), the dependent variable is an indicator for working 25 hours per week or more and zero otherwise. The estimates for those outcomes are obtained from the whole sample of individuals aged 25-55. Columns (5) – (6) estimate the impact of the HIBEX on individuals' log weekly hours. Finally, columns (7) – (8) display the results from a regression of registered employment, an indicator equal to one if the employee is a registered worker and zero if she works off-the-books. For both hours and employment registration, the sample is limited to salaried employees in the private sector, and the specification includes additional controls for industry and firm size.

The first row in Table 3 presents the estimates of the interaction coefficient β in Equation (1), which captures the impact of the HIBEX on the outcomes of interest. The second row displays the estimates of the treatment variable (*AnyChildren* in Equation 1). For completeness, Table A1 in Appendix A reports the results for the complete set of coefficients in all four regressions. The last row in Table 3 reports the average of the dependent variable prior to the HIBEX implementation, 2004-2007. Regression estimates are qualitatively similar with and without controlling for state-by-semester fixed effects. The discussion thus focuses on the specifications with the full set of controls, which should better account for state-specific shocks over the period.

Column (2) in Table 3 indicates that following the policy change the employment rate for parents with at least one child increased by a statically significant 1.5 percentage points relative to childless individuals. In terms of the pre-intervention average, this effect represents a 2.3 percent increase in the probability of working. The results in column (4) confirm another predicted result. As discussed above, the implicit increase in net income induced by the HIBEX is only available for those working more than 25 hours of work per week because of the benefit's eligibility rule. The coefficient on the full time work indicator reported in column (4) indicates a positive and significant effect of HIBEX eligibility of 1.6 percentage points (a 2.8 percent increase relative to the pre-reform average). This estimate is close to the overall increase in employment for the treatment group, suggesting that the positive response in full time employment was predominantly driven by an increase in the proportion of parents participating in the labor market.

The log hours worked per week regressions for the subsample of employees, presented in Table 3, column (6), indicates a positive effect of HIBEX of about 0.9 percentage points, statistically significant at the 10 percent level. This effect is relatively small, supporting the suggestion that the

[23] Results are robust, but lower in size, by using an alternative labor force participation definition of the variable, setting it equal to one if the individual worked positive hours or was unemployed during the last month, and zero otherwise (see Appendix A, Table A2, Column 1).

increase in full time employment attributed to the HIBEX was due to a change in the extensive margin rather than an increase in hours among those already working (intensive margin).[24] Moreover, the potential reduction in labor supply from the reform's income effect for individuals with children already in a registered full time job was negligible, or at least not large enough to cancel out the increase in hours of work from those moving to full time employment because of the reform.

Finally, column (8) in Table 3 presents the estimates of the effect of the HIBEX on the probability of compliance with SSA for working individuals – i.e., that their job is registered or "on" the books. As previously discussed, for eligible individuals at the margin of indifference between registered and non-registered jobs, the HIBEX represents an additional financial incentive which should encourage employment in jobs providing social insurance coverage for their dependent children. Before the reform, parents out of the labor force or in off-the-books jobs would pay for health care insurance (or out of pocket expenditures) for their children in the private market if they could afford it, or alternatively rely on the means tested public health sector. After the reform, parents in registered jobs benefit from a substantial *de facto* subsidy for their children's insurance premiums, which implies a clear disincentive to tax evasion in the form of off-the-books employment. The results in Table 3, column (8) confirm the relevance of this margin of adjustment. The reform significantly induced private sector workers with at least one child to work in a registered job, with an increase in registered employment of about 1.5 percentage points relative to the control group of childless individuals. In terms of the pre-intervention average, this effect represents a 2 percent increase in the rate of compliance with payroll taxes and social security regulations.

Taken together, the pattern of results in Table 3 indicates that eligible individuals responded to the health insurance benefit extension as predicted in Section 3. The response to an increased in-(registered)-work benefit resulted in higher levels of labor force participation in a manner consistent with the reform's eligibility rules, with effects along the extensive and intensive (increase in full-time work) margins of labor supply. While it cannot be formally tested in the data, the adjustments in hours appear to have occurred through job changes (off to on the books transitions) rather than hours changes within the same job, which is in line with the existing evidence on hours and benefit reforms for other countries (see Blundell et al., 2008). The increase in employment following the change in the tax-benefit system was reflected overwhelmingly in an increase in registered work, indicating the

[24] Estimates using hours worked (excluding zeros) as dependent variable shows positive effects but not statistically significant at standard levels, as reported in Appendix A, Column (3), Table A2. However, when participation effect is included by estimating the model on total hours (including zeros), the coefficient of impact increases from 0.05 to 0.52, and its effect results statistically significant at 1 percent level (see Appendix A, Column (2), Table A2). These estimates are consistent with the earlier finding that the increase in employment on individuals with children is mainly driven the effect on probability of working in 25 hours or more employment.

presence of joint labor supply and tax evasion responses. The results confirm the quantitative importance of evaluating incentive effects along both margins.

5.2 Heterogeneous responses

While the health insurance reform was not targeted to any particular socio-economic group, the labor supply literature indicates that changes in tax and benefit systems can have heterogeneous effects. The heterogeneity might be due to differences in the magnitude of the HIBEX financial incentive, because of different individual valuations of the benefit, or because of varying preferences over work. To reconstruct this puzzle, this section establishes whether the HIBEX had a differential impact by age, educational attainment, gender and marital status of the beneficiary. These estimates also allow a comparison with the behavioral responses to financial incentives for the same groups in the existing literature for developed countries.

The results from these exercises are presented in Table 4. The regression models are first re-estimated by age group, where the effects are ambiguous a priori. Younger individuals may place a lower value on the bundle of social insurance benefits than their older peers because they derive less utility from the pension component, will is available in a further date. This is not necessarily the case for the health insurance benefit. For younger individuals the reform might increase the value of this benefit since they are more likely than older individuals to have children in the age range covered by the benefit extension. For instance, while 88.8 percent of individuals in the sample aged 25 to 39 had children younger than 10 years (for those with at least one child), the figure is 46.1 percent for parents aged 40 to 55.

The results presented in column (1) of Panel A in Table 4 indicate a statistically significant increase of 2.7 percentage points in the employment rate of parents aged 25-39 with respect to the control group (childless individuals in the same age range), which represents an 8 percent increase with respect to the pre-reform average. For individuals aged 40-55, the estimates on employment (column (5)) are not significantly different from zero. A similar pattern emerges with the probability of full time work in columns (2) and (6). The estimates are not statistically significant at standard levels for hours of work (conditional on working) for both age groups. Finally, as with the overall population, the impact on employment for younger individuals is mainly driven by registered jobs, with a significant increase of 1.8 percentage points in registered work and a positive but not statistically significant effect for older individuals. However, the hypothesis that younger and older workers with children respond in the same way to the incentives to hold a registered job cannot be rejected (*p*-value: 0.454).

Panel B in Table 4 presents similar results by educational group. Individuals with higher formal education levels may be more affected by the HIBEX, since they have been found to have a higher valuation of social insurance programs than those with less education (Levy, 2008). Alternatively, these individuals might be more likely to be near the registered/not registered margin, because characteristics such as educational achievement can be correlated with the possibility of choosing between off and on the books jobs, while individuals with lower productive endowments may be sort in or rationed out of registered jobs (Boeri and Garibaldi, 2007; Fields, 2009; Gunther and Launov, 2012). The estimates in Panel B of Table 4 are consistent with these expectations. The reform implied a statistically significant increase of 2.1 percentage points on the probability of being in a registered job for eligible individuals with some college education (2.2 percent higher with respect to the pre-reform average). On the other hand, there was no statistically significant impact of the HIBEX on eligible individuals with high school or below, as detailed in columns (4)-(8). Additionally, as reported in columns (1)-(5), the reform had a significantly positive impact on employment irrespective of educational level (at the 10 percent level for those with some college, and at the 5 percent level for those in the lower education group), with a magnitude statistically indistinguishable between both groups (*p*-value: 0.313). The results also indicate a 4 percentage point increase in the probability of working full time for individuals with some college education, almost double the magnitude of the effect on employment for this group, and no impact on full time work for those with lower education levels. Moreover, individuals with children in the higher education group increased their weekly hours of work by about 3.7 percentage points after the reform (column (7), Panel B). This finding is consistent with the minimum hours of work requirement from the HIBEX, suggesting that the overall change in the employment pattern can be attributed to workers switching from off the books to registered jobs, which typically exhibit longer hours as described earlier.

With respect to marital status, the existing evidence suggests that the labor supply of single parents (and in particular single mothers) is more responsive to financial incentives than that of other groups (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Francesconi and van der Klauuw, 2007). To test for this possibility in the case of Uruguay, Panel C in Table 4 presents the estimates of the reform effects for single and married individuals separately. Eligible single parents are about 2.3 percentage points more likely to be employed after the introduction of the HIBEX, with a substantially lower effect of 1 percentage point for married individuals with children (statistically significant at the 5 and 10 percent levels, respectively). However, the hypothesis of equal employment response for both groups cannot be rejected, albeit by a low margin (*p*-value: 0.128). The effects on full time employment, weekly hours and registered work are positive and significant for single parents, but there are no significant effects for any of these outcomes for married parents.

