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Maurizio Franzini

The Perverse Effect of Flexible Work Arrangements on Informality*

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The Perverse Effect of Flexible Work Arrangements on Informality*

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Abstract

Flexible work arrangements are on the rise in many countries, ranging from Germany's mini-jobs to UK's zero-hours contracts. These contracts allow for quick labor demand adjustments and are also seen as a way to discourage undeclared work, and more than 10 years ago Italy introduced what was arguably one of the most flexible alternative work arrangements: "labor vouchers." The labor vouchers could be easily purchased to pay for occasional work, with no additional paper work. Between 2008 and 2016 the number of 10-euro labor vouchers purchased in a year went up from 500,000 (less than 1 per 100 inhabitants) to almost 300 million (about 5 times the Italian population). Using random timing in labor inspections, as well as the abolition of labor vouchers, we document a perverse effect of badly designed alternative work arrangements: they severely disrupt the work of labor inspectors, allowing firms to *increase* the amount of undeclared work. Firms who use vouchers for this purpose are shown to hire more regular part-time and fixed-term workers when vouchers become unavailable.

Keywords: informality, labor vouchers, flexible work arrangements, occasional work, zero-hour contracts.
JEL codes: J23, H26.

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Sommario

Forme contrattuali altamente flessibili sono in aumento in molti paesi, dai mini-jobs in Germania ai contratti zero-hours nel Regno Unito. Questi contratti permettono aggiustamenti della domanda di lavoro e vengono anche considerati come un modo per scoraggiare il ricorso al lavoro non dichiarato. Da più di 10 anni l'Italia ha introdotto quello che può essere considerato come una delle forme contrattuali più flessibili, i voucher, che possono essere facilmente acquistati per utilizzare lavoro occasionale, senza aggravi burocratici aggiuntivi. Dal 2008 al 2016 il numero di voucher da 10 euro acquistati in un anno è aumentato da 500,000 (meno di 1 per 100 abitanti) a circa 300 milioni (circa 5 volte il numero della popolazione italiana). In questo lavoro si utilizza il timing nelle ispezioni del lavoro e l'abolizione dei vouchers per documentare un utilizzo perverso di tali forme contrattuali altamente flessibili: se non ben congegnati tali forme contrattuali possono influenzare negativamente il lavoro degli ispettori del lavoro, consentendo alle imprese di aumentare l'ammontare di lavoro non dichiarato. Le imprese che usano voucher per questa finalità si trovano inoltre ad aumentare l'uso del lavoro part time e a tempo determinato quando vouchers vengono aboliti.

1 Introduction

In most advanced economies, approximately 10 to 20 percent of the economy is hidden.[1]. Shadow employment is very persistent and, if anything, its share is increasing over time, as legal firms increasingly employ undeclared workers (Ulyssea, 2018). Overregulation and the legal cost and consequences of labor contracts are the main driving force of shadow work, where firms operate on the intensive margin of informality.

In response to this, countries are introducing more flexible labor contracts, so called alternative work arrangements (AWAs).[2] About 90 percent of European countries have arrangements with no guaranteed working hours (casual work), allowing, on one hand, firms to quickly adjust labor demand, and, on the other hand, workers to have more flexible work schedules.[3]

Most experts expect that the acquired flexibility coupled with a reduced bureaucracy would lure undeclared work out of the shadow. For example, according to Eurofound, European Union's Agency for the improvement of living and working conditions, "(s)ome of these new forms (of employment) have been developed to help formalise undeclared work practices."[4]

This belief would also be consistent with previous work on the relationship between market rigidities and informality. More rigid employment protection legislation has been shown to reduce job creation and destruction, thus overall turnover, which in turn, should push firms to hire workers with more temporary contracts, or completely off the books (for an early and a more recent survey, see Schneider and Enste, 2000, Ulyssea, 2020).[5]

[1] In developing countries the share is closer to 50 percent (Schneider and Enste, 2000).

[2] For an overview on this early stage literature see Boeri et al. (2020), while Katz and Krueger (2019) cover AWAs for the US and Adams and Prassl (2018) and Datta et al. (2019) for Europe.

[3] For example, Chen et al. (2019) estimate that Uber drivers, possibly due to selection, benefit enormously from real-time flexibility. Moreover, for emergency department physicians Chan (2018) shows that work schedules can distort effort allocation and patient care.

[4] See https://www.eurofound.europa.eu/topic/undeclared-work.

[5] Theoretical and empirical contributions that show that labor market rigidities increase informality include Johnson et al. (1998), Blanchard and Portugal (2001), Fugazza and Jacques (2004), Albrecht et al. (2009b), Maloney (2004).

We build a simple partial-equilibrium labor demand model that shows that this is only true as long as AWAs do not interfere with the work of labor inspectors. While the economic literature has generally disregarded labor inspectors, we add to our model the possibility that flexible labor arrangements may complicate labor inspectors' ability to uncover undeclared work.

The model shows that whenever we disregard the work of labor inspectors, more flexible jobs lead to a reduction in hiring and/or firing costs, reducing the amount of undeclared work (see Albrecht et al., 2009, Bosch and Esteban-Pretel, 2012, and Ulyssea, 2018). But a slight twist to the model dramatically changes these predictions. If contracts are allowed to have no work schedule, firms may simply underreport the number of hours worked by their casual employee. For example, in the UK workers may work under "Zero Contract Hours" and work more than the officially declared number of hours. In Italy, workers paid with vouchers may receive a single voucher, so as to justify their physical presence in the workplace, and be paid the rest of their work under the table. Detecting this more subtle type of evasion complicates the task of labor inspectors. This additional mechanism overturns the previous result, as vouchers lead to more rather than less undeclared work.

In spite of the growing importance of AWAs, empirical research on AWAs is limited, with lack of data being one the main obstacles. Labor force surveys contain too little detail to identify these arrangements (Katz and Krueger, 2019), and whenever the paid sums do not contribute to future social security benefits, even administrative data are uninformative.

Moreover, as shown by Mas and Pallais (2017), workers select into AWAs, which hinders the estimation of counterfactual scenarios. This makes it hard to assess whether the availability of flexible work arrangements lead to an exploitation of workers and to more job insecurity. An additional obstacle is that in order to understand whether AWAs reduce the prevalence of shadow work, such work needs to be somehow measurable.

Italy is well-suited to address whether AWAs reduce informality. Within the European

Union, only the Eastern European countries, Spain and Greece, have a higher prevalence of undeclared work[6] and about 15 years ago, in an attempt to incentivize firms to regularize undeclared workers, Italy introduced what was arguably the most flexible work arrangement, labor vouchers.[7] In addition, in 2017 vouchers were abolished, which represents a great opportunity for causal inference.

Up until 2017, employers could purchase 10-euro vouchers from the Italian Social Security Administration, or from banks or tobacco shops, fill in the worker's name, and use it to pay for work without the need of a proper labor contact. The worker would later exchange vouchers for money. For every 10 euro paid by the employer the worker received 7.50 euro, 1.30 euro covered the social security contributions, 70 cents the health insurance, and 50 cents the commission fee paid to the social security administration. Compared to regular labor contracts, vouchers literally annihilated bureaucracy, as well as hiring and firing costs, and policy makers believed that this would discourage the use of undeclared work.[8]

Descriptive evidence, included in a report produced by the Italian Social Security Administration, seemed to support this view. In 2015, regions where the average number of vouchers per worker was higher tended to have less undeclared work (Anastasia et al., 2016). The two extreme cases were the regions of Lombardy and Calabria. The rich Northern region had an average of 78 vouchers per casual worker and was estimated to have less than 10 percent of undeclared work. The poor Southern region, where one in four workers was estimated to be undeclared, used about half as many vouchers per casual worker. Even though the implied elasticity is close to -1, differences in economic activity may explain this pattern. Economic growth is likely to increase the demand for vouchers and to reduce workers willingness to work under the table.

[6] According to Williams et al. (2017), 17.2 percent of Italian work in undeclared (the EU average is 16.4).

[7] In Europe voucher-based work is also available in Austria, Belgium, Ireland, Finland, France, Hungary, Lithuania, and Slovenia (Mandl, 2020).

[8] In the first budget proposed by the new center-right government headed by Giorgia Meloni, che governing coalition is proposing to reintroduce labor vouchers for the agricultural and the tourism sector.

Our economic model points at a quasi-experimental approach to test some of its predictions. We use a unique data set, drawn from three separate Italian administrative records: i) employer-employee social security records that cover the period 2008-2017; ii) daily firm-level purchases of vouchers between 2014 and 2017; and, finally, iii) data on the universe of labor inspections between 2008 and 2017. Leveraging on unpredictability of the timing of labor inspections, we find clear evidence that–as soon an inspection starts–some firms tend to immediately increase their use of vouchers.

Moreover, such jump ceases to exists when the government introduced a small change, requiring firms to announce the use of vouchers with at least a one-hour notice. The one hour notice gave inspectors enough time to uncover any undeclared work, forcing firms to stop using vouchers on the spot. Unfortunately, after this policy change firms that were previously misusing AWAs start buying more vouchers irrespective of inspections. Buying a voucher per worker per day firms are insured against the risk of inspections.

Next, we test whether AWAs displace regular work. Our model predicts that when AWAs become unavailable, firms may either revert back to informal jobs or hire fixed term or part-time workers. Dividing inspected firms into those that upon an inspection on average increased their use of vouchers and those that did not, we analyze what these two sets of firms do in March 2017, when vouchers were indeed abolished. Given that individual-level changes in the use of AWAs around labor inspections are a noisy measure of a systematic misuse of AWAs, these estimates are lower-bounds of the true effects.

We show that about 18 percent of inspected firms are misusing AWAs, and that "misusing" firms revert to the next most flexible work contracts, hiring about two additional fixed term workers, representing a 50 percent increase with respect to the pre-abolishment average of misusing firms. Because of these substitution effects, the total declared wage bill, which includes the cost of vouchers, shows no changes.

