

# Tax evasion and productivity: do firms escape EPL through informality? Evidence from a regression discontinuity design\*

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## Abstract

Tax compliance has costs and benefits which may depend on the institutional environment in which firms operate. The relationship between tax evasion and productivity is not always unambiguous and firm size can be a crucial issue whenever firms are constrained by the institutional framework in a measure that depends on their size. We argue that firms may respond to string employment protection legislation through accrued informality thus (partially) offsetting the negative effect of tax evasion on productivity. We exploit the Italian dismissal legislation imposing higher firing costs for firms with more than 15 workers and show that tax evasion reduces job turnover for firms above the 15 workers threshold; furthermore, while the overall effect of tax evasion on firms' productivity is negative, the differential effect for firms above the threshold as compared to smaller firms is positive and significant.

Key words: tax evasion, EPL, productivity, firm size, RD estimation

JEL Classification: D02; D22; D24

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## 1 Introduction

Tax and social security evasion is a common phenomenon in both developing and developed countries. The informal sector accounts for 10% to 20% of GDP in most OECD countries, 20% to 30% in Southern European OECD countries and in Central European transition economies (Schneider and Enste, 2000). The incidence of informality varies considerably both across and within countries. Many factors explain this cross- country variation: (1) the level of taxation and fiscal regulations (Loayza, 1996; Friedman et al., 2000; Loayza et al., 2005; Packard et al, 2012); (2) the strictness of labour and product market regulations (Botero et al, 2003; Djankov et al., 2002; Loayza et al., 2005; Packard et al, 2012); (3) the quality of institutions (Friedman et al., 2000; Schneider, 2000; Loayza et al., 2005; Packard et al, 2012).

A growing attention is devoted to tax compliance as a determinant of growth. It has been show that in general informality is detrimental for firms' productivity even when it is just partial. This may occur for a number of reasons. Informal firms have a limited access to capital markets and to market support institutions and law enforcement. Moreover, they tend to invest less both in physical and human capital (World Bank, 2007).

However, if there are several reasons to believe that tax evasion undermines growth, on the other hand firms can resort to informality to cope with excess regulatory pressure; in this case the effect of tax evasion on productivity may not be so clear cut. Indeed, formality implies costs which go beyond the burden of taxes, such as compliance with legal requirements or rent seeking bureaucracy, so that evasion may not necessarily be detrimental to growth (Sarte, 2000). Among the others, the presence of high labour adjustment costs induced by rigid labour market regulation (e.g. EPL) may create incentives for firms to hire workers in the informal sector (Bosch and Esteban-Pretel, 2012; Di Porto et al, 2016). Therefore, in an overregulated market, the added flexibility resulting from informality may enable firms to operate more efficiently (Almeida and Carneiro, 2012).

In this paper we analyze the effect of informality on the productivity of firms which are subject to different degree of labour market regulation (EPL). We claim that, in presence of high firing costs, informality may allow firms greater flexibility in their employment and production decisions which, in turn, can lead them to operate more efficiently and increase productivity. As a result, the negative effect of informality on productivity may be relatively lower for firms facing higher firing costs.

The impact of informality on firms' productivity is analyzed by means of a regression discontinuity design (RDD) that exploits the variation in EPL across Italian firms below and above an employment size threshold (15 employees), the EPL provisions being more stringent above the 15-employee threshold. The discontinuous change in legislation at the threshold should result in a discontinuous

effect of informality on firms' behavior. We use this idea to study two related aspects. First we consider whether in presence of a large share of tax evasion, firms' job reallocation in the formal labour market drops just above the threshold, implying that informality reduces turnover of formal (regulated) jobs when firing costs are higher since firms have an incentive to hire workers in the informal sectors.<sup>1</sup> Second, we analyze whether the negative effect of informality on the productivity of firms just above the threshold is relatively abated as compared to the impact below the threshold.

In order to identify the impact of tax evasion on firm's labor adjustments and productivity we have to tackle the problem of endogeneity. Indeed It may be that propensity to compliance, labor adjustments and productivity are driven by local factors such as for example the quality of institutions, cultural factors, etc; in addition our results could be biased by reverse causality, as tax compliance may be influenced by labor market conditions and firms' performance. We address endogeneity in two ways: i) we include firm's fixed effects; ii) we identify two instruments for tax evasion, namely the judges' turnover in civil courts and the timing of local (province) elections. Both instruments are used separately and jointly.

As far as judges' turnover within judicial districts is concerned, it is well known that tax compliance depends on the level of the enforcement of legislation, which in turn is related to courts' efficiency. One of the determinants of Courts' internal organization has been shown to be the turnover rate of the judges (Guerra and Tagliapietra, 2005). Judges turnover is not related to our dependent variables nor is determined by local factors; indeed the transfer of a judge is the result of independent decisions taken from different agents and approved on the bases of objective circumstances provided for by the law. It is then exogenous with respect to our empirical model.

As to the timing of local (province) level elections, the impact of the occurrence of elections on the probability of auditing is a well-established result in the literature (Besley et al. 2014; Skouras and Christodoulakis, 2013); a change in the (local) ruling coalition can represent a shock to local entrenched powers and by this way influence the enforcement of tax regulation. Although Italian provinces have no tax competence (as most taxes are established at the central Government level while local taxes are decided upon and collected by municipalities) tax auditing and collection is carried out a province level, within the same boundaries of province constituencies. In addition, province elections occur at intervals which are established by law (i.e. every 5 years), therefore the timing of election is exogenous.

Our preliminary results show that informality reduces labor reallocation rates in the formal sector for firms facing higher firing costs (since they tend to adjust more on the informal market).

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<sup>1</sup>There is a large empirical and theoretical literature which shows that strict EPL has a negative effect on both job creation and job destruction, reducing the overall turnover of regular jobs (Bertola et al, 1999; Blanchard and Portugal, 2001; Hijzen et al, 2013; Vindigni et al, 2015).