The impact of the reform on both the extensive (2.3 percentage points increase, column (3)) and the intensive (4.3 percent increase in hours, column (4)) margins of single parent's labor supply seems to be due to the 5.1 percentage point increase in full time work. Similarly, the results indicate that the incidence of registered work increased by 6 percentage points, which represents a large 10 percent increase relative to the pre-reform average. The pattern of results is similar when focusing only on women (Table 4, Panel D), which is consistent with the fact that 90 percent of lone parents are single mothers. The increase in employment is larger for single mothers, although the difference in coefficients with respect to married women is not statistically significantly different from zero. The impact on all outcomes (except employment) is positive and statistically significant only for single women. Single mothers thus appear to adjust to the reform on both the extensive and intensive margins, with changes in employment mostly due to an increase in registered jobs. Finally, the results for single men (presented in Appendix A, Panel B, Table A4) are not statistically significant for any of the main outcomes with the exception of registered work (4.8 percentage points increase), and there are no significant effects at all for the full men sample (Appendix A, Panel A, Table A4).[25]

The labor supply and tax evasion behavioral responses for lone parents, and in particular single women, are consistent with the theoretical predictions outlined in Section 3. Married women did not reduce their labor force participation as a response to introduction of the HIBEX – the estimates actually indicate an increase in employment for this subgroup. A possible explanation for this finding is that women whose spouses were not eligible to the HIBEX and worked part time or in unregistered jobs could react to the new incentives and change their pattern of labor force participation (Fransesconi et al., 2009).[26] For registered work, there is a positive but not significant effect for married individuals and for the subgroup of married mothers. This may be due to the fact that the reform links the individual worker's registration status to the extended benefit. For married parents, there is an incentive to avoid the "double taxation" or overlap of payroll taxes related to registered employment, since the health coverage of the couple's children needs only one of the parents to comply with the SSA. Primary earners may have increased their registered employment to gain the additional health coverage, and at the same time secondary workers might have moved towards off-the-books jobs.[27]

This potential pattern of work re-allocation within the household may have resulted in a lack of significant effects on overall registered work among married parents in Table 4. Table 5 presents the

[25] There is however a pattern of results similar to that of the full sample when restricting the exercise to men aged 25-34 and for those with some college education (results available upon request).

[26] Although this would be an interesting issue to explore, the sample size becomes too small when the employment status of married women is conditioned to that of their partners.

[27] ECH data show that women in married-couple families are predominantly secondary earners. Roughly 84 percent of all wives, and 73 percent of employed wives, earn less than their spouses. Furthermore, only 10 percent of all married women with children self-report in the sample as the head of the household.

results of an exercise where the impact of the HIBEX on registered work is estimated for individuals in married couples (for married women and married men separately). The results in the first column for each subsample use the main specification used so far, while the second column presents the results with non-labor income as an additional control, although the estimates are similar in the two alternatives.[28] Among all married men, the point estimate indicates a statistically significant increase of 1.7 percentage points in registered work after the reform (Panel A, Table 5). On the contrary, for married women there is evidence of a reduction in registered work of about a half percentage point following the HIBEX, although this change is not statistically different from zero. Panels B and C in Table 5 report estimates conditional on the educational level of individuals in married couples. For the subgroup of workers with lower education (Panel B), there are no significant effects on registered work after the reform. On the other hand, there is a statistically significant response on this form of tax compliance for individuals with higher education in married couples, which can be expected from the previous evidence on the higher responsiveness of this group to the financial incentives introduced by the reform. As detailed in Panel C, and consistent with the within-household reallocation hypothesis, wives with higher education levels in couples with children are about 2.2 percentage points less likely to work in registered jobs after the HIBEX, while their spouses display an increase in registered work of 4.6 percentage points.

The response patterns in registered work for married couples are in line with the theoretical predictions discussed above, at least for the subgroup with higher education. They are also consistent with the existing evidence on the negative labor supply responses to financial incentives for secondary workers (Eissa and Hoynes, 2004). These results also confirm the importance of social insurance benefits for the pattern of informal sector employment of secondary workers in Latin American countries (Galiani and Weinschelbaum, 2011).

5.3 Treatment intensity: Estimates by age and number of children

The reform could have generated different labor market responses depending on the number and the age of the children benefited by the health insurance benefit extension. More children implies a larger financial incentive, since the increase in the payroll tax contribution was independent of the number of children who gained access to health insurance, while the SSA covered the premiums of all dependent children irrespective of their number. Moreover, younger children are associated with

[28] The non-labor income of the wife includes the husband's actual earnings. The assumption is that secondary workers make their labor supply decisions taking into account the primary earner's wage income and other household non-labor income, but the wife's work decisions do not affect those of their husbands (see Eissa and Hoynes, 2004).

higher insurance and out-of-pocket health expenditures, and thus might 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 Equation (1) is extended to allow for different treatment effects by the number of dependent children and by the age range of these children. The resulting specification is:

$$Y_{ijt} = \alpha + \sum_k \delta^{(k)} AnyChildre \quad n^{(k)}{}_{ijt} + \sum_k \beta^{(k)} AnyChildre \quad n^{(k)}{}_{ijt} * Post_t + X_{ijt}'\gamma + \delta_t + \varphi_j + \tau_{jt} + \varepsilon_{ijt} \quad (2)$$

where $AnyChildren^{(k)}{}_{ijt}$ 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 with the youngest aged 0-10; two or more children with the youngest aged 11-18. The matrix X_{ijt} includes the same covariates as in Equation (1) and the same set of state and time controls.

The results from regressions based on Equation (2) are presented in Table 6. The rows in this table report the estimates of the HIBEX impact for each number of children/age range group, $\beta^{(k)}$. The results are presented for the full-time employment and registered work dependent variables – the effects for employment is similar to that of full-time work, and there are no significant effects on log weekly hours.

The estimates for the overall sample in Panel A of Table 6 indicate that the response in full time work is concentrated among individuals with two or more children, irrespective of their age range, with a 2.2 percentage points statistically significant effect (column (1)). On the contrary, column (2) in Panel A indicates a positive and significant effect of the reform on registered work only for individuals with one dependent child, irrespective of the age range (the hypothesis of equal effects for those with one child aged 0-10 and 11-17 cannot be rejected – *p*-value of 0.484). Individuals with more dependent children have positive but smaller responses along the registered work dimension, especially for those with younger children, although the coefficients are not statistically different from zero.

Panel B in Table 6 presents these regression estimates restricting the sample to single women. Column (1) indicates an increase in full time work of about 8.5 percentage points for single mothers with one child aged 0-10 when compared to single childless women. This impact is roughly 4 percentage points for lone mothers with one child aged 11-17, although the difference in coefficients in not statistically significant at the usual levels (*p*-value: 0.249). The stronger effects for single mothers of young children are in line with the evidence on larger employment responses to financial

incentives for this type of women in the existing literature (for the case of EITC, see Eissa and Liebman, 1996 and Meyer and Rosembaum, 2001, and Francesconi and van der Klaauw, 2007, in the case of the WTFC). Column (2) in turn reports the results on registered work for single mothers. The coefficients are positive and sizeable, although not significantly different among them, indicating that the increase in registered work for single mothers does not depend strongly by the number and age of dependent children.

5.4 Wages and underreporting of salaried earnings

As discussed above, a strand of the labor literature studies the impact of in-work amenities, mandated benefits and public programs on wages. The analysis presented below attempts to capture potential effects of the health reform in Uruguay on salaried earnings. These estimates follow from a simple reduced form approach, and do not represent a general equilibrium analysis of the reform's implications. They are however consistent with the predicted effects and add to the interpretation of the type of labor market changes induced by the reform.

The HIBEX might be expected to reduce wages of eligible workers if they are willing to trade-off lower compensation levels for the extended benefit. Alternatively, the reform may be expected to increase the relative salaries of off-the-books workers, since their employers might have to compensate them for the opportunity cost of the additional benefit (Levy, 2008). Table 7 provides some evidence on the presence and size of these effects. The results correspond to difference-in-difference estimates of the effect of the HIBEX on wages, with the usual set of controls included for the previous outcomes.

For the full sample, the results in column (1) indicate a slight decrease in the wages of the treatment group relative to the control group, but these estimates are not statistically significant at the usual levels. Table 7 also presents results for registered and off-the-books workers separately. As anticipated in the previous paragraph, the reform reduced the wages of eligible registered workers with respect to those of childless individuals. The coefficient in column (3) indicates a fall in wages of about 1.8 percent. On the other hand, the HIBEX did not reduce the wages of eligible non-registered workers – the coefficient (columns (2)) is positive but small (about 0.7 percent) and not significant. The change in wages for registered workers might be due to working individuals switching from off-the-books to registered employment either within the existing job or after changing occupation, but in both cases it indicates a trade-off between the additional benefit and remuneration.

Finally, the analysis so far has covered payroll tax evasion and compliance with social security along the extensive margin – i.e., the decision whether to evade or not (Sandmo, 2011). It is still

possible, however, that tax and benefit policies have an effect along the intensive dimension of evasion – i.e., how much to evade in the context of partial evasion. For earnings, this would amount to concealing or under-reporting. The tax evasion literature has covered this dimension mostly in the case of self-employment, since under-reporting of salaried earnings is considered difficult, at least in the United States (Slemrod, 2007). However, recent evidence from Hungary (Tonin, 2011) indicates that changes in labor market regulations such as the minimum wage can have an impact on underreporting of salaried earnings, reflecting some degree of collusion between the employer and the employee.

While Tonin's (2011) evidence is indirect and relies on individual income and expenditure patterns, the ECH data contains a unique source of direct evidence on under-reporting of salaried earnings for the purpose of payroll tax evasion. As described in the data section, registered workers are asked whether the payroll taxes and contributions paid correspond to the full amount of their earnings. The reform granted a valuable benefit to eligible workers, and this additional benefit out-weighted the increase in the payroll tax for most workers. This difference might have induced some degree of collusion between employees and employers to underreport the workers' earnings to the SSA in order to lessen the higher payroll tax liability. Additionally, eligible individuals who were previously employed off-the-books might have more tolerance to tax evasion.

The results from this specific question in the survey confirm this effect. Column (4) of Table 7 presents the difference in difference estimates of the HIBEX impact on the probability of underreporting of earnings in a registered job to the SSA. The effect is positive and statistically significant – an increase in 0.8 percentage points in underreporting for eligible individuals after the reform with respect to the control group. Table 2 indicates an incidence of underreporting of about 10 percent over the period under study, so the HIBEX effect represents an 8 percent increase in this form of tax evasion. This evidence indicates that changes in the tax and benefit system can induce quantitatively substantial behavioral responses along the intensive margin of tax evasion besides their impact in the incidence of shadow employment.