The paper proceeds as follows. Section 2 describes the institutional setting. Section 3

presents and solve a simple labor demand model of labor vouchers and the optimal choice of contracts, with and without the option to go shadow. Section 3 highlights also the main empirical predictions. Section 4 describes the data set used in the paper. Section 5 presents the main empirical evidence of the paper while Section 6 summarizes and concludes.

2 The Italian Alternative Work Arrangement: Vouchers

In 2008 the Italian legislator introduced AWAs in the extreme form of labor vouchers, but with considerable restrictions: employers could only spend a maximum of 5000 euro in vouchers for each employee; only students and retirees were allowed to receive vouchers, and only in the agricultural sector.

Several small changes to the initial conditions led to a steep increase in the use of AWAs. Early on the center-right government extended vouchers to all workers in the agricultural sector, not just students and retirees. More limitations were lifted in the following years, and, as shown in Figure 2, this led to a rapid growth in the monthly number of 10 euro vouchers sold: from a few thousands in 2008 to a peak of almost 20 million in 2016.

In 2009 vouchers became available in the retail sector, tourism and service sector, and for house keepers. One year later they were completely liberalized, opening up to all sectors and all workers. After a temporary setback in 2012, when the worker's 5000-euro limit was made more stringent, as it applied to the sum across all the employers and not to each employer separately, the 2014 labor reform allowed vouchers not to be related to occasional work, and their annual limit increased to 7000 euro.

The use of vouchers reached a peak in 2016, when the pressure from the labor unions to reform their use or abolish them completely intensified. In October of 2016 a first change

was imposed, as firms had to inform the Social Security Administration at least 60 minutes before using a voucher. A few months later, as the pressure intensified and a new government took over, vouchers were completely abolished.[9]

Before exploiting some of these changes in our empirical analysis, we develop a model that generates precise predictions about how AWAs influence the firms' hiring and firing decisions, as well as the decision to employ irregular workers.

3 A Labor Demand Model of Jobs, Temporary Jobs and AWAs

3.1 The Environment and the Institutions

We consider a stylised labor demand model in which a given firm is characterised by a collection of heterogeneous tasks. Each task is characterised by an individual probability $\lambda \in [0, 1]$ of becoming unproductive. Tasks may become unproductive for a variety of adverse technological reasons.[10] Productive tasks generate a homogenous output equal to y. A firm is defined by a finite number $Z/2$ (with $Z \in \mathbb{N}$) of intervals representing tasks.[11] Firms have to decide whether to activate tasks within the ordered set of intervals: $\{[\lambda_1 - \lambda_2], [\lambda_3 - \lambda_4], ...[\lambda_{Z-1}...\lambda_Z]\}$, with $\lambda_j < \lambda_{j+1}$, $j \in Z$.

In other words, we consider a firm that has drawn Z different values from a cumulative distribution function $F(\lambda)$ of tasks with probability of becoming unproductive below λ, so that $F(1) = 1$ (the corresponding density function is $f(\lambda)$). A task produces output y for the fraction of time $1 - \lambda$, while it produces 0 for the rest of the time (normalized to be

[9] A much more limited version of vouchers was reintroduced at the end of 2017.

[10] λ can also be interpreted as a technological destruction rate: with probability λ the productivity of the job drops to zero.

[11] We assume that Z is even, otherwise one can simply redefine $Z' = 2Z$.

one). The wage ω paid to each worker for each task is taken as given by the firm.[12] The model is partial equilibrium and we only focus on which type of contract the firm will offer to different λ tasks, though later we are also going to allow firms to hire undeclared workers to evade taxes.[13]

Labor regulations allow for three types of regular contract for a given task: open ended, fixed term jobs and AWAs/voucher (we use the words AWAs or vouchers interchangeably). Different contracts have different termination costs. When faced with an open ended contract that is unproductive, the firm is better off paying a firing tax equal to $-F$. In line with the Italian legislation, we assume the tax to be a multiple of the wage rate: $F = f\omega$.[14] In what follows, we shall indicate with $J^o(\lambda)$ the value to the firm of a λ task with an open ended contract. Fixed term contract are active for a fraction $1-\rho$ of the time. When a firm opens a fixed term contract to a task it commits to paying the worker for an expected duration equal to $1-\rho$, regardless of the job specific value of λ. The advantage of a fixed term contract is that the firm does not pay any firing costs when the expected duration ρ strikes. The cost associated of such contract, however, is that the firm can be forced to pay the worker even if λ strikes and productivity drops to 0. In what follows, $J^{ft}(\lambda)$ indicates the value to the firm of a task regulated by a fixed term contract. Finally, the firm can open AWAs. AWAs do not have any cost, but are characterized by an expected duration $1-\rho^v$ where ρ^v is considerably larger than ρ. In practice, it is as if AWAs can be terminated any time at no cost. In what follows, we shall indicate with $J^{awa}(\lambda)$ the value to the firm of a AWA.

In addition, the labor market is characterised by a payroll tax τ, regardless of the type of contract. The tax is paid on a flow basis by the firm and at first we assume that the

[12] The model can be easily solved with rent sharing.

[13] We are implicitly assuming that workers are ex-ante homogenous and can be employed for any of the tasks.

[14] Setting the tax to be also proportional to the duration of the contract would not change the results.

tax cannot be evaded. In section 3.2 we consider the case of tax evasion. There is a fixed cost of opening a given task equal to K, meaning that a firm will open a λ task as long as $J^i(\lambda) \geq K$, where the subscript i stands for open ended contract, fixed term or AWAs.[15]

Firms choose the type of contract to offer, conditional on each expected destruction probability λ in one of its intervals:

$$J^*(\lambda) = Max_{\{i=[o,ft,awa]\}} \left\{ J^i(\lambda), K \right\} \quad \forall \ \ \lambda \in \{[\lambda_1 - \lambda_2], ...[\lambda_{Z-1} ... \lambda_Z]\} \tag{1}$$

To solve this maximization, we need to specify the expected value of different jobs. The value of an open-ended job is

$$J^o(\lambda) = (1 - \lambda)(y - \tau - \omega) - \lambda F. \tag{2}$$

The value of the firing tax $F = f\omega$ has the restriction that $f < 1 + \frac{\tau}{\omega}$, so the firing tax for open ended contracts must be smaller than the regular tax. Otherwise the firm would be better off keeping an unproductive task and pay the worker. Conversely, the value of a λ type task under a fixed term contract is

$$J^{ft}(\lambda) = (1 - \rho)\left[(1 - \lambda)(y - \omega - \tau) - \lambda(\omega + \tau)\right], \tag{3}$$

where at rate ρ the task is destroyed at no cost. Yet, as argued above- with probability $\lambda(1 - \rho)$- the firm is forced to pay the wage until expected duration.

Finally, the value of a AWA is

$$J^{awa}(\lambda) = (1 - \rho^v)\left[(1 - \lambda)(y - \omega - \tau)\right] \tag{4}$$

[15] Assuming different Ks for the different contract does not change any of the following results.

The maximization problem satisfies the reservation property, since all the job values are monotonic and decreasing in λ. Further, one can easily show that $J^o(0) > J^{ft}(0) > J^{awa}(0)$. Further, $J^{awa}(1) = 0 > J^{ft}(1) = -(\omega + \tau) > J^o(1) = -F$. The maximization is thus an envelope of three downward sloping lines, and the firm's choice can be described by two reservation values $\tilde{\lambda}^F$ and $\tilde{\lambda}^v$. The reservation probability can be characterized as the solution to

$$J^o(\tilde{\lambda}^{ft}) = J^{ft}(\tilde{\lambda}^{ft}); \qquad \text{and} \qquad J^{ft}(\tilde{\lambda}^{awa}) = J^{awa}(\tilde{\lambda}^{awa}). \tag{5}$$

$\tilde{\lambda}^{ft}$ is the expected duration that makes the firm indifferent between an open ended job and a fixed term job. Similarly, $\tilde{\lambda}^{awa}$ makes the firm indifferent between a AWAs and a fixed term job. The intuition of this result is very strong. For a given net flow productivity $y - \omega - \tau$, firms have a strong ordering of which task to open according to their expected destruction rate, with open ended contracts suitable for tasks with long expected duration and AWAs suitable for tasks with very low duration. In addition, AWAs create labor demand opportunities that would not otherwise be exploited if the AWAs were not there. In other words, AWAs respond to firm demand of flexibility for jobs with very low expected duration. For simplicity, in what follows we indicate with $\tilde{y}$ the net flow value of the job so that $\tilde{y} = y - \omega - \rho$.[16] There exists also a maximum λ^{max} that is the solution to $J^*(\lambda^{max}) = K$, and the firm does not activate tasks for any $\lambda > \lambda^{max}$. If $\lambda_Z < \lambda^{max}$, total employment at the firm is thus $n = \sum_{j=2}^{Z} F(\lambda_j) - F(\lambda_{j-1})$, where j is even. Conversely If $\lambda_Z > \lambda^{max}$, total

[16] The threshold λs are:

$$\begin{cases} \tilde{\lambda}^{ft} & = & \frac{\rho\tilde{y}}{\rho\tilde{y}+(f\omega-(1-\rho)(\omega+\tau)} \\ \tilde{\lambda}^{awa} & = & \frac{(\rho^v-\rho)\tilde{y}}{(\rho^v-\rho)\tilde{y}+(1-\rho)(\omega+\tau))} \end{cases} \tag{6}$$

Note that existence of two thresholds- and thus two fixed term contracts- require that the duration of AWAs is sufficiently short, or that

$$\rho^v > \frac{\rho F}{F + (1-\rho)(w+\tau)}.$$

employment at the firm is thus $n = \sum_{j=2}^{T+1} F(\lambda_j) - F(\lambda_{j-1})$, where j is even and λ_T is the largest value of λ in the firm set so that $\lambda_T \leq \lambda^{max}$.

3.2 Shadow Employment and the Misuse of AWA

We now introduce the possibility of evading taxes by underreporting contracts associated to specific tasks. A shadow task allows firms to avoid paying the tax τ. In terms of the type of contract, we talk of a general λ task, and we let $\tilde{J}^i(\lambda)$ be the value of a representative λ task that is employed with a shadow job, or irregular worker, where i refers to the 3 type of contracts as above.