Moreover, firms with higher firing costs experience productivity gains from informality resulting from the possibility to adjust in the unregulated sector; in other words, while the overall effect of tax evasion on productivity is negative – in line with the literature – the incremental effect of tax evasion on labor productivity for firms facing higher firing costs (i.e. firms above the 15 workers’ threshold) is positive. A quantification of our results suggests that the reduction in job allocation for firms above the threshold as compared to those below should they move from the most to the least compliant provinces would be of more than 36%, while the resulting positive incremental effect of productivity would be more than 15%.

## **2 The institutional background and the identification strategy**

### **2.1 Employment protection legislation and the 15 employees threshold**

According to Italy’s Statuto dei Lavoratori, passed in 1970, an individual dismissal is legal only when it satisfies a just cause, e.g. it can be justified by an objective reason (concerning the production activity for example) or subjective reasons, which are mainly related to misconduct on the part of the worker. The worker has always the right to appeal the firm’s decision and the final outcome ultimately depends on the court’s ruling on the specific case. If the worker does not appeal the firing decision, or if the dismissal is ruled fair, the legislation does not impose any severance payment to the firm.<sup>2</sup> Conversely, when the dismissal is ruled unfair, the judge imposes a specific compensation on the firm.

The maximum compensation to which unlawfully fired workers are entitled to varies with firm size in two important dimensions. For firms with less than 16 employees, the unfairly dismissed worker must be compensated with a fixed severance payment that varies between 2.5 and 6 months of salary independently on the length of judicial procedure and with no obligation of reinstatement of the dismissed worker. Conversely, for firms with more than 15 employees, to which Article 18 of the Statuto dei Lavoratori applies, the worker is entitled to a compensation equal to forgone wages, social security and health insurance contributions for a period from the date of the dismissal to the judicial settlement of the case (with a minimum of 5 months and with no upper limits). Moreover he/she can choose either to be reinstated in the firm or to be paid an additional financial compensation of 15 months of salary.<sup>3</sup> This implies that: (a) firing costs for firms above the 15 employees threshold are always higher than those for smaller firms; and (b) the cost of unfair dismissals increases with the duration of the labour trial only for firms with more than 15 employees .

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<sup>2</sup>When the layoff is ruled fair, a common practice in Italy is that the labour union pays all the legal costs.

<sup>3</sup>The recent reform of the Italian labour market, in force since July 18, 2012, has changed some of the rules related to the termination of the employment relationship. In particular, also for firms with more than 15 employees, it has been restricted reinstatement to certain specific cases of unfair or unjustified dismissal and introduced an upper limit up to 24 months’ salary to the compensation a firm has to pay in case of unfair dismissal. The change in legislation does not affect our estimates, since our data covers the period 2006-2010.

A number of papers have exploited the discontinuity in EPL at the 15 employees threshold in Italy. These studies are mainly focused on identifying the overall effect of EPL on firm's employment decisions and productivity.<sup>4</sup> Differently, our empirical strategy allows us to identify the extent to which informality affects firms' productivity in different firing costs regimes. More specifically, our identification scheme is based on the idea that the costs of informality are different for firms below and above the threshold, i.e. firms subject to a stricter EPL may benefit more from hiring in the informal sector. The discontinuity in firing legislation at the 15-employees threshold should allow us to isolate the effect of interest (the differential impact of informality on productivity) from other (unobserved) factors such as the degree of local economic development, quality of (local) institutions, that may affect all firms within the same location.

## 2.2 The instrumental variables

In order to corroborate our results and control for the potential endogeneity of the indicator of judicial inefficiency, we also present two-stage least squares (2SLS) estimates using two instruments for the duration of labour trials: (1) the judges' turnover in judicial districts and (2) the timing of the local (province) elections.

### 2.2.1 Judges' turnover

Tax compliance depends on the level of enforcement of legislation (detection and fines), which in turn is related to the efficiency of civil courts. It has been shown that judges' turnover has a quite strong impact on the efficiency of Courts' internal organization (Guerra and Tagliapietra, 2005). Therefore this instrument is correlated with the extent of local informality through the efficiency of judicial courts and also satisfies the exclusion restriction as the transfer of judges from one office to another is the result of a number of decisions taken by agents at different levels of the judiciary hierarchy who respond to different sets of incentives. The transfer generally follows a three-step procedure: i) the publication of vacant positions to be filled ii) the request of the judge who is willing to be transferred and occupy the vacant position; iii) the approval by the self-governing body of the judiciary, the Consiglio Superiore della Magistratura (CSM). Judges' turnover rate within

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<sup>4</sup>Boeri and Jimeno (2005) study the effect of employment protection on lay-off probabilities by comparing small and large firms. Garibaldi et al (2004), and Schivardi and Torrini (2008) assess the effects of employment protection on the size distribution of Italian firms, by looking at the probability of firm size adjustments around the 15 employee threshold. Similarly, Hijzen et al (2013) analyse the effect of different EPL provisions on the composition of workforce, worker turnover and productivity of firms above and below the 15 employees threshold. All these papers identify the effect of employment protection by exploiting the fact that Italian firms with fewer than 15 employees are subject to lower dismissal costs than firms with more than 15 employees. Other studies exploit both the discontinuity in EPL at the 15 employees threshold as well as the temporal variation in the legislation, in order to assess the effect of reforms on job flows (Kluger and Pica, 2008), wages (Leonardi and Pica, 2013), productivity and capital deepening (Cingano et al, 2015). Our study builds on these prior works in one major respect. Differently from the papers mentioned above, the aim of our analysis is to isolate the economic effects of the varying degree of efficiency of labour courts from the effect of the labour legislation itself, by exploiting both the discontinuity of EPL at the 15-employees threshold as well as the cross-district and over-time variation of labour courts' efficiency (and then firing costs).