6 Robustness tests: Falsification exercises and specification checks

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

As discussed earlier, the interpretation to the difference-in-difference estimates of the HIBEX impact as causal effects relies on a series of assumptions. The main identifying assumption is that the changes in outcomes would have been similar for eligible and non eligible individuals in the absence of the reform. While these counterfactuals are inherently non-testable, it is still possible to evaluate auxiliary hypothesis consistent with the assumption. One option is to consider the pre-reform period and establish whether there were common trends in the outcomes of interest, such that the reform represents a departure from the previous parallel changes. For instance, Uruguay's economy grew steadily during the period under study, and individuals with and without children might respond differently (even conditional on their observable characteristics) to episodes of growth. Hence, if employment was increasing faster for the treatment group with respect to the control before the reform, the HIBEX impact estimates would be capturing a spurious correlation rather than the effect of the program.

As it is usual in difference in difference studies, common pre-reform trends are tested by means of a falsification exercise. This consists of re-estimating the main model on a sample restricted to the pre-reform period (2004-2007) and testing the effects of placebo policies. For instance, observations for the years 2004 and 2005 can be labeled as the pre-reform period, and 2006 and 2007 as the post-reform period, with the interactions and other terms defined according to this 2006-2007 placebo treatment. The resulting estimate, if significant, would indicate systematic pre-reform differences in the trends of the underlying outcomes, casting doubts about the validity of the control group as a counterfactual. Table 8 presents the results of this exercise for the four main outcomes presented in Tables 3 to 6. The coefficient capturing the effect of the reform (the *AnyChildren*Post* interaction) is not statistically significant at standard levels for any of false experiments considered (varying the pre-post fictitious reforms years). These placebo estimates thus support the assumption that the treatment and control groups exhibited similar trends in the main outcomes (conditional on the observable control variables) before and after the reform.

Another threat to the identification strategy is that other changes contemporaneous to the reform under study might affect the treatment and control groups differently. The institutional background section detailed another substantial change in Uruguay's social insurance system – a substantial increase in the stipend and coverage of the AFAM ("asignaciones familiars") over the period under analysis. This means-tested program provides a monthly cash transfer for each dependent children of low income families. The maximum monthly benefit for an eligible family with two children under 18 years old was on average UYU 471 in 2007, and it increased to UYU 1220 in 2010 (in 2010 prices). The AFAM expansion might not be properly controlled for in the base specification. Panel A in Table 9 presents the results of an exercise that follows Eissa and Liebman (1996) and Hoynes (2012) in their strategies to control for contemporaneous reforms. The table

presents estimates similar to those of Equation (1), with the addition of interaction terms between a semester indicator and a measure of the monthly AFAM benefit corresponding to each family. The treatment effect (the coefficient on *AnyChildren*Postreform*) remains statistically significant for the main outcomes after controlling for time-varying AFAM benefits. The estimates appear larger than the baseline results in Table 3, which could be expected given the work disincentives introduced by the means test and the AFAM transfer (Moffitt, 2002). Moreover, the AFAM's means test implies that the authorities must verify and monitor household income levels. The increase in AFAM benefits may have generated additional incentives to work off-the-books and conceal labor income. The estimates of the HIBEX impact based on a sample limited to individuals ineligible for AFAM, however, confirms the main results of the paper (see Panel C, Table A3, Apendix A).

A further concern for identification is that the compositions of the treatment and control groups may have changed differently over the period, confounding treatment with composition effects. The summary statistics in Table 2 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 individuals with and without children have clear differences in their age distributions (see Figure 4), the trends in their labor supply may differ by cohort. Panel B in Table 9 presents the results from a specification that adds a full set of birth-cohort/semester interactions, based on 8 year cohorts. The main results are robust to the inclusion of these additional controls. The regressions are also estimated including interaction terms between the *AnyChildren* indicator and the set of demographic covariates, in order to allow control variables to enter separately for the treatment and the control groups. Results in Panel C indicate that the main results are also robust to this alternative. Finally, another specification includes interactions of all demographic controls with the post-2008 indicator, to explore if other covariates could explain the responses in labor market outcomes for individuals with children relative to those without children after 2008. The estimated coefficients for the main outcomes are somewhat smaller, but they remain significant at the usual confidence levels (Panel D, Table 9). The results from this set of tests suggests that changes in the composition of the treatment and control groups do not introduce a spurious correlation between the outcomes and the reform, and do not seem to drive the main result.

Panels E and F in Table 9 present additional specification checks and variations of the main estimates. When including in the sample observations from the reform year (2008), the results (Panel F) remain similar to the baseline estimates, with the exception of a non-significant effect on weekly hours. Panel G in turn shows that the main results remain unchanged when the regressions are estimated without the ECH sampling weights.

Finally, Table 10 presents similar robustness checks for the wages and earnings under-reporting outcomes for samples limited to registered (column (1)) and unregistered (column (2)) workers. As with Table 9, the main results are consistent with the baseline estimates for all but one of the alternatives (D) for wages, and the HIBEX effects on underreporting in column (3) remain statistically significant and positive for all specifications.[29]

7 Discussion and Conclusions

The main objective of this paper was to illustrate the relevance of joint behavioral responses on the labor supply and tax evasion margins. An extension of the coverage of health insurance for registered workers in Uruguay resulted in an increase in labor supply, as could be expected from the analysis of entitlement effects. The reform also generated responses in off-the-books employment and on the under-reporting of salaried earnings. In this setting, the relevant trade-offs for workers and governments become more complex. In terms of tax evasion, the decision is not simply between off-the-books versus registered work, but between full non-compliance, full compliance, and "on-the-books" 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 account for these additional dimensions.

These additional margins have relevant policy consequences that are not contemplated by the canonical model. For instance, social transfer programs may induce work disincentives, but in a setting with tax evasion they might instead end up encouraging off-the-books employment, with very different fiscal implications. Moreover, these issues are especially important for households' welfare and their ability to cope with shocks when social insurance benefits are tied to registered work. While the case study presented here involved an expansion of benefits, the underlying mechanisms are particularly relevant in the context of austerity initiatives and reductions of entitlements.

The implications for the analysis are not only relevant for developing economies with large informal labor forces. The policy consequences matter for advanced economies for at least two reasons. On the one hand, most OECD countries experienced an increase in their shadow economies over the past two decades (Schneider and Enste, 2000; Schneider, 2005), and thus face similar types of trade-offs as the one described here for Uruguay, at least for a subgroup of their labor forces. For instance, Potter Gunter (2010) finds that in the United States higher levels of in-work benefits result in an increase in registered work and a fall in off-the-books hours. On the other hand, the registered/non registered jobs dichotomy discussed here can be applied, mutatis mutandi, to the

[29] The falsification test for this outcomes were not performed because the ECH survey started to collect information on underreporting only in 2006, which does not provide a wide enough time frame for the placebo experiments.

context of dual labor markets in developed countries, with increasing shares of employment under fixed-term contracts and of jobs exempted from social security benefits (Boeri, 2011). Temporary contracts and reduced entitlements are not illegal, but they induce in advanced economies similar types of trade-offs as those discussed here, for instance for the reform of social insurance systems and for employment protection legislation. While most employers might not be willing to engage in outright evasion in developed countries, they can still strategically accommodate their hiring patterns to seize the opportunities offered by employment regulations to minimize their costs. Recent work indicates that Hawaii's employers, who face a health insurance mandate for full time permanent workers, are substantially more likely than those in other states to rely on part-time workers who are excluded from the benefit (Buchmueller et al., 2011).

Moreover, the original evidence on under-reporting of salaried earnings is directly related to recent contributions on the elasticity of taxable income for high earnings individuals (Chetty, 2009; Piketty, Saez and Stantcheva, 2011). This literature studies how better-off workers are able to avoid taxes and bargain over the form of their compensation, and establishes some powerful results on optimal taxation when considering these additional margins. The results presented here imply that lower income workers might also engage in tax avoidance (in fact, tax evasion in the form of under-reporting) and on bargaining over the form of compensation and their type of labor contract. Further research could study similar effects in developed countries, and establish their implications for optimal taxation of the low and middle brackets.

The growing series of studies at the intersection of tax evasion/shadow employment and labor supply responses to tax and benefit systems would also benefit from more theoretical work. For instance, Boeri and Garibaldi's (2007) search model could be extended to incorporate under-reporting and the allocation of hours between on and off the books jobs for each individual worker. Models of this type could also make a link between the standard labor supply literature and the work on labor informality in developing countries.

Finally, the results in this study would greatly benefit from additional evidence derived from longitudinal data on workers' actual transitions between different forms of employment, unemployment and inactivity. These additional results would shed light on the allocation of workers and on the wage and work conditions bargaining between employers and employees.

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Tables

Table 1
Payroll tax contributions for social insurance benefits before (2007) and after (2008) the health insurance benefit extension

	Payroll tax contribution (% of salary)								
	Employee			Employer			Total		
	2007	2008	Difference	2007	2008	Difference	2007	2008	Difference
A. Retirement	15	15	0	7.5	7.5	0	22.5	22.5	0
B. FRL	0.125	0.125	0	0.125	0.125	0	0.25	0.25	0
C. Health Insurance									
C.1 salary Less than 2.5 times BPC									
C.1.1 With children	3	3	0	5	5	0	8	8	0
C.1.2 Without children	3	3	0	5	5	0	8	8	0
C.2 Salary more than 2.5 times BPC									
C.2.1 With children	3	6	3	5	5	0	8	11	3
C.2.2 Without children	3	4.5	1.5	5	5	0	8	9.5	1.5
D.1 Total (A+B+C.1.1)	18.125	18.125	0	12.625	12.625	0	30.75	30.75	0
D.2 Total (A+B+C.1.2)	18.125	18.125	0	12.625	12.625	0	30.75	30.75	0
D.3 Total (A+B+C.2.1)	18.125	21.125	3	12.625	12.625	0	30.75	33.75	3
D.4 Total (A+B+C.2.2)	18.125	19.625	1.5	12.625	12.625	0	30.75	32.25	1.5

Notes: “FRL” indicates “Fondo de Reconversión Laboral”. The “Health Insurance” payroll tax includes contribution for both sickness benefit and health insurance coverage. “BPC” indicates “Base de Prestaciones y Contribuciones” and it was equal to UYU 1,775 in 2008 (USD 112 at the 2010 PPP adjusted exchange rate). The amounts of payroll contributions correspond to december 2007 and 2008, respectively.