Further, γ is the probability of inspection, and $C(\lambda)$ is the fine imposed on the firm with undeclared work upon inspection. The main assumption we make is that $C'(\lambda) < 0$. In accordance with common practice, inspectors will charge a higher fine to workers who appear to be in a longer lasting employment relationship (and thus have lower λ). The decision to go shadow is simply

$$J^{i,s}(\lambda) = (1-\gamma)(J^i(\lambda) + \tau) + \gamma(J^i(\lambda) - C(\lambda)) > J^i(\lambda) \qquad i = \{O; F; AWA\}$$

which implies the standard conditions found in most of the shadow employment literature, namely that $(1-\gamma)\tau > \gamma C(\lambda)$ so that going shadow is optimal if the tax evaded is larger than expected fine. At the margin, this implies that if the expected duration $1/\lambda$ is sufficiently low (or the adverse shock sufficiently high), the firm operates the task with a shadow worker or unreported employment.

How does the decision to go shadow change, when vouchers become available? Let us start with the case where AWAs can be activated on the spot. When labor inspectors show up, firms have the option to declare that the task is covered by a voucher, meaning that they can use vouchers as an insurance mechanism. We define these jobs to be "gray:" they are

legitimate but hidden in black as long as the inspector does not show up. Let $\hat{J}^i(\lambda)$ be the value of an irregular task that has the option to activate the voucher conditional on inspection, or the value of a shadow job that has the option to misuse vouchers.

Formally, the existence of vouchers adds an extra choice, generating an option value. The decision to go shadow with a misuse of vouchers corresponds to the following case:

$$\tilde{J}^i(\lambda) = (1-\gamma)(J^i(\lambda)+\tau) + \gamma \underbrace{\left(Max[J^i(\lambda) - C(\lambda); J^{awa}(\lambda)]\right)}_{option\ to\ misuse\ vouchers} > J^i(\lambda). \quad (7)$$

The implicit assumption behind equation 7 is that it is impossible conditional on an inspection, to offer a regular open ended or regular fixed term job to workers who have been hidden behind a voucher. The previous maximization is certainly satisfied for AWA, while it not obvious in the case of open ended and fixed term workers. If, conversely, AWAs cannot be activated on the spot, firms that choose to misuse vouchers need to always buy a voucher to hide a worker. In this case, the corresponding value would be equal to the previous one minus the minimum cost of vouchers ($\epsilon\gamma$), where ϵ is the cost of vouchers.

The general firm problem with both shadow employment and the option to misuse is thus

$$J^*(\lambda) = Max_{\{i=[o,f,awa]\}} \left\{ J^i(\lambda), J^{s,i}(\lambda), \tilde{J}^{s,i}(\lambda), K \right\} \quad \forall \ \lambda \in \{[\lambda_1 - \lambda_2], ...[\lambda_{Z-1} ... \lambda_Z]\} \quad (8)$$

The existence of various thresholds suggest that the model is fairly flexible. The solution can be obtained in two steps: first, for each type of contract, it is possible to solve the option value problem. In the second step, the firm chooses the best contract.

3.3 Inspection, Misuse of AWA, and Their Abolition

The general lessons from the previous section are that regular employment is more likely among open ended contracts, and that misuse of voucher is certainly exploited if freely

available (by definition of equation 8). To solve an explicit example of the model, we assume a linear cost of penalty $C(\lambda) = C_0 + c_1\lambda$, with both $C_0 > 0$ and $0 \leq c_1 \leq 1$. With respect to the distribution $F(\lambda)$, the model implies that there is grey area between tasks covered by fixed term contracts and tasks covered by AWA. These are the tasks in which unreported employment is more likely to emerge in real life labor markets.

The example we carry out is described in Figure 1. The firm potentially operates in 4 different intervals (i.e. $Z = 8$ and $Z/2 = 4$ intervals are indicated in yellow in the Figure) and the solution to the labor demand problem is to use all types of contracts.[17] The top panel of Figure 1 simulates a labor demand problem of a firm that optimally hires regularly open ended and temporary workers, while it hires as shadow workers those who work on tasks above λ_5. In the second panel, right after the inspection, all the shadow workers are officially employed using vouchers. The firm pretends that these workers are regularly hired under AWA schemes. When AWA can be activated on the spot, the prediction is that on average we should expect an increase in AWA on the day of inspection.

We thus have that if

$$J^{AWA}(\lambda) > J^{i}(\lambda) - C(\lambda), \tag{9}$$

firms activate vouchers upon inspection.

Let us assume that for a subset of firms equation 9 is satisfied. This, in turn, implies that

$$\underbrace{(1-\gamma)\tau}_{expected\ tax\ evaded} \quad > \quad \underbrace{\gamma(J^{i}(\lambda) - J^{AWA}(\lambda))}_{expected\ cost\ of\ misusing\ voucher} \tag{10}$$

Yet, from Equation 9 we know that $J^{i}(\lambda) - J^{AWA}(\lambda) < C$, thus *the possibility of misusing voucher makes it more profitable to exercise the option to go shadow.*

[17] The parameters used in the example are $y = 1.4; \omega = .5; f = 1.7; \tau = 0.2$. The arrival rate of the contracts are $\delta_t = 0.18$ and $\delta_v = \delta_t + 0.6$. The entry cost $K = 0.12$, the arrival rate of inspection is $\gamma = 0.06$, while the fee function is $C = 0.4y + 0.45\lambda$.

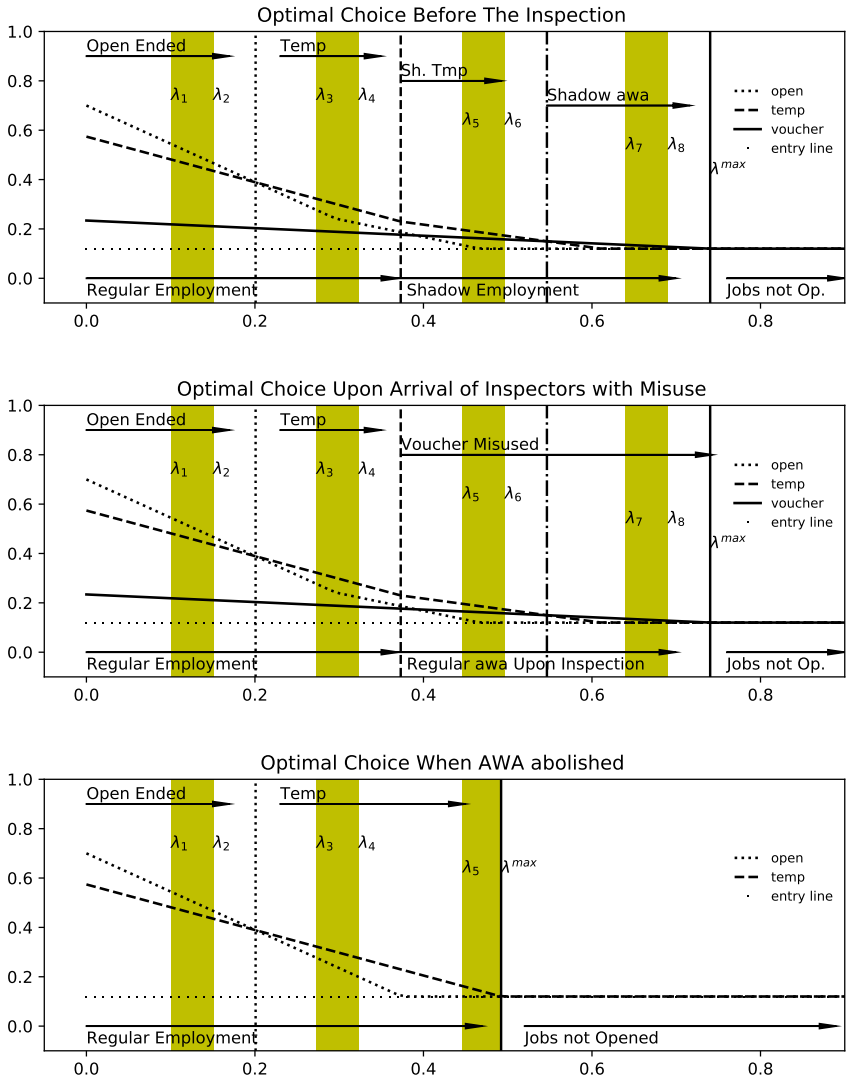

Figure 1: Optimal Labor Demand when AWA can be misused, and when they are outlawed.

Notes: The top panel simulates a labor demand problem of a firm that optimal hires regularly open ended and temporary workers, while it hires as shadow workers some temporary workers as AWA. The shaded area in the top panel corresponds to the intervals for which the firm has active tasks. In the middle panel, shadow employment is covered using AWA when the inspection starts. The bottom panel AWA are abolished and the firm operates only in three intervals, with open-ended and temporary contracts.

The lower panel of Figure 1 predicts firm's labor demand when AWA are abolished. Shadow employment goes down, in this particular case all the way to zero. At the same time, the firm increases regular employment through a larger use of fixed term employment.

Three results follow from equation 9.

1. Some firms may misuse vouchers.

2. The amount of shadow work (through the misuse of vouchers) increases.

3. Regular employment increases if vouchers are prohibited.

Next we test the predictions of our model.

4 Data

We make use of unique data from the Italian Social Security Administrative (Inps) archives, starting with the universe of firm-level employment data for the years 2014 to 2017.[18] In addition we managed to merge the firm level data with, both, the universe of labor inspection and the universe of vouchers used by each single firm.[19] Labor inspections by the Inps institute are carried out to detect full or partial evasion of social security contributions. We have information on the day the inspection started and on the outcome of the inspection (whether a fine was levied and its amount).

For each firm we know how many vouchers have been used each day (as well as the date such vouchers have been purchased).

The summary statistics with the monthly labor market averages for the period January 2015 to September 2016 are shown in Table 1.

[18]Similar data have already been used within the VisitInps programme of the Italian Social Security institute (INPS).

[19]These two sources of data are not available within the VisitInps program and have been directly managed by two of the authors, who are also Inps managers, Edoardo di Porto and Paolo Naticchioni.