each districts depends on the number of vacant positions available which in turn is related to career advancements or retirement. Once a vacant position is created, the judge who is willing to be transferred has to apply to the CSM; as a general rule, judges cannot be transferred to a different assignment or district without their consent.<sup>5</sup> Once applications are received, the CSM decides on the basis of a competitive procedure among candidates. The criteria for the CSM collegial decision are the following: competence, which is assessed on the basis of the functions so far carried out and the judge's capacities; the judge's health status and his/her family members' (offsprings, spouse, parents and brothers/sisters if leaving with the judge, in some cases relatives and relatives-in-law); family ties; merit (which also depends on the fact that in the past the judge has occupied vacancies for which an urgent procedure had been set up or vacancies for which no application had been received); seniority.<sup>6</sup> Therefore, the complexity of the transfer procedure, to which the decisions taken by different agents contribute, is such that the turnover rate in each district ends up to be independent from (local) factors that might also affect firm-level outcomes.

*Such conclusion is also supported by the data in our sample (see descriptive statistics in Table 1 below), which show no clear territorial pattern in the number of unfilled vacant positions (expressed both in units and as a ratio of the total number of judges). Moreover, Figure 4 displays the sample correlation between the average number of vacant positions (normalized to the number of judges in office) and the level of income per capita (as a proxy for the degree of local economic development). The correlation is virtually zero. This supports our conclusion that the openings/closeness of a vacant position in the labour courts is a complex process which is not related to local institutional and economic characteristics.*

### **2.2.2 Local political elections**

The influence of electoral cycle on fiscal choices has been widely investigated since Nordhaus (1975). Recent literature on this topic follows two main approaches. A strand of contributions build on models based on information asymmetries between voters and politicians following Rogoff and Sibert (1988) and Rogoff (1990): according to this line, politicians manipulate budgetary instruments to signal their competency in electoral and pre-electoral periods. Another strand of literature focus on tax choices as a tool to increase the probability of re-election; in this framework not only tax setting

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<sup>5</sup>An important feature of the Italian judicial system is the principle of "inamovibilità", according to which a judge can be transferred to a different Court or to a different assignment only upon his/her consent. The principle of "inamovibilità" is a constitutional provision aiming at assuring the independence of the judiciary, which could be undermined should a judge be compelled to quit his/her activity for suspension or transfer. There are some exceptions namely the need to cover vacancies in cases established by law, as disciplinary actions or for reasons of "incompatibilità ambientale", i.e. the judge is considered incompatible with the workplace. The judge can appeal the CSM decision in all cases.

<sup>6</sup>See the "Circolare 15098 of November 30, 1993 and subsequent amendments. Health status and family ties are not taken in consideration for top positions, such as for example the Supreme Court.

but also the probability of auditing and controls can be influenced by the occurrence of an election. On the empirical ground, focusing on German municipalities Foremny and Riedel (2014) show that the growth rate of local business taxes is significantly lower in election periods while Skouras and Christodoulakis (2013) find that around election audit activity by tax collector is reduced and underreporting increases in Greece; Ronconi (2009) show that the occurrence of a political election influence actual enforcement of labour legislation. In addition, other studies focus on the ruling coalition attitude toward tax compliance of as a determinant of tax evasion: a change in the (local) ruling coalition can represent a shock to local entrenched powers and by this way influence the enforcement of tax regulation (Besley et al, 2014). Fantozzi and Raitano (2015) investigated tax compliance in Italy as a result of a change in ruling parties and found an increase in self-employed underreporting under centre-right coalition.

Building on these contributions, we instrument the tax gap at province level with a dummy indicating the occurrence of a province election in the year. There are several reasons for choosing province level elections. On the administrative point of view, the Italian territory is organized in regions, provinces and municipalities.<sup>7</sup> Although the Italian provinces have no a direct tax competence (as most taxes are established at the central Government level while local taxes are decided upon and collected by municipalities),t a tax auditing and collection is carried out a province level, within the same boundaries of province constituencies (by the so called “Direzioni Provinciali”). Therefore, we interpret the occurrence of a province election as a potential shock to the local established powers. Elections can bring a switch in the local ruling coalition, which may influence firms’ expectation about the severity of tax enforcement and by this way tax compliance. In addition, province elections occur at intervals which are established by law (i.e. every 5 years),<sup>8</sup> therefore the timing of election is exogenous and the time of elections is different across provinces.<sup>9</sup>

### 3 Data sources

#### 3.1 Firms data

Firm level data are drawn from AIDA (Analisi Informatizzata delle Aziende Italiane) produced by Bureau van Dijk (BvD). BvD collects balance sheet data from the national Chambers of Commerce. The version of Aida used in our analysis includes all Italian firms that have reported their financial

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<sup>7</sup>From the administrative point of view, the Italian territory is organized in regions, provinces and municipalities. Provinces have administrative tasks in province areas and in inter municipality territory; they have coordination tasks and are in charge of the implementation of public works in various sectors, including the economic, productive, trade sectors; they carry out programming activity for the provincial territory.

<sup>8</sup>In the period under study the legislation regulating the election and the tasks of province level representatives was the Testo Unico, G.U. n. 227 del 28 settembre 2000, s.o. n. 162/L. The legislation was reformed in 2014. Between 2007 and 2010 elections occurred before the end of the natural term only in 13 provinces. In most cases anticipation was due to the resignation of the President motivated by the choice to campaign in the Parliament election.

<sup>9</sup>In our sample period there are 98 elections episodes: 8 in 2007, 20 in 2008, 68 in 2009 and 8 in 2010.

statement to the national Chamber of Commerce in the period 2007-2010, for a total of more than 800,000 Italian firms operating in all productive sectors. Apart from balance sheet data, Aida provides a wide range of financial and descriptive information (industry and activity codes, firm age, etc.) and the number of employees. Moreover, AIDA gives information on the location of firms at a municipality level, allowing the match of firms' data with the courts' database.