Table 2
Summary statistics. ECH 2004-2010

	Individuals without children				Individuals with children < 18			
	Pre		Post		Pre		Post	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
A. Labor market outcomes								
Employment	0.79	0.41	0.84	0.36	0.67	0.47	0.75	0.43
Dummy for weekly hours ≥ 25	0.69	0.46	0.75	0.43	0.57	0.50	0.64	0.48
Hours worked per week	41.85	14.01	41.80	12.90	41.66	15.88	41.54	14.28
ln(weekly hours)	3.65	0.49	3.66	0.45	3.61	0.58	3.63	0.53
Registered employment	0.80	0.40	0.86	0.35	0.76	0.43	0.83	0.38
Wage per hour	67.18	79.71	80.50	73.52	64.27	79.86	75.98	84.68
ln(hourly wages)	3.90	0.74	4.14	0.69	3.81	0.79	4.03	0.73
Underreporting earnings to SSI	0.10	0.30	0.09	0.28	0.10	0.30	0.10	0.30
B. Background Characteristics								
Age	39.66	9.93	39.67	9.91	38.49	7.62	38.59	7.56
Years of education	10.34	3.95	10.48	3.88	9.31	3.50	9.31	3.43
High school or less	0.72	0.45	0.73	0.44	0.85	0.35	0.86	0.35
Some college or more	0.28	0.45	0.27	0.44	0.15	0.35	0.14	0.35
Male	0.48	0.50	0.49	0.50	0.39	0.49	0.40	0.49
Head	0.63	0.48	0.65	0.48	0.50	0.50	0.52	0.50
Married	0.65	0.48	0.63	0.48	0.89	0.32	0.87	0.33
# children ages 0 - 10	0	0	0	0	1.12	0.99	1.07	0.96
# children ages 11 - 17	0	0	0	0	0.80	0.83	0.82	0.85
# children > 18	0	0	0	0	0.28	0.60	0.26	0.57
Total number of children	0	0	0	0	2.20	1.13	2.15	1.10
Region								
Montevideo	0.71	0.46	0.66	0.48	0.59	0.49	0.55	0.50
North	0.06	0.24	0.06	0.23	0.10	0.30	0.09	0.29
Centre - North	0.07	0.25	0.08	0.27	0.10	0.30	0.11	0.31
Centre - South	0.05	0.23	0.06	0.23	0.07	0.26	0.07	0.26
South	0.11	0.32	0.15	0.36	0.13	0.34	0.18	0.38
Firm size								
1 emp	0.11	0.31	0.08	0.27	0.14	0.35	0.11	0.31
2-4 emps	0.17	0.37	0.15	0.35	0.17	0.37	0.15	0.36
5-9 emps	0.14	0.34	0.12	0.32	0.13	0.34	0.13	0.33
10-49 emps	0.22	0.42	0.24	0.43	0.22	0.42	0.23	0.42
> 49 emps	0.36	0.48	0.42	0.49	0.34	0.47	0.38	0.49
Industry								
Agriculture	0.04	0.20	0.04	0.20	0.06	0.24	0.06	0.24
Industry	0.10	0.30	0.09	0.28	0.11	0.32	0.11	0.31
Manufacturing	0.07	0.26	0.08	0.28	0.08	0.26	0.08	0.26
Construction	0.06	0.23	0.06	0.24	0.07	0.26	0.08	0.28
Trade	0.23	0.42	0.23	0.42	0.20	0.40	0.22	0.41
Transport/commun	0.07	0.26	0.09	0.28	0.08	0.27	0.08	0.27
Finance/professional	0.11	0.32	0.12	0.32	0.08	0.26	0.07	0.26
Education/health	0.19	0.40	0.18	0.39	0.17	0.38	0.17	0.37
Personal	0.12	0.32	0.11	0.31	0.15	0.36	0.14	0.35

Notes: SD denotes the standard deviation. "Pre" refers to the period from 2nd semester of 2004 to 2nd semester 2007. "Post" denotes the period from 1st semester of 2009 to 2nd semester of 2010. # children indicates the number of children by range of age. Data are from survey years 2004-2007 and 2009-2010 of the *Encuesta Continua de Hogares* (ECH). Sample contains individuals age 25-55 for employment and weekly hours ≥ 25 analysis (97552 observations). Sample contains salaried private sector workers age 25-55 for registered employment and weekly hours analysis (71147 observations). Sample contains salaried private sector workers age 25-55, with hourly wages greater than $ 1 (in July 2010 local currency units) for hourly wages analysis (68374 observations). Sample contains salaried private sector workers in registered jobs age 25-55 for underreporting earnings analysis (50669 observations) (see sample construction in the text). Means are weighted with ECH supplement weights.

Table 3
Effect of the health insurance benefit extension on labor supply and tax evasion outcomes. Difference in difference estimates.

	Sample: all individuals, age 25-55				Sample: private sector workers, age 25-55			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment		Weekly hours ≥ 25		Ln (weekly hours)		Registered employment	
AnyChildren*Postreform	0.0152***	0.0151***	0.0150***	0.0159***	0.0097*	0.0090*	0.0123**	0.0150**
	[0.0030]	[0.0033]	[0.0026]	[0.0028]	[0.0048]	[0.0050]	[0.0058]	[0.0060]
AnyChildren	0.0106*	0.0110*	-0.0007	-0.0006	-0.0075	-0.006	0.0171***	0.0168***
	[0.0053]	[0.0054]	[0.0064]	[0.0062]	[0.0096]	[0.0093]	[0.0045]	[0.0042]
Dummies Time and State	No	Yes	No	Yes	No	Yes	No	Yes
Observations	97552	97552	97552	97552	71147	71147	71147	71147
R^2	0.24	0.24	0.28	0.28	0.29	0.29	0.3	0.3
Dependent variable mean	0.67		0.57		41.66		0.76	

Notes: Sample contains individuals age 25-55 (salaried private sector workers for hours per week/registered employment, all individuals for employment/weekly hours ≥ 25) from survey 2nd semester of 2004 to 2nd semester 2007 and 1st semester of 2009 to 2nd semester of 2010 of the *Encuesta Continua de Hogares* (ECH). Coefficients estimates are taken from a regression as described in Equation (1). *Children* equals 1 if the individual has at least one child younger than 18 year old. *Post* equals 1 for years 2009-2010. Regressions are linear probability models for employment, weekly hours ≥ 25 and registered employment, and log-linear for weekly hours. Controls include variables for number of children by age group (3 groups); 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), industry (9) categories (excluded from employment and weekly hours ≥ 25 regressions) and time (11 categories); and interactions between gender and marital status, and time and region of residence (unless otherwise indicated). Robust standard errors in brackets are clustered on children and semester. Regressions are weighted with ECH supplement weights. Reported means are for the regression universe over the pre-reform period (2004-2007).
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4
Effect of the health insurance benefit extension on labor supply and tax evasion outcomes by demographic group. Difference in difference estimates.

Panel A. By age group

	Individuals aged 25-39				Individuals aged 40-55			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employment	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employment
AnyChildren*Postreform	0.0266***	0.0289***	0.0082	0.0186***	0.0038	0.0014	0.0056	0.0103
	[0.0040]	[0.0043]	[0.0066]	[0.0063]	[0.0058]	[0.0055]	[0.0090]	[0.0095]
AnyChildren	-0.0003	-0.0187**	0.0073	0.0192***	0.0470***	0.0382***	-0.0075	0.0263***
	[0.0063]	[0.0070]	[0.0096]	[0.0056]	[0.0072]	[0.0087]	[0.0154]	[0.0082]
P-value [25-39] = [40-55]					0.0018***	0.0026***	0.8234	0.4541
Observations	53451	53451	39208	39208	44101	44101	31939	31939
R^2	0.27	0.3	0.31	0.31	0.22	0.26	0.29	0.31
Dependent variable mean	0.59	0.50	41.99	0.7	0.68	0.58	41.82	0.75

Panel B. By educational level

	High school or less				Some college or more			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employment	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employment
AnyChildren*Postreform	0.0081*	0.0025	0.0024	0.0049	0.0167**	0.0402***	0.0367***	0.0212***
	[0.0043]	[0.0026]	[0.0066]	[0.0071]	[0.0066]	[0.0093]	[0.0103]	[0.0051]
AnyChildren	0.0152**	0.0084	-0.0063	0.0257***	0.0051	-0.0236	-0.0306**	-0.0096
	[0.0066]	[0.0066]	[0.0113]	[0.0052]	[0.0124]	[0.0152]	[0.0141]	[0.0084]
P-value High =college					0.3131	0.0006***	0.0181**	0.0378**
Observations	81117	81117	57039	57039	16435	16435	14108	14108
R^2	0.25	0.30	0.31	0.30	0.09	0.11	0.22	0.15
Dependent variable mean	0.65	0.54	42.21	0.72	0.82	0.69	39.12	0.92

Table 4
(continued)