There are about 1.8 million firms with an average workforce of about 7 workers. Most of these workers have permanent and full-time contracts. About 23 percent of firms (about 400,000) use at least one voucher. While the total workforce is similar to the rest of firms, firms that use vouchers use more zero-hour contracts, more temporary and part-time contracts and, therefore, fewer permanent and full-time ones. Inspected firms represent only 1.58 percent of firms and tend to be considerably larger than the rest. They have on average 43.5 workers, but in relative terms their share of full-time and permanent contracts is similar to the rest of firms.

When we focus on firms that have been inspected and use at least one voucher we are left with 3472 firms, or 0.19 percent of firms. The number of employed workers is similar to the one of inspected firms, but with a higher share of part time contracts and temporary contracts. We are going to comment later on the last two columns, where we divide the 3472 firms into those who presumably misused vouchers ("jump up") and those who did not ('jump down").

Further, since we know the exact day vouchers are used and inspections take place, we can construct daily-level data on the use of vouchers around labor inspections. Information on the number of workers within each firm, by the labor contract that has been signed, is instead measured at the monthly level.

5 Empirical Evidence

5.1 Identification

We just saw that inspections are very rare events. A randomly picked firm has a 1 in 130 chance of being inspected in a given year and about a 1 in 50,000 chance of being inspected in a given day. While some firms are more likely to be inspected than others–for example,

larger firms–for inspections to be effective, the timing of inspections is unpredictable. Thus, from a firm's perspective, the day labor inspectors enter the firm's premises is as good as random.

Given this randomness, that we can document, our model suggests a fairly simple test for whether an inspected firm is using vouchers to hide undeclared work. When facing a labor inspectors, firms employing undeclared workers should "exercise the option" to use vouchers. We compare the daily use of vouchers just before and after an inspection (the "treatment"), following firms several days before and after the inspection. We use all firms that have been inspected at least once after 2014 and that have used at least one voucher in the period.

If the timing of inspections is as good as random, the vector of observable characteristics (X_j) of a firm j should be unable to predict the exact date (t_j) of a labor inspection. We use as balance test the joint F-test that all the characteristics in the following cross-sectional regression have no predictive power ($\beta = 0$): $t_j = \alpha + \beta' X_j + \epsilon_j$.

We look at AWAs signed by firm j between 180 days pre and 90 days post an inspection that happens on day t. We start analyzing firms inspected before October 2016, which is when firms had to inform the Social Security Administration an hour in advance before using a voucher. Given that a single voucher would be sufficient to avoid the fine, our outcome is equal to one when in a given event day τ firm j uses at least one voucher, and 0 otherwise ($DV_{j,\tau} = 1\{\#Vouchers_{j,\tau} > 0\}$). Out of the 180 days before the inspection date, the first 90 days are going to serve as control period:

$$DV_{j,\tau} = \sum_{k=-90}^{90} \beta_k D_{\tau+k} + f(t) + \epsilon_{j,\tau} \ . \tag{11}$$

$D_{\tau+k}$ is a dummy variable equal to one for event day $\tau + k$ and zero otherwise. In addition, given that the time series of AWAs is far from stationary (see Figure 2) it is important to control for calendar time $f(t)$. We start using calendar time fixed effects (e.g. year, month,

day of the month, and day of week) and show how the results differ when we do not control for time or control for a simple linear time trend.

The unpredictability of the timing of the inspections is crucial when setting up the correct specification for our model. This is because focusing on just treated firms that are treated at different times, conditional on firm fixed effects, calendar time and event time of the inspection are collinear. In our setup, given the stark increasing trends in the number of vouchers sold (see Figure 2), it is important to control for calendar time. Thus, to be able to estimate the event time effects, we cannot control firm fixed effects.

Fortunately, when the timing of the event is random, and we are going to provide evidence of it, the treatment period is orthogonal to firms' characteristics (or their time invariant intercept).[20] Our strategy is similar to the identification used in Parker et al. (2013), which exploits the randomized timing of disbursement of the 2008 Economic Stimulus Payments in the US. It is also worth to notice that due to such randomness, we do not have to specify a two-way fixed effects model, thus we do not have to worry of the well-known possible biases raising from this design (see Goodman-Bacon, 2021, Sun and Abraham, 2020).

We are going to see that the treatment effects are fairly stable over time. Allowing for firm-specific constant treatment effects we can collapse the coefficients and use the whole pre-inspection period as baseline:

$$DV_{j,\tau} = \beta_j D_{\tau \geq 0} + f(t) + \epsilon_{j,\tau} \ , \tag{12}$$

where β_j represents the firm-specific post-pre inspection differences in the likelihood of using vouchers.

Based on the estimated $\hat{\beta}_j$, we identify misbehaving firms and highlight how they respond to the introduction of the SMS[21] requirement and to the abolition of vouchers. In particular,

[20] See Borusyak et al. (mimeo).

[21] SMS are Short Messages or Text Messages in mobile telecommunication.

we combine the results from the previous model with the October 2016 SMS requirement and the March 2017 abolition of vouchers, separating firms with positive changes from the rest: $\widehat{M}_j^{\eta} = 1\{\hat{\beta}_j > \eta\}$, with $\eta = 0$.[22]

In other words, we use behavioral changes driven by the inspections to identify firms that are likely to misuse vouchers: for each inspected firm we compute their average use of vouchers before and after the inspections and classify firms into those who on average increase their use and those who do not.

Our model predicts that firms that are likely to misuse AWAs ($M_j = 1$), would start buying more vouchers in October and would, a few months later, either fall back into signing cheaper part-time or fixed term contracts, revert back to hiding work altogether, or abandon the low-productivity task altogether. Our definition of misbehaving firms is subject to misclassification, both of type I and II, which biases the estimates towards 0. We are going to exploit the possibility of changing our definition of misbehaving firms.

The empirical models are simple difference-in-differences, before and after October 2016 or March 2017 between firms who presumably misused AWAs and those who did not. Since all firms share the same event date, we are not in a staggered design and do not have to worry about dynamic treatment effect biases (see Sun and Abraham, 2020). In order to assess the parallel trend assumption, we estimate event study differences with leads and lags. The number of lags are constrained by the period spanned by the data, and we exclude the event time $\tau = -2$ (respectively, August 2016 and January 2017, allowing for some anticipation effect).

The outcomes which are available at the monthly level (m) are i) total number of vouchers used, the ii) log of total wage bill (including vouchers) and the total number of workers with the following contracts: iii) part-time, iv) full-time, v) fixed-term, with vi) open-ended contracts. The event study difference-in-difference controls for firm and year by month fixed

[22]Later we test whether the results are robust to the choice of more stringent cutoffs, $\eta > 0$.

effects:

$$Y_{j,t}^{m} = \sum_{k \neq -2} \gamma_k \widehat{M_j^0} \times D_{\tau(t)+k} + \mu_j + \mu_t + \varepsilon_{j,t} \ , \tag{13}$$

where τ is the event period, which is 0 either in October 2016 or in March 2017, and $D_\tau = 1$ in event period τ and 0 otherwise.

To better understand the issue of misclassification in misusing firms $\widehat{M_j}$, we use a constant difference-in-difference model:

$$Y_{j,t}^{m} = \gamma \widehat{M_j^\eta} \times D_t + \mu_j + \mu_t + \varepsilon_{j,t} \ , \tag{14}$$

where D_t is a indicator that equals one after the abolition of vouchers. Defining $p(\eta)$ and $q(\eta)$ to be type I and II errors of the misclassified and unobserved variable M_j, it can be shown that under constant treatment effects $\frac{\widehat{\gamma}(\eta)}{1-p(\eta)-q(\eta)}$ converges in probability to γ. This implies that with constant treatment effects the η^* that maximizes $\widehat{\gamma}(\eta)$, minimizes the misclassification bias. Later, we discuss whether η can tell us something about the fraction of firms that misuses flexible work arrangements.

In our final analysis we look at how the introduction of the SMS requirement, as well as the abolition, has influenced underreporting. Since underreporting is not directly observable, we need to rely on labor inspections. Yet, only a handful of firms are inspected more than once in our data. Hence, we cannot use the same identification strategy used for labor outcomes and need to rely on coarser definition of treatment. We distinguish firms based on whether they have used any voucher before the SMS requirement.

An observation is going to be a labor inspection between January 1, 2016 and December 31, 2017, ending up with 20,819 observations. The treated firms are those that have used vouchers in the pre-SMS period, i.e. 4,269 observations. We use as outcome variable the evaded contribution, to measure underreporting and define three main treatment periods:

Pre-SMS requirement (May 1 to October 16, 2016), Post-SMS requirement (October 17, 2016 to March 17, 2017), Post-Abolition (March 18, 2017 to December 31, 2017).

5.2 The Misuse of AWAs: Evidence from Labor Inspections

5.2.1 Random Timing of Labor Inspections

We start by testing whether the timing of labor inspection is predictable. The left and right panel of Figure 3 show the linear regression coefficients of standardized variables where the dependent variable is the exact day labor inspectors start their visit. In the left panel we do not control for any function of time, while in the right panel we add year, month and day of the week fixed effects. These effects are supposed to capture trends, seasonality as well as reduced inspectorial activity during weekends. Without time effects the coefficients tend to be distributed around zero, but with fairly large standardized changes. Firms with a large fraction of part-time workers were inspected almost 3 standard deviations later compared to the other firms. Firms in the transport, retail and service sector were inspected a standard deviation earlier. Overall, the F-test has a p-value of zero, so we fail to reject random timing. But as soon as we control for calendar year, month, and day of the week, the changes become two orders of magnitude smaller, and on top of being very close to zero none of the effects is significantly different from zero. The corresponding F-test has a p-value of 0.41, and thus we fail to reject random inspection timing.

5.2.2 Vouchers and Labor Inspections

Given that the timing of inspections is random, we can estimate differences in the likelihood of using vouchers around the time of inspections. We estimate the event study differences in Equation 11 using a linear probability model.