The Aida database has a drawback, as it does not allow to distinguish between newly created firms and firms that simply enter the sample at a given period  $t$  but were already operating in the period before; similarly, it is not possible to identify firms' closures from firms that exit the sample for other reasons. Therefore, we have restricted the analysis to continuing firms, e.g. firms that are in the sample for at least two consecutive periods.<sup>10</sup> Given this limitation and after cleaning the database from outliers and missing information, our final sample consists of around 160,000 private firms operating in both the manufacturing and non manufacturing sectors.<sup>11</sup> Observations are annual and cover the period from 2007 to 2010. We define job reallocation at firm level as the absolute value of the change in employment between two consecutive periods divided by the average employment between the two periods (see Capellari et al. 2011; Kugler and Pica, 2008, Autor et al, 2007):

$$JT = \frac{|n_{ft} - n_{ft-1}|}{\frac{1}{2}(n_{ft} + n_{ft-1})}$$

Labour productivity is obtained as the log of value added per worker. To preserve comparability between treatment and control groups, we further restrict the sample to firms within the interval 10-20 employees, yielding a sample size of slightly more than 50,000 observations (19464 firms). Tables 3 shows the descriptive statistics.

#### TABLE 1 AROUND HERE

In our sample the average firm has a value added per worker of around 61 thousands euros and employs 14 workers. The average rate of job reallocation is around 19 percentage point. No significant differences can be observed in the level of productivity and reallocation rates between treatment and control groups.

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<sup>10</sup> *Our sample is therefore unbalanced and potentially includes entry and exit firms. In section XXX we provide some results obtained by using a balanced sample which includes only firms that are continuously observable every year from 2007 and 2010.*

<sup>11</sup> The sectors are: (1) Agriculture, forestry and fishing; (2) Mining and quarrying; (3) Food, beverages and tobacco; (4) Textiles; (5) Wood products; (6) Paper products, publishing and printing; (7) Refined petroleum, nuclear fuel and chemical products; (8) Rubber and plastic products; (9) Other non-metallic products; (10) Basic metals and fabricated metal products; (11) Machinery and equipment; (12) Electrical and optical equipment; (13) Transport equipment; (14) Other manufacturing sectors; (15) Electricity, gas and water supply; (16) Construction; (17) Wholesale and retail trade, Repairs; (18) Hotels and restaurants; (19) Transport and communications; (20) Other services. The financial and public sectors are excluded from the analysis.



### 3.2 Tax gap data

We draw data on tax evasion from the Italian Revenue Agency (Agenzia delle Entrate) dataset. The dataset contains data on the expected as well as the actual financial revenue reported to fiscal authorities in the 107 Italian provinces from 2007 to 2010. The novelty of the database relates to the methodology used to estimate tax gap based on an integration of the top-down approach, based on the comparison and cross-checking (matching) between national accounts and administrative fiscal data, and the bottom-up approach, based on results from fiscal audits (Carfora et al., 2015). The indicator of tax evasion (*tax\_gap*) has been constructed as the relative difference of the expected and the reported revenue, i.e.

$$tax\_gap = \frac{(expected\ revenue - reported\ revenue)}{expected\ revenue}.$$

Table 2 reports some descriptive statistics concerning the tax gap over the sample period 2007 to 2010 for the 107 Italian provinces, which are ranked from the most to the least tax compliant.

TABLE 2 AROUND HERE

The data show great territorial heterogeneity in the extent of tax evasion; for example, the differences of the tax gap between the province at the 5th (Agrigento) and 95th (Torino) percentile of the tax compliant distribution 'is around 0.53 percentage points. The standard deviation across provinces is quite large, being more than one half of the average. The within-district time-series variation of tax gap is also substantial: the standard deviation normalized on the mean ranges from 0.06 (in Trento) to 0.26 (in Isernia), with a sample average of around 0.12. This descriptive evidence suggests considerable heterogeneity in informality both cross-sectionally and over time.

### 3.3 Data on judges turnover and elections

We draw data on the number of judges allocated to each judicial districts as well as on the number of inbound and outbound judges from 2006 to 2010 from courts' self regulating body (CSM) database. Judges turnover at district level is calculated as in Guerra and Tagliapietra (2015) according to the following formula:

$$Turnover = \frac{100x(number\ of\ outbound\ judges + number\ of\ inbound\ judges)}{number\ of\ expected\ judges} /$$

## 4 Empirical specification

We estimate the effect of informality on the turnover of formal jobs and labour productivity using firm level data. At firm level, the size of firms plays a significant role in moderating the impact of informality on firms adjustments and productivity. In our analysis, we exploit the discontinuity

of firing legislation at the 15-employees threshold to identify the causal effect of informality on productivity.

In order to control for the fact that informality can be related to the local economic development, we also include local per capita income. Moreover, estimating the impact of judicial inefficiency at firm level allows us to enrich our controls on productivity. Given that (time variant) differences in the regional underlying economic conditions (as GDP per capita) may not necessarily capture all the factors affecting firm productivity (e.g. those factors related to the institutional environment at large), depending on the specification considered we include a full set of district-by-year and sector-by-year dummies. In this way we can rule out any possible source of endogeneity arising from (time and district variant) omitted factors - not already captured by the income per capita - which could influence both productivity and informality.