Panel C. By marital status

	Single				Married			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employment	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employmen
AnyChildren*Postreform	0.0228***	0.0511***	0.0434***	0.0605***	0.0092*	0.0038	0.0004	0.0075
	[0.0055]	[0.0064]	[0.0117]	[0.0113]	[0.0049]	[0.0033]	[0.0071]	[0.0059]
AnyChildren	0.0588***	0.0157	-0.0383	0.0060	0.0015	-0.0014	0.0016	0.0229***
	[0.0102]	[0.0174]	[0.0278]	[0.0150]	[0.0058]	[0.0059]	[0.0090]	[0.0046]
P-value single = married					0.128	0.0000***	0.0083***	0.0002***
Observations	17323	17323	14106	14106	80229	80229	57041	57041
R^2	0.11	0.14	0.26	0.38	0.26	0.31	0.31	0.28
Dependent variable mean	0.74	0.54	36.20	0.60	0.67	0.57	42.44	0.78

Panel D. By marital status of women

	Single women				Married women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employment	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employmen
AnyChildren*Postreform	0.0278***	0.0700***	0.0673***	0.0842***	0.0166*	0.0066	0.0133	0.0102
	[0.0062]	[0.0057]	[0.0132]	[0.0127]	[0.0086]	[0.0080]	[0.0129]	[0.0083]
AnyChildren	0.0431***	-0.0103	-0.0582*	-0.0093	-0.0229**	-0.0417***	-0.0174	0.0257***
	[0.0123]	[0.0171]	[0.0291]	[0.0165]	[0.0095]	[0.0105]	[0.0158]	[0.0082]
P-value single = married women					0.3614	0.0000***	0.0142**	0.0001***
Observations	12005	12005	9352	9352	45128	45128	24147	24147
R^2	0.12	0.13	0.25	0.41	0.13	0.13	0.25	0.33
Dependent variable mean	0.721	0.513	35.212	0.592	0.469	0.336	34.132	0.719

Notes: Sample contains individuals age 25-55 (salaried private sector workers for hours per week/registered employment, all individuals for employment/weekly hours ≥ 25) from survey 2nd semester of 2004 to 2nd semester 2007 and 1st semester of 2009 to 2nd semester of 2010 of the *Encuesta Continua de Hogares* (ECH). Coefficients estimates are taken from a regression as described in Equation (1). *Children* equals 1 if the individual has at least one child younger than 18 year old. *Post* equals 1 for years 2009-2010. Regressions are linear probability models for employment, weekly hours ≥ 25 and registered employment, and log-linear for weekly hours. Controls include variables for number of children by age group (3 groups); 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), industry (9) categories (excluded from employment and weekly hours ≥ 25 regressions) and time (11 categories); and interactions between gender and marital status, and time and region of residence. Robust standard errors in brackets are clustered on children and semester. Regressions are weighted with ECH supplement weights. "P-value" is a *t*-test of the equality of the Children*Post coefficients on the two populations. Reported means are for the regression universe over the pre-reform period (2004-2007).
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5
Effect of the health insurance benefit extension on registered employment of married couples. Difference in difference estimates.

	Sample: private sector workers, age 25-55			
	Married women		Married men	
	(1)	(2)	(3)	(4)
Panel A. Total sample				
AnyChildren*Postreform	-0.004	-0.0043	0.0170**	0.0173**
	[0.0103]	[0.0103]	[0.0079]	[0.0078]
AnyChildren	0.0270**	0.0269**	0.0156**	0.0156**
	[0.0098]	[0.0098]	[0.0073]	[0.0072]
Non-labor income	No	Yes	No	Yes
Observations	13288	13288	22176	22176
R^2	0.34	0.34	0.24	0.24
Dependent variable mean	0.73	0.73	0.82	0.82
Panel B. Low education sample				
AnyChildren*Postreform	-0.0114	-0.0119	0.0037	0.0042
	[0.0143]	[0.0142]	[0.0118]	[0.0119]
AnyChildren	0.0167	0.0168	0.0248**	0.0240**
	[0.0139]	[0.0139]	[0.0113]	[0.0113]
Non-labor income	No	Yes	No	Yes
Observations	10141	10141	19155	19155
R^2	0.33	0.33	0.25	0.25
Dependent variable mean	0.67	0.67	0.8	0.8
Panel C. high education sample				
AnyChildren*Postreform	-0.0224*	-0.0219*	0.0458***	0.0459***
	[0.0113]	[0.0116]	[0.0133]	[0.0134]
AnyChildren	0.0101	0.0093	-0.0199	-0.0197
	[0.0113]	[0.0114]	[0.0193]	[0.0194]
Non-labor income	No	Yes	No	Yes
Observations	3147	3147	3021	3021
R^2	0.24	0.24	0.14	0.14
Dependent variable mean	0.93	0.93	0.93	0.93

Notes: Sample contains married couples age 25-55 from from survey 2nd semester of 2004 to 2nd semester 2007 and 1st semester of 2009 to 2nd semester of 2010 of the *Encuesta Continua de Hogares* (ECH). The regression universe in "Total sample" is restricted to salaried private sector workers. The universe in "low education sample" is restricted to salaried private sector workers with high school educated or less. The universe in "high education sample" is restricted to those with some college, college educated and more. Coefficients estimates are taken from a regression as described in Equation (1). *Children* equals 1 if the individual has at least one child younger than 18 year old. *Post* equals 1 for years 2009-2010. Regressions are linear probability models for registered employment. Controls include variables for number of children by age group (3 groups) and non-labor income (unless otherwise indicated); dummy variables for: age (6 categories), gender, head of the household status, education (6 categories), region of residence (19 categories), firm size (5), industry (9) categories and time (11 categories); and interactions between time and region of residence. Robust standard errors in brackets are clustered on children and semester. Regressions are weighted with ECH supplement weights. . Reported means are for the regression universe over the pre-reform period (2004-2007).
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6
Effect of the health insurance benefit extension on labor supply and tax evasion outcomes by age and number of children. Difference in difference estimates.

	Sample: all individuals, age 25-55	Sample: private sector workers, age 25-55
	(1)	(2)
Estimates by number and age of children	Weekly hours ≥ 25	Registered employment
Panel A. Total sample		
One child aged 0-10	0.0129	0.0226**
	[0.0081]	[0.0097]
One child aged 11-17	0.0025	0.0147*
	[0.0086]	[0.0077]
Two children or more,	0.0219***	0.0117
youngest aged 0-10	[0.0055]	[0.0069]
Two children or more,	0.0220**	0.0108
youngest aged 11-17	[0.0092]	[0.0132]
Observations	97552	71147
R^2	0.28	0.3
Panel B. Single mothers		
One child aged 0-10	0.0856***	0.0796***
	[0.0289]	[0.0229]
One child aged 11-17	0.0379**	0.0701***
	[0.0173]	[0.0189]
Two children or more,	0.0826***	0.1025***
youngest aged 0-10	[0.0154]	[0.0253]
Two children or more,	0.0882**	0.0819***
youngest aged 11-17	[0.0325]	[0.0259]
Observations	12005	9352
R^2	0.13	0.41

Notes: Sample contains individuals age 25-55 (salaried private sector workers for compliance with SSI) from survey 2nd semester of 2004 to 2nd semester 2007 and 1st semester of 2009 to 2nd semester of 2010 of the *Encuesta Continua de Hogares* (ECH). Rows reports coefficients estimates for each number–by–age range of children group which are taken from a regression as described in Equation (2). Regressions are linear probability models for employment, weekly hours ≥ 25 and registered employment, and log-linear for weekly hours. Controls include variables for number of children by age group (3 groups); 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), industry (9) categories (excluded from employment and weekly hours ≥ 25 regressions) and time (11 categories); and interactions between gender and marital status, and time and region of residence (unless otherwise indicated). Robust standard errors in brackets are clustered on children and semester. Regressions are weighted with ECH supplement weights. Reported means are for the regression universe over the pre-reform period (2004-2007).
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7

Effect of the health insurance benefit extension on wages and under-reporting of salaried earnings, overall and by job registration status (private sector workers, age 25-55). Difference in difference estimates.

	Ln (hourly wage)			Pr (undereporting earnings)
	(1)	(2)	(3)	(4)
	All workers	Unregistered employment	Registered employment	Registered employment
AnyChildren*Postreform	-0.0134	0.0068	-0.0176*	0.0080**
	[0.0096]	[0.0220]	[0.0098]	[0.0037]
AnyChildren	0.0575***	0.0546**	0.0478***	-0.0110**
	[0.0108]	[0.0207]	[0.0128]	[0.0046]
Observations	68374	13438	54936	50669
R^2	0.41	0.23	0.41	0.06

Notes: Sample contains salaried private sector workers age 25-55 from survey 2nd semester of 2004 to 2nd semester 2007 and 1st semester of 2009 to 2nd semester of 2010 of the *Encuesta Continua de Hogares* (ECH). Coefficients estimates are taken from a regression as described in Equation (1). *Children* equals 1 if the individual has at least one child younger than 18 year old. *Post* equals 1 for years 2009-2010. Regressions are linear probability models for underreporting salary earnings and log-linear for hourly wage. Controls include variables for number of children by age group (3 groups); 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), industry (9) categories and time (11 categories); and interactions between gender and marital status, and time and region of residence. Robust standard errors in brackets are clustered on children and semester. Regressions are weighted with ECH supplement weights. In hourly wage analysis 2753 were dropped because their hourly wage is lower than $1 (in 2010 UYU).

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8
Falsification test: comparing labor supply response in individuals with and without children before the HIBEX reform. "Pre"= 2004-2005, "Post"= 2006-2007.