Figure 4 plots the coefficients $\beta_{\tau+k}$, that is the difference in the use of vouchers between

event date $\tau + k$ and days between 90 and 180 prior to the inspection. Upon inspection there is a clear change in the likelihood of using vouchers. Moreover, the evidence suggests that conditional on year, month, and day of the week fixed effects there are i) no pre-trends in the use of vouchers prior to the inspection, ii) no major anticipation effects, and iii) fairly stable treatment effects.

Appendix Figure 12 shows that without calendar time controls the probability of using at least one vouchers grows over time almost linearly (right panel), but that adding a simple linear time trend is enough to center the pre-period around zero (left panel).

The increase in the likelihood of using vouchers right after an inspection is 0.88 percentage points (SE 0.16), which corresponds to a relative increase of about 18 percent. The largest changes happen on the day of the inspection and the day after, respectively 1.5 (30 percent) and 1.4 percentage points (29 percent). If we consider that over time firms may also have the option to ask undeclared workers to stay home, these are large effects. Moreover, the figure shows that these effects persist for at least 90 days.

Having many pre-inspection periods allows us to perform randomization inference. Focusing on pre-inspection data ($\tau < 0$), we sequentially generate fictitious inspection dates for $-120 \leq \tau \leq -30$ and estimate

$$DV_{j,\tau} = \beta_k D_{\tau - k \geq 0} + f(t) + \epsilon_{j,\tau} \ , \tag{15}$$

where $D_{\tau-k\geq 0} = 1$ when $\tau - k \geq 0$ and zero otherwise. The histogram of all the placebo β_ks shown in Figure 7 is centered around zero and is far away from the vertical line, which corresponds to the estimated $\widehat{\beta_0} = 0.88$. Chance is unlikely to have generated such a large change in behavior.

The data contain information about the firms, which allows us to look for heterogenous effects. In particular, we test whether pre-SMS effects differ across economic sectors and

firm's characteristics. Given the static treatment effects and the lack of pre-trends, we collapse the treatment effects and use the whole pre-inspection period as baseline:

$$DV_{j,\tau} = \beta D_{\tau \geq 0} + f(t) + \epsilon_{j,\tau} \ . \tag{16}$$

Table 2 shows that the jump is about the same in the retail sector, the tourism sector and the manufacturing sector. For the "Other sectors" the jump is slightly lower, while it is completely absent in the construction sector. This is likely to depend on the fact that in the construction sector work injuries are so common that firms prefer to buy a voucher per casual worker per day to insure them against work-related accidents, irrespective of being inspected.

In Table 3 we perform additional heterogeneity regressions. Column 1 to 4 show that the jump is fairly constant across Italian regions, although it is slightly larger in the more productive North than in the South, with the Center of Italy being in between. Columns 5 to 7 show that medium aged firms (those that started between 5 and 14 years earlier) are more likely to use vouchers to cover undeclared work compared to young and old ones. Firm size is highly predictive of the size of the effects, with large firms (more than 15 employees) being the ones with larger jumps (Column 10). Finally, the last column shows that the jump is about 40 percent larger for firms whose share of part-time workers is above the median. This is in line with the opinion of many labor inspectors that part-time work is sometimes used to hide what are truly full time workers, as it lowers the social security contributions as well as the tax burden.

5.2.3 The 60-minutes SMS Requirement

Next we analyze the October 2016 SMS reform, which introduced the 60-minutes messaging requirement, and thus broke the possibility of on-the-spot insurance. Firms would still be

able to exercise the option on a daily level, buying at least one voucher per worker, but this should not generate a jump on the day of the inspection.

Figure 5 shows, indeed, that once the SMS was introduced the jump disappears.[23] Yet, as discussed in Section 3, firms that were using vouchers to insure against the inspection risk may still do so by buying at least a voucher per worker per day, ahead of possible of inspections.

Defining "misusing" firms to be those who pre-SMS requirement upon inspection became more likely to use vouchers, we can look at how their use of vouchers changed after October 2016. Figure 6 shows the event-study differences between "misusing" and "non-misusing" firms. The estimates are noisy, possibly because the reform may have also pushed some misbehaving firm to stop using vouchers altogether, but firms appear to use 20 more vouchers per month, which represents a 20 percent increase compared to baseline, and there is evidence of some anticipation, possibly because the introduction of the October SMS requirement was known several months before implementation.

5.3 The Effect of Vouchers on Regular Employment and Total Wage Bill

5.3.1 The Effect of Vouchers on Regular Employment

We have shown that AWAs can be used to hide undeclared work, with and without informational requirements aimed at avoiding such behavior (the SMS). The next relevant question is whether vouchers i) crowd out regular work, ii) hide work that would otherwise be fully undeclared (black rather than gray), iii) give rise to new job opportunities (whether declared

[23]The reason why the whole figure is shifted below zero is that the post-SMS period is subject to strong negative trends in the use of vouchers, and calendar time fixed effects (year, month, day of the month, and day of week) do not capture such changes. In order to center the figure around zero, the Appendix Figure 13 uses -60 to -30 event days as a comparison period.

or undeclared).

Theory predicts that in the event that AWAs become unavailable, firms should hire some of the "gray" workers using the next most flexible work arrangement (see Figure 1).

The next most flexible work arrangements is arguably the combination of temporary and part-time labor market contracts.

Our event-study difference-in-difference Figure 8 shows that temporary part-time contracts go up by about 1, representing a 50 percent increase compared to the pre-abolition average. The difference between "misusing" and "non-misusing" in the total number of temporary part-time employees is fairly flat in the months leading to the March 2017 abolition of the vouchers and then increases by about one additional worker.

These workers also drive the results for the total number of workers (see Figure 9) as that change is only slightly above 1 (the average number of workers is around 40). Figure 10 separates the effects for the two dimensions: temporary vs. permanent (upper panel) and part-time vs fulltime (lower panel). The largest effects show up for temporary workers. For these workers the difference-in-differences are close to 2, an almost 50 percent change. There are no effects, and if anything negative effects, for open-ended, permanent contracts (upper right panel). As for part-time workers versus full-time ones, both groups show similarly sized effects, indicating that firms seek flexibility with respect the duration of the labor contracts.

As for the total wage bill, including the cost of AWAs, we find no evidence of significant changes (see Appendix Figure 14). This suggests that the labor costs saved on vouchers is perfectly offset by the labor costs on all other contracts, which might explain why these firms reduce the number of open-ended contracts.

5.3.2 Misclassification Error

Based on the evidence on the closest substitute contract for AWAs, we use temporary and temporary part-time contracts to assess the importance of misclassification errors. Misclas-

sification is driven by the way we identify "misusing" firms. When defining $\widehat{M}_j^{\eta} = 1\{\hat{\beta}_j > \eta\}$, with $\eta \geq 0$, type I and II errors depend on η. Thus, we estimate Equation 14 varying ηs between 0 and 7.5 percent, which corresponds to respectively 50 and 8 percent of misusers. Figure 11 shows that no matter whether we use temporary contracts or temporary and part-time workers the optimal η is close to 0.03, corresponding to a fraction of misbehaving firms of about 18 to 19 percent. According to the R-squared, there are two ηs that fit the data best, 0.015 and 0.03. The first one corresponds to a fraction of misbehaving firms that is slightly below 30 percent. When compared to the coefficients estimated with $\eta = 0$, the coefficients increase by about 60 percent, which implies that the relative changes in the number of temporary and temporary part-time workers increase from 50 percent to more than 75 percent.

5.4 The Effect of Vouchers on Evasion

Since the yearly probability of inspection is less than 1 percent, between 2014 and 2017 only a handful of firms are inspected more than once . This implies that we cannot use the two-step identification strategy, where we first construct a measure of misbehavior based on the use of vouchers when inspections take place.

We can only compare the amount of evaded social security contributions for firms that have or have not used any vouchers between January and October 2016. Separating inspected firms based on whether they have used at least one voucher, we are using a weaker identification, as only a fraction of firms that use voucher tend to hire "gray" workers. In our sample, 4,269 have used at least one voucher and 16,550 have not.

During the baseline period (January to October 2016) firms that have not used vouchers evaded on average 26,835 euro. Table 4 shows that, compared to control firms, treated firms appeared to evade more when vouchers were abolished, while at the height of the use of

vouchers (Pre and Post-SMS) evasion appeared on average lower. It is interesting to notice that during this period control firms were actually fined larger amounts, as if to compensate the reduced fines in the treatment group. The results change little when we control for the share of part-time workers, fixed-term workers, its interaction, average wages and a cubic monthly trend.

6 Conclusions

In an attempt to keep up with an increasing demand by firms for flexible work contracts, legislators around the world are coming up with labor contracts that at times allow firms to hire workers for just a little amount of work, i.e. on-call work, zero-hour work, labor vouchers, mini-jobs, etc.

Recent research shows that these contracts help firms deal with rapidly changing demand and help workers, especially in the gig economy, to smooth their work. Yet, the same research has also mentioned that these contracts may lead to poor career development prospects, stagnating wages, and excessive exposure to uninsurable income risk, in the short and long run (Boeri et al., 2020). This paper documents an additional important risk: these labor contracts may complicate the job of labor inspectors, whose task is to uncover undeclared work. We show that upon random inspections firms use alternative work arrangements to hide undeclared work. This counteracts the expected reduction in undeclared work coming from the reduced hiring and firing costs.

But there is also evidence that small changes to the bureaucratic requirements of alternative work arrangement can partially close these loopholes. When employers were forced to signal such arrangements in advance, labor inspectors would have enough time to identify undeclared workers. This forced misbehaving firms to buy at least one voucher every day, to avoid being fined by the inspectors. We estimate that 18 percent of firms with AWAs, used

AWAs to hide undeclared work, and that these firms hired more fixed-term and part-time workers when AWA were banned. This evidence and the evidence that the total wage bill stayed constant, suggest that firms did not lower the demand for declared labor when AWAs became unavailable.

Overall, our results suggest that in countries with large shares of undeclared work, alternative work arrangements that apply *erga omnes*, without restrictions, may bring more harm than good. Moreover, if firms use vouchers only when inspected, restricting the total number of vouchers used by workers or firms to be below some threshold, which has been Italy's primary restriction, is unlikely to have any bite. Rules that restrict the use of AWA to certain categories, like students or retirees, are more likely to balance the need for flexibility with legality and the protection of employment rights.