The model specification is as follows:

$$y_{ft}^r = \beta_1 tax\_gap_t^r + \beta_2 size_f^r + \beta_3 (taxgap_t^r \times size_f^r) + \beta_4 income_t^r + X_{fst}^r \gamma + \mathbf{D}\eta + \eta_f + u_{ft}^r \quad (1)$$

where the dependent variable  $y_{ft}^r$  is either the job turnover or productivity (log of value added) depending on specification;  $tax\_gap_t^r$  is the informality index (*in log*);  $size_f^r$  is a dummy variable which takes value 1 for large firms i.e. firms with number of employees larger than 15, and zero otherwise;<sup>12</sup>  $X_{fst}^r$  are a set of other controls which include a polynomial of a third degree in firm size,  $\eta_f$  stands for firm fixed effects which "absorb" any time-invariant unobservable attributes at firm level, and in particular the effect of the different (time-invariant) EPL regime that applies to firms above and below the 15 employees threshold. The indices  $r$  and  $t$  refer to provinces and year respectively.  $D$  is the matrix of dummies that includes, depending on the specification considered, district-by-year<sup>13</sup> and industry-by-year dummies while  $u_{ft}$  is the error term. The coefficient on  $tax\_gap_t^r$  gives the common effect of trial length on small and large firms due to discounting, while the interaction term  $tax\_gap_t^r \times size_f^r$  captures the differential effect of informality induced by the more strict EPL regimes on large firms productivity. Here again the variable  $tax\_gap_t^r$  is instrumented with :the judges' turnover in judicial districts and the timing of local (province)

<sup>12</sup>We define firms as small if they have less than 15 employees in all years and large if they have more than 15 employees in all years of the sample period. We also check the robustness of our results to a different threshold definition (see section 7.3) and to the inclusion in the sample of firms which move above and below the threshold over the sample period. Results are available upon request.

<sup>13</sup>The inclusion of district-by-year dummies allows to control for all district-specific time-varying characteristics (for example, the quality of local infrastructure) which have the same effects across firms. Notice that this set of dummies absorbs the main effect of trials' length, as this variable only varies by district and time.

elections, while the interaction  $tax\_gap_t^r \times size_f^r$  is instrumented by interacting each instruments with the dummy  $size_f^r$ .

Our identification strategy relies on the assumption that firms do not endogenously sort in or out of treatment. In specification 1, firm fixed effects capture all time-invariant unobserved factors that may affect the propensity of firms to self-select above or below the threshold. Graph 1 displays the distribution of firms between 10 to 20 workers. If any significant manipulation occurred because of increased firing costs the graph should display a dip on the 15 workers threshold, which is not observed; rather density smoothly declines with the increase in the number of workers.<sup>14</sup>

[GRAPH 1 HERE]

Unfortunately our data do not contain information on tax evasion at firm level, so we could not directly infer any difference in tax compliance between small firms and big firms. Indeed firm fixed effects cannot account for the selection induced by time varying factors as informality themselves. The employment growth regressions reported in Section 5.2 suggest that the employment growth regressions that informality does not affect firms propensity to grow around the threshold and therefore lend further support to our identification strategy.<sup>15</sup>

We also address the potential endogeneity of tax evasion in two ways: i) we include firm's fixed effects; ii) we instrument  $tax\_gap$  (and its interaction) with two instruments, namely the judges' turnover in judicial districts and the timing of local (province) elections. The two instruments will be used in the IV regressions both separately and jointly, together with a full set of district and district-year dummies (depending on the specification considered), thus producing three sets of estimates. The results are remarkably robust to every specification. Moreover, in all regressions we control for differences in the economic development at district level by including among the regressors district per capita income (GDP). Finally, depending on the specification considered, we show that our results are also robust to the inclusion of a full set of district-year and sector-year dummies. In this way, we can rule out any possible source of bias arising from (time and district variant) omitted factors - not already captured by the regional income per capita - which could influence both productivity and judicial inefficiency, thus yielding further support to our identification strategy.

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<sup>14</sup>The same pattern is observed also in other studies on the Italian EPL, so that our graphical results on the of significant manipulation are in line with a body of empirical analysis focusing on the effects of the discontinuity in dismissal costs in Italy (Boeri and Jimeno, 2005; Schivardi and Torrini, 2008; Leonardi and Pica, 2013, Cingano et al. 2014).

<sup>15</sup>The result that EPL provisions do not affect firms propensity to grow is not new in the empirical literature. See among the others, Cingano et al (2015), Leonardi and Pica (2013) and Schivardi and Torrini (2005) which examine the effect of EPL on firms' size distribution below and above the 15 employees-threshold in Italy.

## 5 Results

### 5.1 Baseline model

In Table 3 we report the effect of informality on the reallocation of formal (regulated) jobs at firm level estimated as in equation 1, using all sets of instruments shown so far. As the variable measuring informality varies both across provinces and time, we are able to control for any time-invariant unobserved firm characteristics by the use of firm fixed effects, thus fully exploiting the firm level dimension of the dataset.

TABLE 3 AROUND HERE

Tax evasion has no significant effect on labour adjustments for firms under the 15 workers threshold while it has a negative impact for firms employing more than 15 workers. Nevertheless, the coefficient of the interaction term is always negative and significant and the overall coefficient estimated for large firms is around 0.31 when both instruments are used in the estimation. Based on the coefficients reported in column 3, the estimated differential effect for firms above the 15 employees threshold is -0.405. This implies that an increase of one (within) standard deviation (0.046) of *tax\_gap* would lead to a 2 percentage points decrease in job turnover for firms above the threshold as compared to smaller firms. This corresponds to more than 11 percent of the average turnover.

In all cases the two instruments are relevant, as the null that the equation is underidentified is always rejected, while the F statistics is well above the rule of thumb threshold of 10 in all cases. Furthermore, the overidentification test does not reject the null that the instrument are uncorrelated with the error term. First stage regressions are displayed in table 4, while table 6 shows findings from OLS estimates.<sup>16</sup>

Table 4 shows the effect of tax evasion on firms' labour productivity.