	Sample: all individuals, age 25-55		Sample: private sector workers, age 25-55	
	(1)	(2)	(3)	(4)
	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employment
AnyChildren*Postreform	0.0086	0.0019	-0.0032	0.007
	[0.0055]	[0.0040]	[0.0096]	[0.0049]
AnyChildren	-0.2038***	-0.2064***	-0.1146**	-0.1838***
	[0.0231]	[0.0123]	[0.0393]	[0.0299]
Observations	59375	59375	41636	41636
R^2	0.25	0.29	0.3	0.31

Notes: Sample contains individuals age 25-55 (salaried private sector workers for hours per week/registered employment, all individuals for employment/weekly hours ≥ 25) from survey 2nd semester of 2004 to 2nd semester 2007 of the *Encuesta Continua de Hogares* (ECH). Coefficients estimates are taken from a regression as described in Equation (1). *Children* equals 1 if the individual has at least one child younger than 18 year old. *Post* equals 1 for years 2006-2007. Regressions are linear probability models for employment, weekly hours ≥ 25 and registered employment, and log-linear for weekly hours. Controls include variables for number of children by age group (3 groups); 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), industry (9) categories (excluded from employment and weekly hours ≥ 25 regressions) and time (11 categories); and interactions between gender and marital status, and time and region of residence (unless otherwise indicated). Robust standard errors in brackets are clustered on children and semester. Regressions are weighted with ECH supplement weights.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9
Effect of the health insurance benefit extension on labor supply and tax evasion outcomes. Further speciation checks.

	Sample: all individuals, age 25-55		Sample: private sector workers, age 25-55	
	(1)	(2)	(3)	(4)
	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employment
Panel A. Add monthly AFAM benefit				
AnyChildren*Postreform	0.0287***	0.0336***	0.0102**	0.0571***
	[0.0039]	[0.0033]	[0.0045]	[0.0063]
Observations	97552	97552	71147	71147
Panel B. Add time- cohort effect				
AnyChildren*Postreform	0.0169***	0.0145***	0.0113**	0.0175***
	[0.0031]	[0.0035]	[0.0053]	[0.0056]
Observations	97552	97552	71147	71147
Panel C. Covariates-children interactions				
AnyChildren*Postreform	0.0126***	0.0145***	0.0113**	0.0166**
	[0.0036]	[0.0026]	[0.0047]	[0.0060]
Observations	97552	97552	71147	71147
Panel D. Post-2006 interactions				
AnyChildren*Postreform	0.0094**	0.0090***	0.0131*	0.0170***
	[0.0039]	[0.0026]	[0.0071]	[0.0057]
Observations	97552	97552	71147	71147
Panel E. Including 2008				
AnyChildren*Postreform	0.0105**	0.0094**	0.0041	0.0117**
	[0.0039]	[0.0043]	[0.0055]	[0.0051]
Observations	117001	117001	85749	85749
Panel F. Unweighted				
AnyChildren*Postreform	0.0133***	0.0143***	0.0134**	0.0107*
	[0.0023]	[0.0025]	[0.0053]	[0.0059]
Observations	97552	97552	71147	71147

Notes: Sample contains individuals age 25-55 (salaried private sector workers for hours per week/registered employment, all individuals for employment/weekly hours ≥ 25) from survey 2nd semester of 2004 to 2nd semester 2007 and 1st semester of 2009 to 2nd semester of 2010 of the *Encuesta Continua de Hogares* (ECH). Coefficients estimates are taken from a regression as described in Equation (1). *Children* equals 1 if the individual has at least one child younger than 18 year old. *Post* equals 1 for years 2009-2010 (2008-2010 for Panel E). Regressions are linear probability models for employment, weekly hours ≥ 25 and registered employment, and log-linear for weekly hours. Controls include variables for number of children by age group (3 groups); 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), industry (9) categories (excluded from employment and weekly hours ≥ 25 regressions) and time (11 categories); and interactions between gender and marital status, and time and region of residence (unless otherwise indicated). Panel A reports results with interactions of a monthly family AFAM benefit variable with a full set of semester-dummies.. Panel B reports results with a full set of birth cohort-semester interactions. Panel C reports results with *Children* indicator interacted with demographic controls. Panel D reports with *Children* and *Post* indicators interacted with demographic controls. Panel E change the universe to include year 2006. Robust standard errors in brackets are clustered on children and semester. Regressions are weighted with ECH supplement weights (except for Panel F).
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10
Effect of the health insurance benefit extension on wages and under-reporting of salaried earnings by job registration status (private sector workers, age 25-55). Specification checks

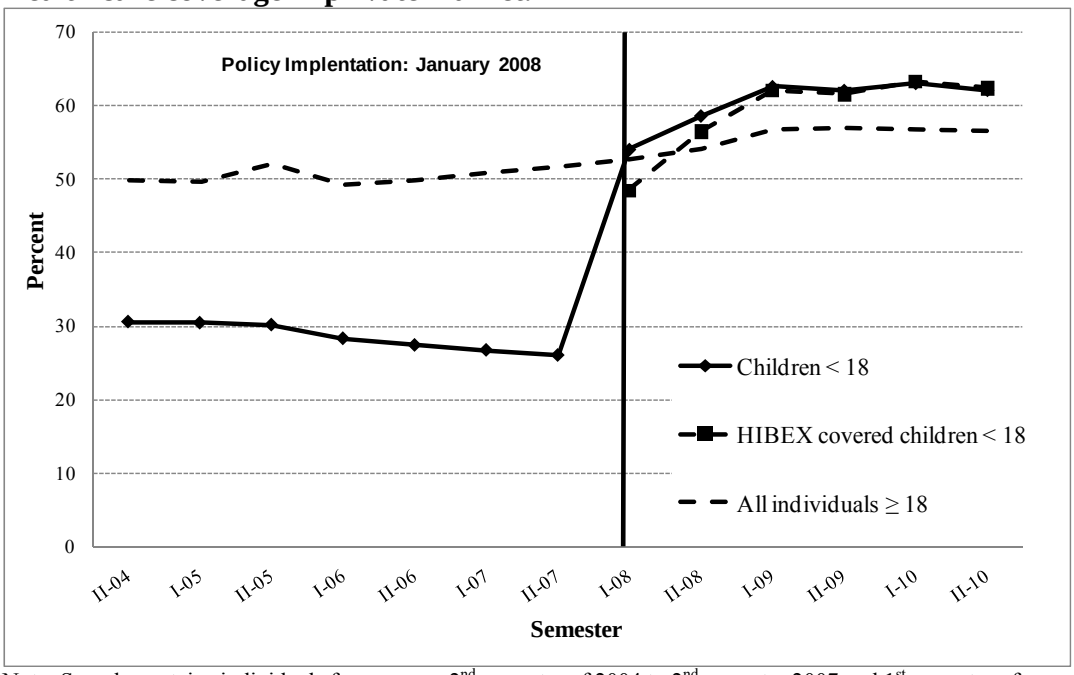
	Ln (hourly wages)		Pr (undereporting earnings)
	(1)	(2)	(3)
	Unregistered employment	Registered employment	Registered employment
Panle A. False experiment (period 2004-2007)			
AnyChildren*Postreform	0.009	-0.0105	--
	[0.0247]	[0.0122]	
AnyChildren	0.0667**	0.0365**	--
	[0.0239]	[0.0168]	
Observations	9016	31172	--
R^2	0.23	0.4	--
Panel B. Add monthly AFAM benefit			
AnyChildren*Postreform	0.0173	-0.0401***	0.0066**
	[0.0187]	[0.0097]	[0.0028]
Observations	13438	54938	50669
Panel C. Add time- cohort effect			
AnyChildren*Postreform	-0.0166	-0.0202*	0.0073**
	[0.0193]	[0.0112]	[0.0034]
Observations	13438	54938	50669
Panel D. Covariates-children interactions			
AnyChildren*Postreform	-0.0005	-0.0112	0.0067*
	[0.0225]	[0.0093]	[0.0036]
Observations	13438	54938	50669
Panel E. Including 2008			
AnyChildren*Postreform	0.0068	-0.0184*	0.0080**
	[0.0220]	[0.0097]	[0.0037]
Observations	13438	54938	50669

Notes: Sample contains salaried private sector workers age 25-55 from survey 2nd semester of 2004 to 2nd semester 2007 and 1st semester of 2009 to 2nd semester of 2010 (except for Panel A) of the *Encuesta Continua de Hogares* (ECH). Coefficients estimates are taken from a regression as described in Equation (1). *Children* equals 1 if the individual has at least one child younger than 18 year old. *Post* equals 1 for years 2009-2010. Regressions are linear probability models for underreporting salary earnings and log-linear for hourly wage. Controls include variables for number of children by age group (3 groups); 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), industry (9) categories and time (11 categories); and interactions between gender and marital status, and time and region of residence. Panel A presents results for a falsification exercise by using pre-reform data (see notes in Table 8). Panel B reports results with interactions of a monthly family AFAM benefit variable with a full set of semester-dummies. Panel C reports results with a full set of birth cohort-semester interactions. Panel D reports results with *Children* indicator interacted with demographic controls. Panel E reports with *Children* and *Post* indicators interacted with demographic controls. Panel E change the universe to include year 2006. Robust standard errors in brackets are clustered on children and semester. Regressions are weighted with ECH supplement weights

* significant at 10%; ** significant at 5%; *** significant at 1%.

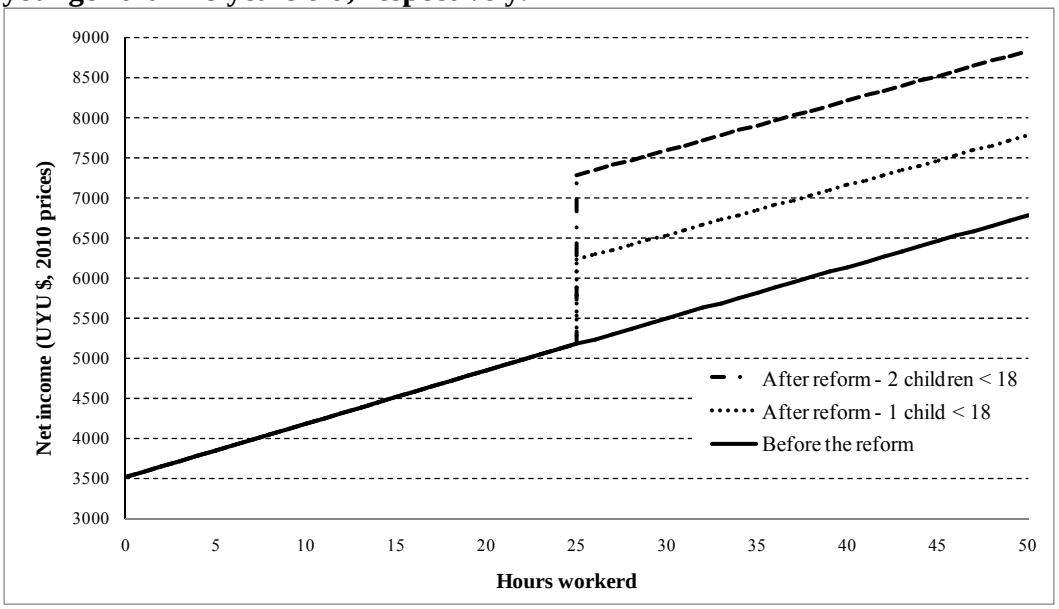
Figures

Figure 1
Health care coverage in private market.