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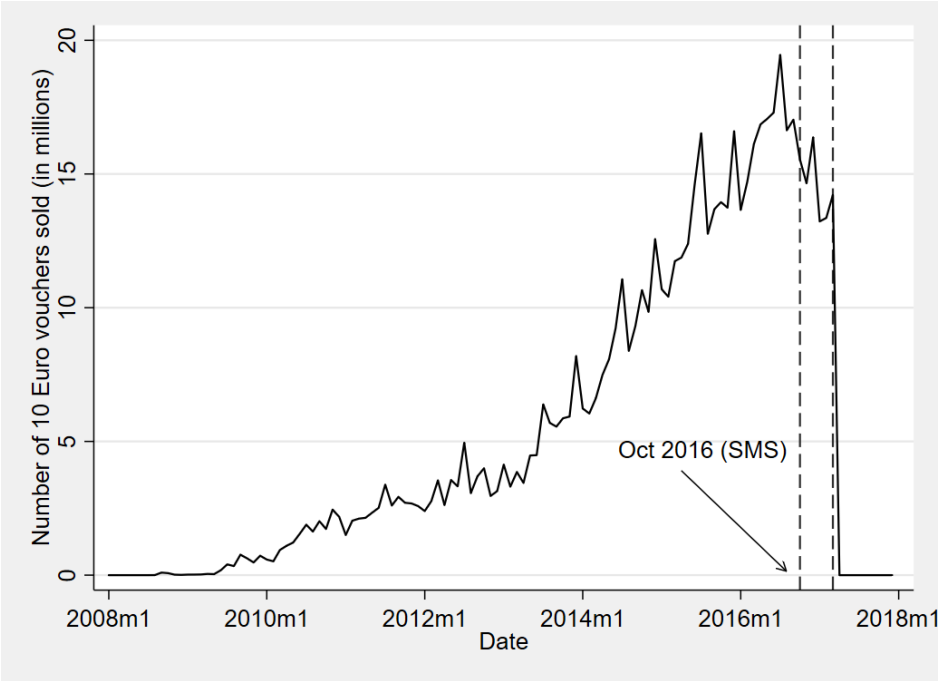

Figure 2: Vouchers Sold

Notes: The figure plots the monthly total number of 10-euro vouchers sold.

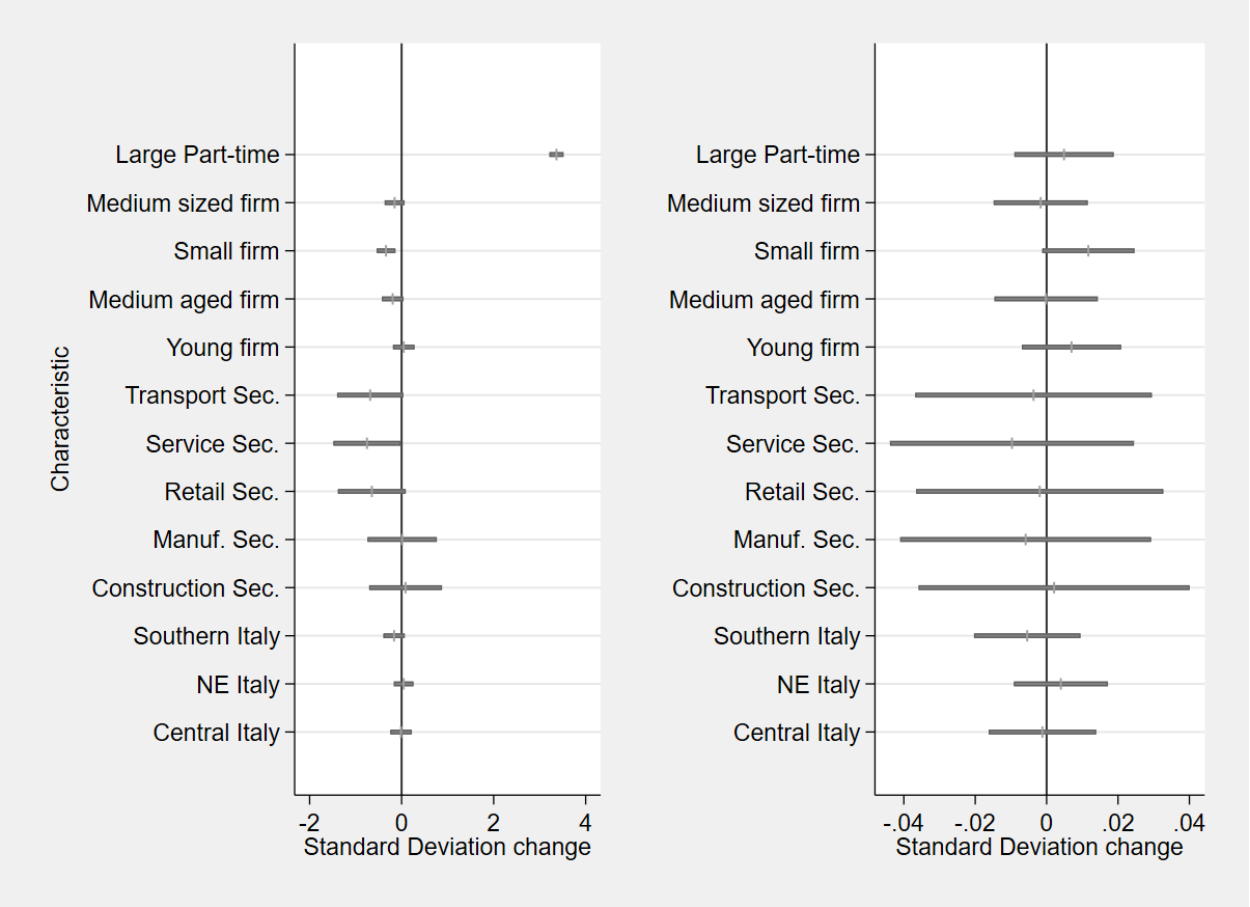

Figure 3: Balance Test

Notes: The figure plots the coefficients of standardized time of inspection on firm characteristics. The regression on the right controls in addition for year, month, and day of the week fixed effects. The p-values for the F-test that sets all coefficients equal to zero are respectively 0 and 41 percent. The vertical bars represent the 95 percent confidence intervals based on robust standard errors.

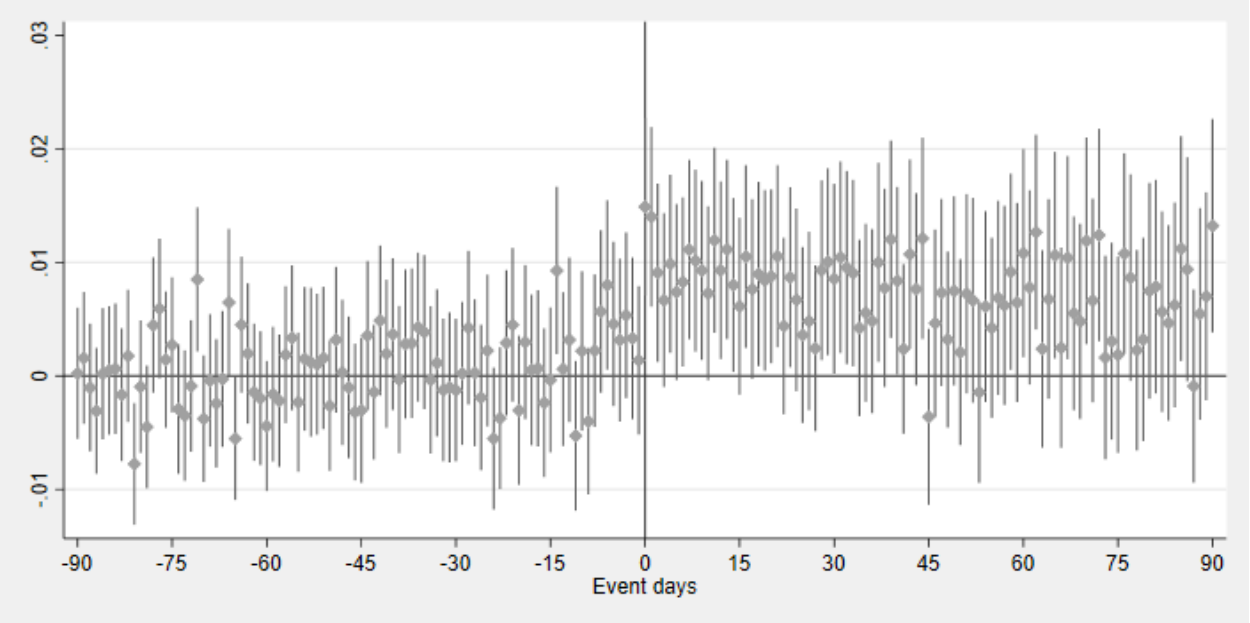

Figure 4: Event Study pre-SMS

Notes: The figure plots event study coefficients, where the event is a labor inspection. The excluded time period is between 180 and 90 days prior to the inspection. The regression controls for year, month, and day of the week fixed effects. Standard errors are clustered at the firm level.