TABLE 4 AROUND HERE

In all cases the overall effect of tax evasion on firms' productivity is negative and significant; this result is in line with previous literature. However, the interaction coefficient is positive and significant. This implies that in presence of a stricter employment legislation (firms above the threshold), firms react by adjusting in the informal sector. This mitigates the negative impact of informality on productivity. Based on the coefficients reported in column 3, the estimated differential coefficient for firms above the 15 employees threshold is 0.609, which implies that an increase of one (within) standard deviation of *tax\_gap* would raise the average value added per worker in firms above the threshold by 14%.

The differential impact informality on job reallocation and productivity for firms above and be-

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<sup>16</sup>The coefficients of the first stage regressions show that both instruments are correlated with our endogenous regressors confirming the relevance of our instruments.

low the 15-employees threshold is remarkably robust to the inclusion of district-by-year dummies (columns 3, 6 and 9). In this specification the main effect ( $tax\_gap$ ) is not included because it is absorbed by the district-year dummies, which also capture all time variant and district specific factors that may affect simultaneously the extent of informality and firms' productivity.<sup>17</sup> The stability of the coefficient on the  $tax\_gap \times size$  interaction term when adding district-year dummies provides further reassurance that the estimated differential effect is not driven by any possible source of bias arising from district-time variant omitted factors.

## 5.2 Robustness

### 5.2.1 Robustness to different sample size

In Table t we check the robustness of our main results by restricting the sample to a more homogeneous group of firms within the size interval 6-25 employees. Our results are robust to both the change in the size range and alternative specifications of the polynomial in firm size. The estimated coefficients on the interaction ( $tax\_gap \times size$ ) are similar to those in Tables 3 and 4, confirming the relative negative effect of informality of the turnover rate of regulated jobs and the positive effect on productivity in the firms facing higher firing costs.<sup>18</sup>

TABLE 7 AROUND HERE

### 5.2.2 Firm growth regressions

Finally, we check for the potential sorting induced by courts' delays by estimating the following linear probability model for the probability of growth of firms around the threshold (see Schivardi and Torrini, 2008):

$$g_{ft} = \mathbf{S}_{ft}\boldsymbol{\gamma} + (\mathbf{S}_{ft} \times tax\_gap_t^r)\boldsymbol{\delta} + \mathbf{X}_{ft}\boldsymbol{\beta} + D\varphi + \eta_f + u_{ft}^r \quad (2)$$

where  $g_{ft} = 1$  if firm  $f$  in year  $t$  has a larger size than in  $t - 1$ . The term  $\mathbf{S}_{ft}$  denotes a set of size dummies for firms with 13, 14 and 15 employees and  $tax\_gap_t^r$  is our variable for informality at province level. The matrix  $\mathbf{X}_{ft}$  includes a set of controls at firm level such as a polynomial in firm size. Finally, we also include a full set of district-by-year and industry-by-year dummies to control for all district and sector unobserved time varying factors and firm fixed effects to account

<sup>17</sup>In this set of regressions the identification of the effect comes entirely from the differential in the EPL provisions for firms above and below the threshold.

<sup>18</sup>An additional concern may be that the 15-employees threshold is imprecisely estimated at firm level due to the complex calculation of the workforce (e.g. part-time and atypical workers) and misreporting of employment data in AIDA (see for a discussion Hijzen et al., 2014). In order to address this problem, we check the robustness of our results to the exclusion of firms between 14 and 18 employees from the sample. We therefore compare the effect of trials' length on productivity of firms between 5 and 13 employees (control group) and firms between 18 and 25 employees (treatment group). Those firms are sufficiently above and below the threshold to avoid the problem of measurement error in the estimation of the size cut-off. Results are remarkably robust to this change and available upon request.

for firm-specific time-invariant factors that may affect firms' propensity to grow. We instrument the interactions ( $S_{ft} \times tax\_gap_t^r$ ) with the two instruments (interacted with the size dummies) already used in our baseline specifications (e.g. judges' turnover rates and a dummy for local elections). The results are reported in Table 8.

TABLE 8 AROUND HERE

Column 1 shows that the probability of expansion of firms just below 15 employees is not significantly different from that of other firms.<sup>19</sup> Moreover, all the interaction terms are not significant, implying that informality do not affect such a probability. Columns 2- 4 report the results obtained splitting the sample in different productivity percentile and once again the growth probability for firms just below the threshold is not significantly affected by informality in any of the sub-samples. These results confirm that informality does not affect significantly the propensity to grow in the formal sector around the 15-employee threshold and therefore self-selection of firms into treatment/control.

## 6 Conclusions

We assess the impact of informality on labour market adjustments and then firm's productivity. We argue that in presence of high firing costs, informality may allow firms greater flexibility in their employment and production decisions which, in turn, can lead them to operate more efficiently and increase productivity.

Exploiting the variability of the extent of tax compliance across Italian provinces and the discontinuity of the firing legislation at the 15-employees threshold, we show that informality significantly reduces the reallocation rate of regulated jobs. This provides some evidence of the substitution between formal (regulated) jobs and jobs created in the informal sector for firms in operating in a stricter EPL regime. Moreover, while the overall effect of tax evasion on productivity is negative – in line with the literature – the incremental effect of tax evasion on labor productivity for firms facing higher firing costs (i.e. firms above the 15 workers' threshold) is positive. This last result suggests that firms operating in a highly regulated environment may get some benefit in terms of productivity from adjusting in the informal sector.

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<sup>19</sup>This result is in line with that in other empirical studies for Italy on the effect of EPL discontinuity at the 15 employees – threshold (see for example Schivardi and Torrini, 2008).