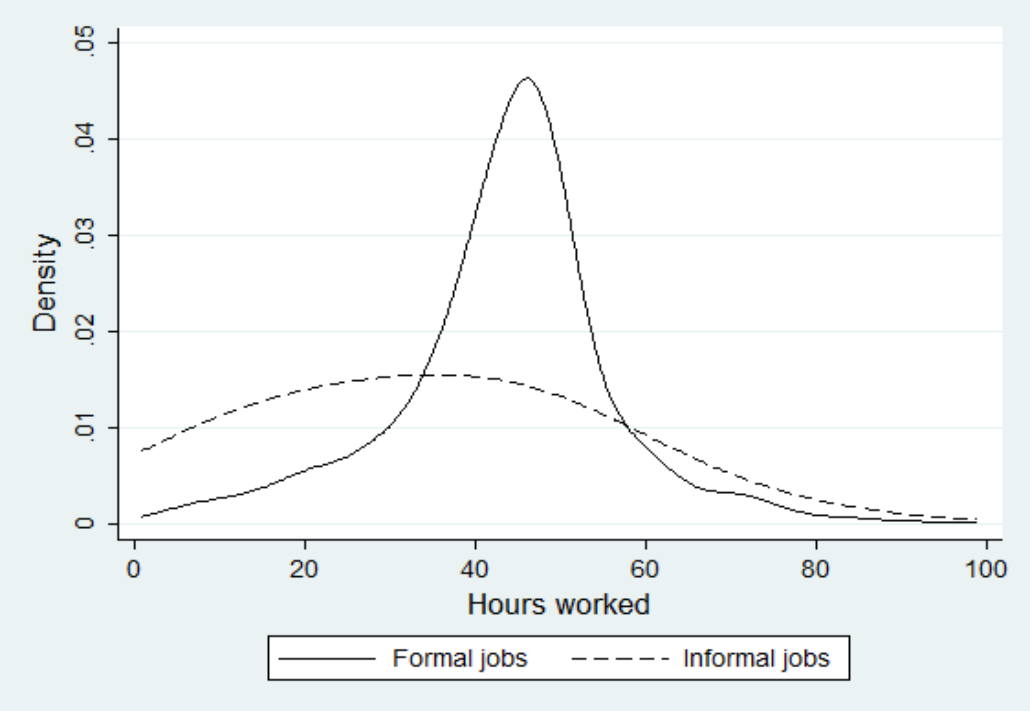
Note: Sample contains individuals from survey 2nd semester of 2004 to 2nd semester 2007 and 1st semester of 2009 to 2nd semester of 2010 of the *Encuesta Continua de Hogares* (ECH)).

Figure 2
Change in budget constraint after the reform for a lone parent with one or two children younger than 18 years old, respectively.



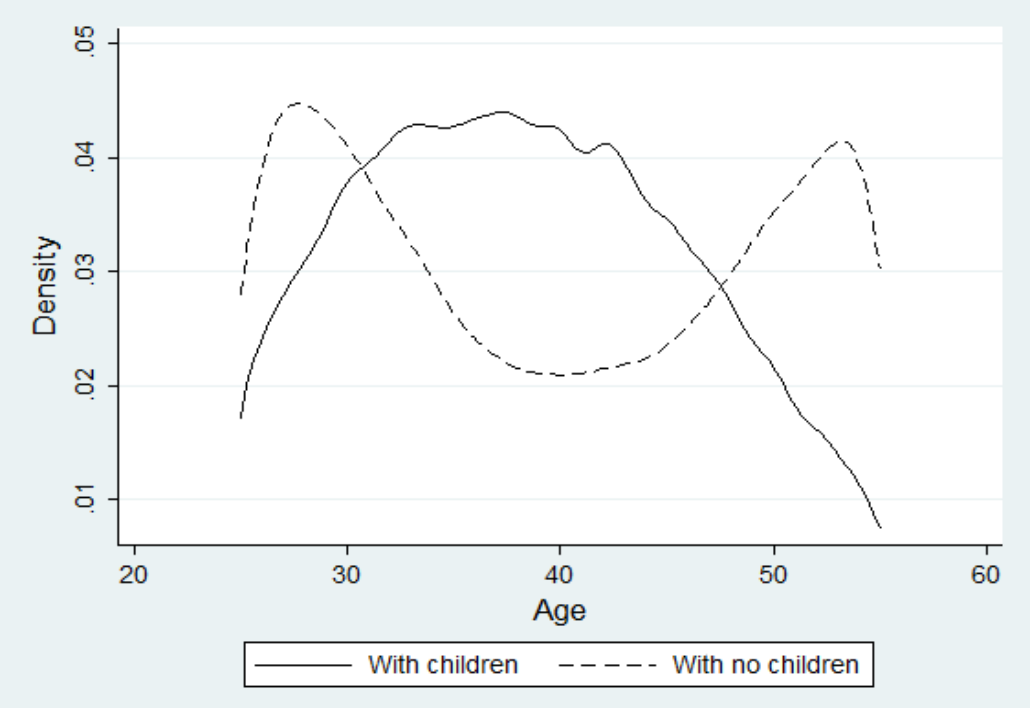
Note: Assumes hourly wage of UYU 78.6, non-labor income of UYU 3,500 and the cost of a health insurance plan in the private market in 2007 of UYU 1,030 (in 2010 prices).

Figure 3
Hours of work distributions for employed individuals, registered and off-the-books jobs



Notes: Sample contains salaried private sector workers age 25-55 from survey 2nd semester of 2004 to 2nd semester 2007 and 1st semester of 2009 to 2nd semester of 2010 of the *Encuesta Continua de Hogares* (ECH).

Figure 4
Age distributions for individuals by presence of children.



Notes: Sample contains salaried private sector workers age 25-55 from survey 2nd semester of 2004 to 2nd semester 2007 and 1st semester of 2009 to 2nd semester of 2010 of the *Encuesta Continua de Hogares* (ECH).

Apendix A

Appendix Table A.1
Effect of the health insurance benefit extension on labor supply and tax evasion outcomes. Full difference in difference regression results.

	Sample: all individuals, age 25-55		Sample: private sector workers, age 25-55	
	(1)	(2)	(3)	(4)
Variables	Employment (Table X, Column 1)	Weekly hours ≥ 25 (Table X, Column 2)	Ln (weekly hours) (Table X, Column 3)	Registered employment (Table X, Column 4)
AnyChildren*Postreform	0.0151***	0.0159***	0.0090*	0.0150**
	[0.0033]	[0.0028]	[0.0050]	[0.0060]
AnyChildren	0.0110*	-0.0006	-0.006	0.0168***
	[0.0054]	[0.0062]	[0.0093]	[0.0042]
# children ages 0-10	-0.0598***	-0.0553***	-0.0253***	-0.0321***
	[0.0023]	[0.0026]	[0.0039]	[0.0018]
# children ages 11-17	-0.0267***	-0.0306***	-0.0192***	-0.0276***
	[0.0023]	[0.0026]	[0.0051]	[0.0030]
# children ≥ 18	-0.0116***	-0.0113***	0.0032	-0.0158***
	[0.0027]	[0.0031]	[0.0051]	[0.0043]
Male	0.0704***	0.1756***	0.0686***	-0.0226***
	[0.0099]	[0.0139]	[0.0085]	[0.0078]
Head	0.1069***	0.1058***	0.0268***	0.0198***
	[0.0088]	[0.0074]	[0.0041]	[0.0048]
Married	-0.1379***	-0.0894***	-0.0265**	0.0702***
	[0.0090]	[0.0095]	[0.0094]	[0.0099]
Complete primary	0.0551***	0.0584***	0.0440***	0.0622***
	[0.0063]	[0.0061]	[0.0117]	[0.0088]
Incomplete high school	0.1115***	0.1241***	0.0464***	0.1107***
	[0.0050]	[0.0049]	[0.0115]	[0.0083]
Complete high school	0.1666***	0.2119***	0.0511***	0.1540***
	[0.0093]	[0.0063]	[0.0131]	[0.0082]
Incomplete college	0.2015***	0.2209***	-0.0218*	0.1473***
	[0.0102]	[0.0097]	[0.0121]	[0.0098]
Complete college or more	0.2605***	0.2070***	-0.0701***	0.1429***
	[0.0125]	[0.0103]	[0.0119]	[0.0100]
Age 30-34	0.0305***	0.0270***	0.0211***	0.0285***
	[0.0066]	[0.0053]	[0.0064]	[0.0061]
Age 35-39	0.0328***	0.0217***	0.0270***	0.0480***
	[0.0074]	[0.0070]	[0.0060]	[0.0068]
Age 40-44	0.0205**	0.0124	0.0328***	0.0573***
	[0.0084]	[0.0077]	[0.0061]	[0.0073]
Age 45-49	-0.0101	-0.0263***	0.0153	0.0463***
	[0.0086]	[0.0088]	[0.0101]	[0.0070]
Age 50-55	-0.0627***	-0.0799***	0.0162*	0.0433***
	[0.0107]	[0.0100]	[0.0086]	[0.0085]
Male*head	0.2530***	0.2403***	0.1079***	0.0125
	[0.0086]	[0.0107]	[0.0111]	[0.0110]