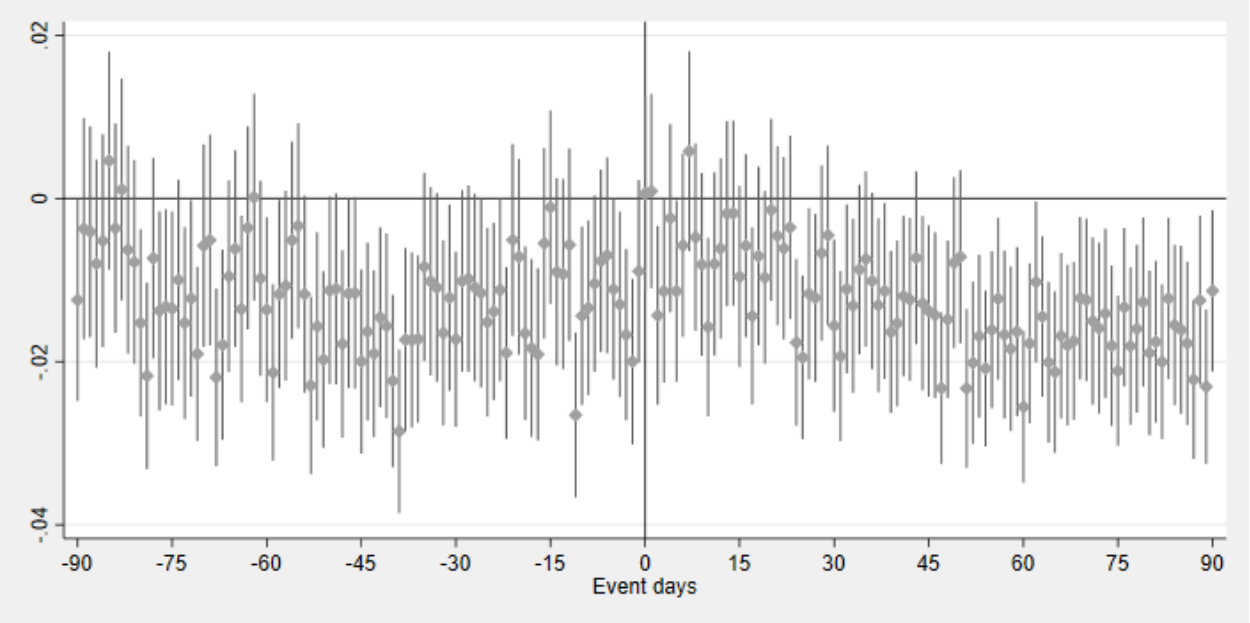

Figure 5: Event Study post-SMS

Notes: The figure plots event study coefficients, where the event is a labor inspection. The excluded time period is between 180 and 90 days prior to the inspection. The regression controls for year, month, and day of the week fixed effects. Standard errors are clustered at the firm level.

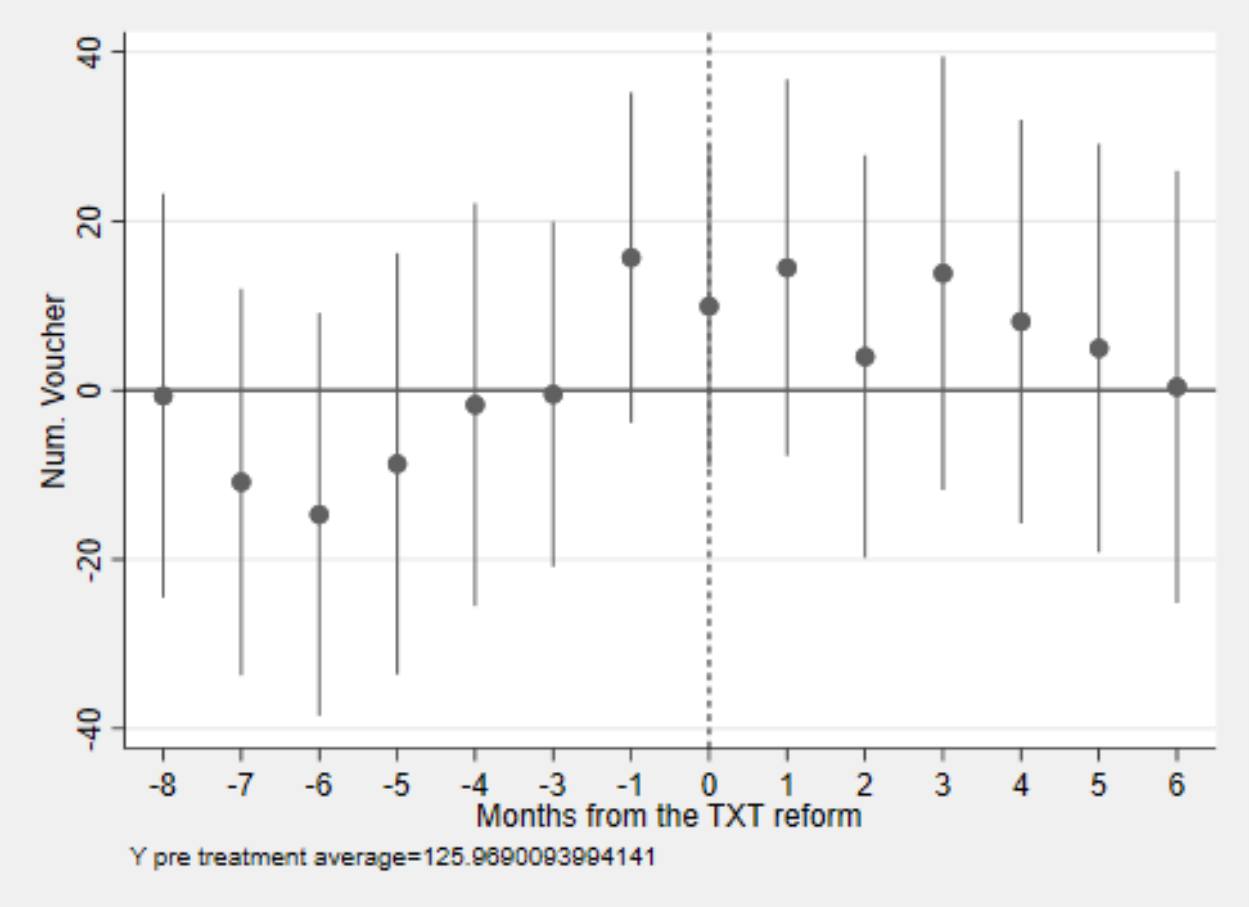

Figure 6: Event Study post-SMS

Notes: The figure plots differences in the number of vouchers used at firms that on average "mis-used" vouchers and those that did not, 8 months before and 6 months after the abolition of vouchers. Standard errors are clustered at the firm level.

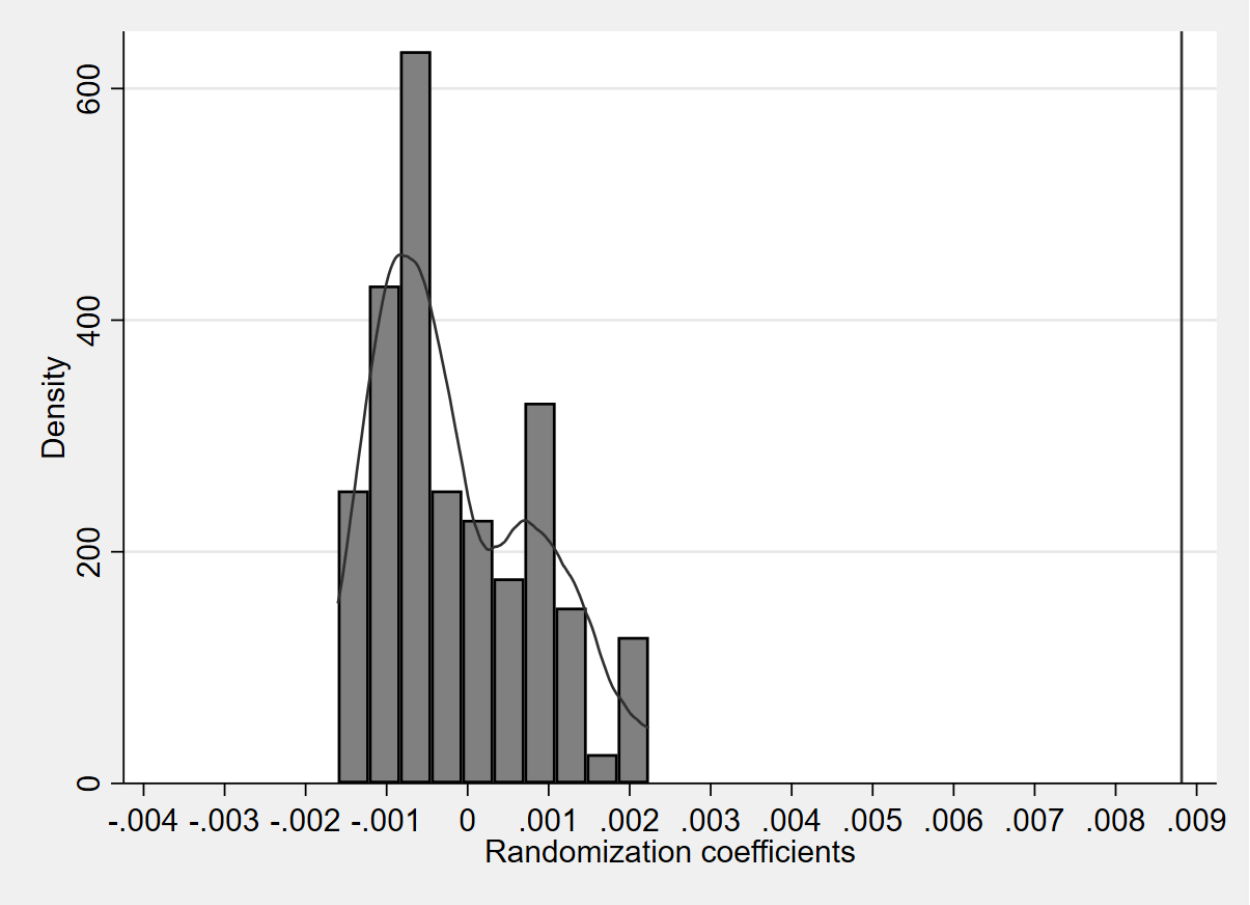

Figure 7: Randomization Test

Notes: The figure plots the density of the randomization coefficients in a simple difference model: $DV_{j,\tau} = \alpha + \beta D_{\tau-t} + \epsilon_{j,\tau}$ where $t < 0$ measures the event time of inspection (it's zero the day of the inspection), while $D_{\tau-t}$ takes value one t days before the inspection. The vertical line on the right corresponds to the coefficient when the timing of the labor inspection is correct ($t = 0$) and the full sample is used ($t \lesseqgtr 0$).