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Table 1: Descriptive statistics on firm variables

	Mean	St. Dev.	p10	p50	p90
Whole sample (11-20)					
VA per worker (000)	61.01	103.18	26.63	47.62	97.28
Flows	0.19	0.25	0	0.10	0.44
Firm size	14.35	2.64	11	14	18
Small firms (11-15)					
VA per worker (000)	61.36	105.7	27.077	47.75	97.91
Flows	0.18	0.25	0	0.09	0.44
Large firms (16-20)					
VA per worker (000)	60.25	97.38	25.75	47.25	95.6875
Flows	0.19	0.26	0	0.11	0.46

*Source:* AIDA database and authors' calculations.

Table 2: Descriptive statistics on Tax Gap

Name	tax gap	st. dev	Name	tax gap	st. dev	Name	tax gap	st. dev
Roma	0.116	0.021	Pisa	0.280	0.043	Pesaro U.	0.477	0.086
Trieste	0.118	0.024	Lecco	0.280	0.045	Pavia	0.482	0.064
Monza	0.125	0.012	Cagliari	0.292	0.029	Asti	0.495	0.038
Bologna	0.128	0.018	Napoli	0.303	0.012	Brindisi	0.499	0.036
Milano	0.136	0.011	Venezia	0.305	0.031	Verbania	0.509	0.046
Parma	0.163	0.024	Savona	0.310	0.025	Barletta	0.517	0.024
Genova	0.166	0.020	Treviso	0.314	0.044	Biella	0.537	0.069
Bolzano	0.182	0.005	Vicenza	0.318	0.051	Reggio C.	0.584	0.055
Trento	0.191	0.005	Cremona	0.319	0.024	Pistoia	0.595	0.077
Chieti	0.194	0.034	Varese	0.328	0.049	Latina	0.623	0.033
Torino	0.200	0.035	Vercelli	0.330	0.040	Imperia	0.624	0.043
Gorizia	0.200	0.028	Mantova	0.350	0.080	Campobasso	0.625	0.038
Siena	0.206	0.028	Lucca	0.354	0.054	Trapani	0.628	0.056
Fermo	0.206	0.014	Palermo	0.365	0.036	Grosseto	0.628	0.026
Firenze	0.207	0.056	Ferrara	0.369	0.068	Frosinone	0.631	0.069
Pordenone	0.215	0.046	Sassari	0.371	0.011	Caserta	0.635	0.037
Novara	0.233	0.034	Perugia	0.391	0.034	Nuoro	0.643	0.137
Reggio E.	0.234	0.054	Taranto	0.392	0.080	Oristano	0.650	0.021
Modena	0.234	0.059	Belluno	0.399	0.063	Ragusa	0.671	0.037
Udine	0.236	0.050	Cuneo	0.402	0.024	Salerno	0.683	0.014
Como	0.240	0.026	Rovigo	0.402	0.075	Caltanissetta	0.700	0.072
La Spezia	0.242	0.027	Foggia	0.406	0.021	Viterbo	0.703	0.056
Bergamo	0.245	0.048	L'Aquila	0.413	0.076	Potenza	0.713	0.053
Ravenna	0.251	0.043	Alessandria	0.414	0.036	Avellino	0.726	0.062
Messina	0.255	0.042	Bari	0.415	0.023	Agrigento	0.727	0.035
Aosta	0.256	0.016	Massa	0.430	0.072	Isernia	0.733	0.201
Pescara	0.258	0.037	Terni	0.432	0.034	Ascoli		
Livorno	0.260	0.060	Catania	0.432	0.038	Piceno	0.781	0.077
Ancona	0.265	0.032	Prato	0.434	0.058	Benevento	0.805	0.050
Forlì	0.266	0.030	Arezzo	0.438	0.039	Lecce	0.834	0.027
Piacenza	0.266	0.056	Lodi	0.452	0.088	Cosenza	0.854	0.063
Brescia	0.270	0.057	Catanzaro	0.455	0.042	Enna	0.904	0.034
Padova	0.271	0.041	Rimini	0.471	0.046	Rieti	0.969	0.091
Sondrio	0.274	0.043	Teramo	0.474	0.057	Matera	0.998	0.078
Siracusa	0.274	0.033	Macerata	0.476	0.055	Crotone	1.073	0.090
Verona	0.275	0.025				Vibo V.	1.184	0.039
Mean			0.430					
Between-group st. dev.			0.226					
Whitin-group st. dev.			0.046					

Source: Agenzia delle Entrate database and authors' calculations.

Table 3: The effect of tax gap on firms' labor reallocation - selected results

Instrument(s)	Judges' Turnover	Election	Judges' Turnover and Election
tax gap	0.318 <i>0.398</i>	-0.022 <i>0.5</i>	0.198 <i>0.32</i>
tax gap x size	-0.379 *** <i>0.155</i>	-0.518* <i>0.282</i>	-0.405*** <i>0.143</i>
size	0.082*** <i>0.032</i>	0.111* <i>0.059</i>	0.088*** <i>0.03</i>
gdp	0.065 <i>0.13</i>	-0.034 <i>0.152</i>	0.033 <i>0.116</i>
r2	0.065	0.061	0.065
Obs.	53643	53643	53643
Number of firms	19464	19464	19464
<i>First stage statistics</i>			
Underid Test <sup>a</sup>	707.486	317.216	906.035
chi-sq (1) P-value	0.000	0.000	0.000
Weak id Test	480.409	201.356	370.701
Hansen J stat.			0.522
Chi-sq(2) P-value			0.7703

(\*\*\*) (\*\*) (\*) refer to 1%, 5%, 10% significance levels. Robust standard errors (in italics) are clustered at firm level. All estimates are fixed effects; year dummies are always included; a) Kleibergen-Paap rk LM statistic; b) Cragg-Donald Wald F statistic; Instruments: *turnover*, *elections* and interacted terms.