Appendix Table A.1 (continued)

Variables	(1) Employment (Table X, Column 1)	(2) Weekly hours ≥ 25 (Table X, Column 2)	(3) Ln (weekly hours) (Table X, Column 3)	(4) Register Employment (Table X, Column 4)
Firm size: 2-4 emps	--	--	0.3541***	0.1598***
			[0.0215]	[0.0202]
5-9 emps	--	--	0.4445***	0.3659***
			[0.0268]	[0.0199]
10-49 emps	--	--	0.4928***	0.4899***
			[0.0259]	[0.0212]
> 49 emps	--	--	0.5279***	0.5427***
			[0.0264]	[0.0209]
Industries: Industry	--	--	-0.0466***	-0.0299**
			[0.0110]	[0.0113]
Manufacturing	--	--	-0.0727***	-0.0089
			[0.0124]	[0.0109]
Construction	--	--	-0.0791***	-0.0304***
			[0.0087]	[0.0101]
Trade	--	--	-0.0322***	0.0083
			[0.0106]	[0.0095]
Transport/commun	--	--	-0.0087	0.0301***
			[0.0106]	[0.0100]
Finance/professional	--	--	-0.1249***	0.0259**
			[0.0114]	[0.0093]
Education/health	--	--	-0.2876***	-0.0198*
			[0.0118]	[0.0103]
Personal	--	--	-0.2865***	0.0067
			[0.0298]	[0.0194]
Observations	97552	97552	71147	71147
R^2	0.24	0.28	0.29	0.3

Notes: see notes to Table 4. Robust standard errors in brackets are clustered on children and semester. Regressions are weighted with ECH supplement.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.2
Effect of the health insurance benefit extension on employment and hours of work. Difference in difference estimates using alternative definitions of the dependent variable.

	Sample: all individuals, age 25-55		Sample: private sector workers, age 25-55
	(1)	(2)	(3)
	Labor force participation (1 = employed or unemployed)	Total weekly hours (including zeros)	Weekly hours (excluding zeros)
Children*Post	0.0070*	0.5223***	0.053
	[0.0037]	[0.1262]	[0.1279]
Children (<18 dummy)	0.0118**	0.4274	-0.0682
	[0.0049]	[0.2599]	[0.2320]
Observations	97552	97552	71147
R^2	0.22	0.32	0.29

Notes: Sample contains individuals age 25-55 (salaried private sector workers for weekly hours, all individuals for labor force participation/total weekly hours) Notes: Sample contains salaried private sector workers age 25-55 from survey 2nd semester of 2004 to 2nd semester 2007 and 1st semester of 2009 to 2nd semester of 2010 of the *Encuesta Continua de Hogares* (ECH). Coefficients estimates are taken from a regression as described in Equation (1). *Children* equals 1 if the individual has at least one child younger than 18 year old. *Post* equals 1 for years 2009-2010. Regressions are linear probability models for labor force participation, and log-linear for total weekly hours and weekly hours. Controls include variables for number of children by age group (3 groups); 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), industry (9) categories (excluded weekly hours regressions) and time (11 categories); and interactions between gender and marital status, and time and region of residence (unless otherwise indicated). Robust standard errors in brackets are clustered on children and semester. Regressions are weighted with ECH supplement weights.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.3
Effect of the health insurance benefit extension on labor supply and tax evasion outcomes. More specification checks. Difference in difference estimates.

	Sample: all individuals, age 25-55		Sample: private sector workers, age 25-55	
	(1)	(2)	(3)	(4)
	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employment
Panel A. Additional covariates				
AnyChildren*Postreform	0.0152***	0.0159***	0.0090*	0.0153**
	[0.0033]	[0.0028]	[0.0050]	[0.0059]
AnyChildren	0.0123**	-0.0002	-0.004	0.0151***
	[0.0055]	[0.0063]	[0.0093]	[0.0042]
Observations	97552	97552	71147	71147
R^2	0.24	0.28	0.29	0.3
Panel B. Adds department unemployment rate				
AnyChildren*Postreform	0.0149***	0.0143***	0.0086*	0.0120*
	[0.0030]	[0.0026]	[0.0049]	[0.0058]
AnyChildren	0.0109*	-0.0001	-0.0067	0.0173***
	[0.0053]	[0.0062]	[0.0094]	[0.0045]
Observations	97552	97552	71147	71147
R^2	0.24	0.28	0.29	0.3
Panel C. Probit estimation				
AnyChildren*Postreform	0.0059**	0.0077**	--	0.0112*
	[0.0029]	[0.0037]		[0.0070]
AnyChildren	0.0033	-0.0095***	--	0.0099***
	[0.0051]	[0.0046]		[0.0038]
Observations	97552	71147	--	71147
R^2	0.24	0.29	--	0.3
Panel D. Whitout family allowences sample				
AnyChildren*Postreform	0.0227***	0.0268***	0.0101**	0.0152**
	[0.0035]	[0.0030]	[0.0044]	[0.0056]
AnyChildren	-0.0025	-0.0009	-0.0047	-0.0047
	[0.0076]	[0.0105]	[0.0105]	[0.0058]
Observations	53294	53294	40889	40889
R^2	0.22	0.24	0.25	0.32

Notes: Sample contains individuals age 25-(salaried private sector workers for hours per week/registered employment, all individuals for employment/weekly hours ≥ 25) from Notes: Sample contains salaried private sector workers age 25-55 from survey 2nd semester of 2004 to 2nd
semester 2007 and 1st semester of 2009 to 2nd semester of 2010 of the *Encuesta Continua de Hogares* (ECH). Coefficients estimates are taken from a regression as described in Equation (1). *Children* equals 1 if the individual has at least one child younger than 18 year old. *Post* equals 1 for years 2009-2010. Regressions are linear probability models for employment, weekly hours ≥ 25 and registered employment (except for Panel C which provides results for probit regressions, marginal effects are reported), and log-linear for weekly hours. Controls include variables for number of children by age group (3 groups); 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), industry (9) categories (excluded weekly hours regressions) and time (11 categories); and interactions between gender and marital status, and time and region of residence. Panel A reports results adding as controls household size and interactions between gender and head of the household status and age, respectively. Panel B reports results including state unemployment rate instead of state-by-semester dummies in controlling for state-year specific shocks. Panel D change the universe to not include individuals who received AFAM benefit. Robust standard errors in brackets are clustered on children and semester. Regressions are weighted with ECH supplement weights.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A.4
Effect of the health insurance benefit extension on labor supply and tax evasion outcomes by gender and marital status of men. Difference in difference estimates.

Panel A. By gender group

	Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employment	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employment
AnyChildren*Postreform	0.0225***	0.0252***	0.0277***	0.0316***	-0.0027	-0.003	-0.008	0.0021
	[0.0062]	[0.0058]	[0.0093]	[0.0069]	[0.0047]	[0.0071]	[0.0059]	[0.0058]
AnyChildren	0.0003	-0.0291***	-0.0241	0.0140**	0.0211***	0.0325***	0.0250***	0.0204***
	[0.0077]	[0.0093]	[0.0165]	[0.0063]	[0.0055]	[0.0063]	[0.0084]	[0.0049]
P-value female = male					0.0106**	0.0294**	0.0088***	0.0000***
Observations	57133	57133	33499	33499	40419	40419	37648	37648
R^2	0.16	0.15	0.24	0.35	0.03	0.04	0.11	0.25
Mean outcome (2004-2007)	0.51	0.37	34.4	0.69	0.93	0.88	48.01	0.81

Panel B. By marital status of men

	Single men				Married men			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employment	Employment	Weekly hours ≥ 25	Ln (weekly hours)	Registered employment
AnyChildren*Postreform	-0.0301	-0.0277	0.0008	0.0481**	-0.0005	-0.0028	-0.0067	0.0048
	[0.0216]	[0.0356]	[0.0385]	[0.0209]	[0.0044]	[0.0066]	[0.0062]	[0.0068]
AnyChildren	0.0462	0.0245	-0.0612	0.0288	0.0173***	0.0325***	0.0314***	0.0213***
	[0.0378]	[0.0432]	[0.0373]	[0.0466]	[0.0057]	[0.0067]	[0.0083]	[0.0058]
P-value single = married					0.1417	0.4503	0.7504	0.0207**
Observations	5318	5318	4754	4754	35101	35101	32894	32894
R^2	0.08	0.09	0.18	0.36	0.03	0.03	0.1	0.24
Mean outcome (2004-2007)	0.91	0.82	45.89	0.69	0.93	0.89	48.06	0.82

Notes: Sample contains individuals age 25-55 (salaried private sector workers for hours per week/registered employment, all individuals for employment/weekly hours ≥ 25) Notes: Sample contains salaried private sector workers age 25-55 from survey 2nd semester of 2004 to 2nd semester 2007 and 1st semester of 2009 to 2nd semester of 2010 of the *Encuesta Continua de Hogares* (ECH). Coefficients estimates are taken from a regression as described in Equation (1). *Children* equals 1 if the individual has at least one child younger than 18 year old. *Post* equals 1 for years 2009-2010. Regressions are linear probability models for employment, weekly hours ≥ 25 and registered employment, and log-linear for weekly hours. Controls include variables for number of children by age group (3 groups); 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), industry (9) categories (excluded from employment and weekly hours ≥ 25 regressions) and time (11 categories); and interactions between gender and marital status, and time and region of residence. Robust standard errors in brackets are clustered on children and semester. Regressions are weighted with ECH supplement weights. "P-value" is a *t*-test of the equality of the Children*Post coefficients on the two populations. Reported means are for the regression universe over the pre-reform period (2004-2007).
* significant at 10%; ** significant at 5%; *** significant at 1%.