Table 4: The effect of tax gap on firms' productivity - selected results

Instrument(s)	Judges' Turnover	Election	Judges' Turnover and Election
tax gap	-1.720*** <i>0.506</i>	-3.384*** <i>0.752</i>	-2.260*** <i>0.422</i>
tax gap x size	0.563*** <i>0.230</i>	0.746* <i>0.400</i>	0.609*** <i>0.220</i>
size	-0.123*** <i>0.049</i>	-0.161* <i>0.085</i>	-0.133*** <i>0.047</i>
gdp	-0.112 <i>0.201</i>	-0.660** <i>0.294</i>	-0.292 <i>0.184</i>
r2	0.08	0.053	0.074
Obs.	88829	88829	88829
Number of firms	30345	30345	30345
<i>First stage statistics</i>			
Underid Test <sup>a</sup>	762.221	268.950	995.010
Chi-sq (1) P-value	0.000	0.000	0.000
Weak id Test	196.546	196.546	364.584
Hansen J stat.			3.552
Chi-sq(2) P-value			0.1693

(\*\*\*) (\*\*) (\*) refer to 1%, 5%, 10% significance levels. Robust standard errors (in italics) are clustered at firm level. All estimates are fixed effects; year dummies are always included; a) Kleibergen-Paap rk LM statistic; b) Cragg-Donald Wald F statistic; Instruments: *turnover*, *elections* and interacted terms.



Table 5. First stage statistics - selected results

	TG	TG x size	TG	TG x size	TG	TG x size
turnover	-0.00053***	-0.0005***			-0.00052***	-0.00043***
	<i>0.00002</i>	<i>0.00004</i>			<i>0.00002</i>	<i>0.00004</i>
turnover x size	0.00004**	0.00125***			0.00005**	0.00118***
	<i>0.00002</i>	<i>0.00005</i>			<i>0.00002</i>	<i>0.00005</i>
election			0.00399***	-0.00332***	0.00393***	-0.00210***
			<i>0.00025</i>	<i>0.00031</i>	<i>0.00024</i>	<i>0.00030</i>
election x size			-0.00044	0.01292***	-0.00058*	0.00952***
			<i>0.00033</i>	<i>0.00058</i>	<i>0.00032</i>	<i>0.00055</i>
F	335.40	407.06	148.36	309.65	241.88	270.01
p-value	0.00	0.00	0.00	0.00	0.00	0.00
Underid SW <sup>a</sup>	676.10	747.70	299.02	649.23	979.65	1122.61
p-value	0.00	0.00	0.00	0.00	0.00	0.00
(Weak id) SW <sup>b</sup>	675.99	747.58	298.98	649.13	326.49	374.14

(\*\*\*) (\*\*) (\*) refer to 1%, 5%, 10% significance levels. Robust standard errors (in italics) are clustered at firm level. All estimates are fixed effects; year dummies are always included. a) Sanderson-Windmeijer (SW) chi-squared; b) Sanderson-Windmeijer F statistics.

Table 6. OLS selected results

	flows	productivity
TG	0.256***	-0.414***
	<i>0.064</i>	<i>0.073</i>
TG x size	-0.06	0.064*
	<i>0.038</i>	<i>0.036</i>
size	0.016*	-0.019**
	<i>0.009</i>	<i>0.01</i>
gdp	0.084	0.268**
	<i>0.091</i>	<i>0.117</i>
r2	0.068	0.088
Obs	72928	108941

(\*\*\*) (\*\*) (\*) refer to 1%, 5%, 10% significance levels. Robust standard errors (in italics) are clustered at firm level. All estimates are fixed effects; year dummies are always included.

Table 7: The effect of tax gap on firms' labour reallocation and productivity (6-25 workers sample) - selected results

<i>Dependent Variable</i>	<b>Flaws</b>	<b>Productivity</b>
tax gap	.404* <i>.234</i>	-1.799*** <i>0.291</i>
tax gap x size	-.535*** <i>.122</i>	0.512*** <i>0.193</i>
size	.123*** <i>.025</i>	-0.118*** <i>0.041</i>
gdp	.006 <i>.086</i>	-0.075 <i>0.129</i>
Obs	207550	207550
No Firms	67426	67426
Firm FE	YES	YES

(\*\*\*) (\*\*) (\*) refer to 1%, 5%, 10% significance levels. Robust standard errors (in italics) are clustered at firm level. All estimates are fixed effects; year dummies are always included. Instruments: *turnover* and *elections* and interacted terms.

Table 8: Probability of growth around the threshold and tax gap

	Whole sample	Productivity Percentiles		
		25	50	75
size13	-0.052 <i>0.087</i>	0.135 <i>0.186</i>	0.173 <i>0.135</i>	0.105 <i>0.104</i>
size14	-0.152 <i>0.099</i>	0.097 <i>0.23</i>	0.028 <i>0.156</i>	-0.006 <i>0.114</i>
size15	-0.052 <i>0.106</i>	0.248 <i>0.203</i>	0.032 <i>0.152</i>	-0.029 <i>0.123</i>
Tax gap x size13	0.157 <i>0.421</i>	-0.66 <i>0.787</i>	-0.881 <i>0.603</i>	-0.58 <i>0.486</i>
Tax gap x size13	0.589 <i>0.483</i>	-0.627 <i>0.982</i>	-0.256 <i>0.695</i>	-0.118 <i>0.533</i>
Tax gap x size13	0.262 <i>0.512</i>	-1.019 <i>0.851</i>	-0.122 <i>0.668</i>	0.149 <i>0.566</i>
Obs.	52249	9660	22969	37335
Firms	18925	3767	8599	13755
Firm FE	YES	YES	YES	YES

Notes: Robust standard errors in parenthesis are clustered at the firm level. The dependent variable is a dummy that takes the value of 1 if employment at time  $t$  is larger than employment at time  $t-1$ , and 0 otherwise. Firms between 11 and 20 workers are included. All specifications include a polynomial of firm size. Interactions of *Tax gap* are instrumented with the interactions of *Election* and *Turnover*.

Graph 1: Firm size distribution