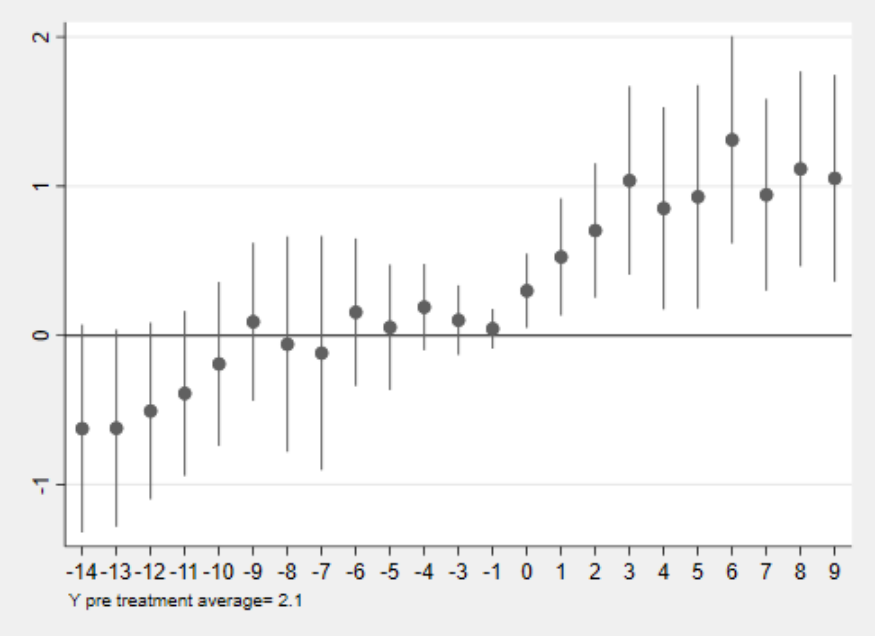

Figure 8: Part-time and Temporary Contracts Around the Abolition of Vouchers

Notes: The figure plots differences in the total number of part-time and temporary workers employed at firms that on average "mis-used" vouchers and those that did not, 10 months before and 9 months after the abolition of vouchers. Standard errors are clustered at the firm level.

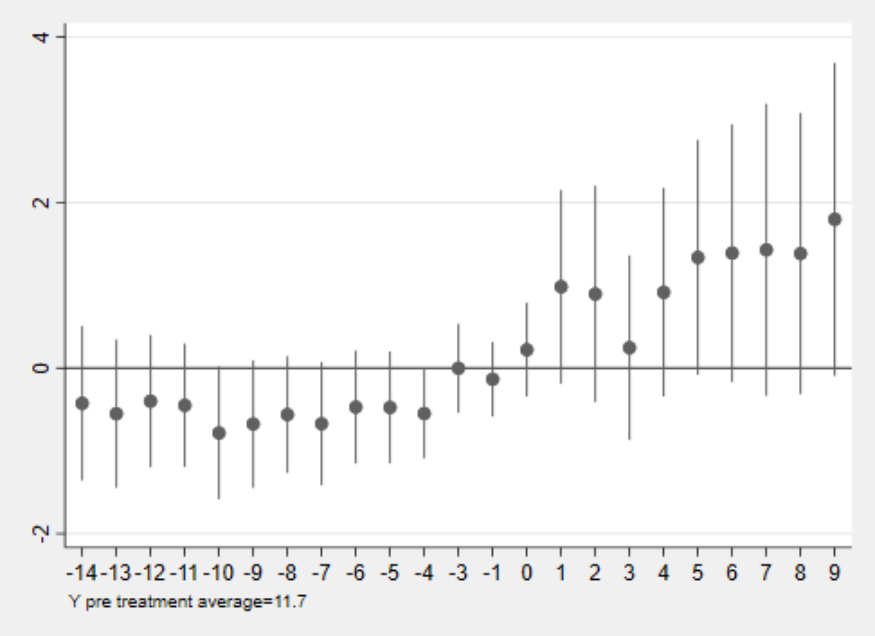

Figure 9: Workers Employed around the Abolition of Vouchers

Notes: The figure plots differences in the total number of workers employed at firms that on average "mis-used" vouchers and those that did not, 10 months before and 9 months after the abolition of vouchers. Standard errors are clustered at the firm level.

Figure 10: Event Study around the Abolition of Vouchers

Notes: The figure plots differences in the number of workers employed at firms that on average "mis-used" vouchers and those that did not, 10 months before and 9 months after the abolition of vouchers. Standard errors are clustered at the firm level.

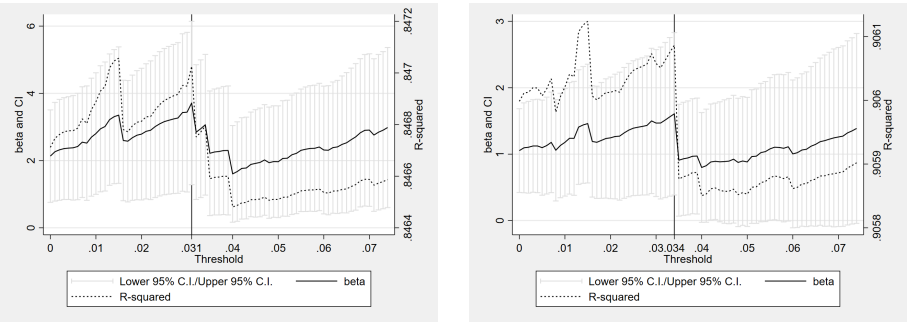

Figure 11: Workers Employed around the Abolition of Vouchers

Notes: The figures plot differences in the total number of temporary workers (left panel) as well as temporary part-time workers (right panel) employed at firms that on average "mis-used" vouchers and those that did not, and the corresponding 95 percent confidence intervals. The definition of misusing firm $\widehat{M}_j^{\eta}$ varies based on the η threshold: $\widehat{M}_j^{\eta} = 1\{\hat{\beta}_j > \eta\}$. Standard errors are clustered at the firm level.

Table 1: Summary Statistics

Sample	Full	Vouchers	Inspected	Both	Jump up	Jump down
Workforce	7.18	7.27	43.49	40.41	36.55	45.98
Monthly Wage bill	14,387	12,131	95,185	62,261	52,958	75,704
Average wage	1,347	1,273	1,316	1,248	1,249	1,246
Monthly Wage bill (FTE)	15,935	13,891	103,619	74,550	65,846	87,128
Workforce (FTE)	6.27	6.21	38.75	33.36	29.21	39.35
Temporary workers	1.10	1.81	8.51	16.44	11.70	23.29
Part-time workers	2.13	2.68	11.13	16.90	17.15	16.53
Permanent workers	6.08	5.46	34.97	23.97	24.85	22.69
Full-time workers	5.05	4.59	32.36	23.51	19.40	29.45
Monthly number of vouchers	27.75	97.60	299.78	251.91	269.53	238.97
Voucher per workforce	4.43	15.73	7.74	7.55	9.23	6.07
Number of firms	1,836,191	416,930	29,063	3,472	1,471	2,001
Fraction	100%	22.7%	1.58%	0.19%	0.08%	0.11%

Notes: This table shows averages for different samples using yearly 2015 and 2016 data. The samples are the following: "Full" is the universe of private firms in 2015 and 2016; "Vouchers" are firms that have used at least one voucher in those years; "Inspected" are firms that have been inspected (almost all are inspected only once); "Both" are firms that use at least on voucher and have been inspected. "Jump up" and "Jump down" are firms that either increase of decrease the use of vouchers upon inspection. "FTE" stands for full-time-equivalent

Table 2: Event Study Regressions by Sector

	(1)	(2)	(3)	(4)	(5)
Sector	Manufacturing	Construction	Retail	Tourism	Other Services
Post-Inspection	0.013***	-0.002	0.012**	0.011***	0.006*
	(0.003)	(0.003)	(0.006)	(0.002)	(0.003)
Constant	0.032***	0.021***	0.030***	0.054***	0.051***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)
Observations	157,329	98,087	208,194	614,758	255,718
R-squared	0.014	0.013	0.010	0.022	0.016
Mean dep. var.	0.0381	0.0201	0.0352	0.0592	0.0541

Notes: Clustered standard errors (by firm) in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Event Study Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Subsample	South	Center	Nort-East	North-West	Young firms	Medium-aged f.	Old firms	Small firms	Medium-sized f.	Large firms	Above-median use of PT
Post-Inspection	0.009**	0.008**	0.010***	0.011***	0.009***	0.011***	0.008**	0.006***	0.008***	0.012***	0.014***
	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)	(0.004)	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)
Constant	0.047***	0.046***	0.047***	0.043***	0.045***	0.048***	0.044***	0.035***	0.044***	0.058***	0.057***
	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.003)	(0.002)
Observations	255,262	256,735	409,649	347,459	581,120	414,932	273,053	461,643	373,953	433,509	446,903
R-squared	0.015	0.015	0.014	0.014	0.014	0.013	0.017	0.012	0.016	0.017	0.005
Mean dep. var.	0.0495	0.0477	0.0504	0.0461	0.0477	0.0498	0.0481	0.0368	0.0466	0.0609	0.0574

Notes: Clustered standard errors (by firm) in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Vouchers and Evaded Contributions

	(1)	(2)	(3)	(4)
Post-Abolition*TREATED	27,979*	30,423**	34,626**	32,968*
	(15,351)	(15,265)	(16,874)	(16,868)
Post-SMS*TREATED	-15,174**	-15,402**	-12,536	-14,354*
	(7,125)	(7,121)	(8,005)	(8,037)
Pre-SMS*TREATED	-8,421	-8,589	-4,940	-5,416
	(6,573)	(6,519)	(6,681)	(6,685)
TREATED	3,889	4,049	394	1,183
	(4,430)	(4,431)	(4,709)	(4,723)
Post-Abolition	23,673***	28,974***	24,610***	24,513***
	(6,095)	(7,492)	(8,022)	(8,218)
Post-SMS	17,505***	11,943**	6,375	7,155
	(5,240)	(5,660)	(6,088)	(5,981)
Pre-SMS	15,516***	22,758***	16,314**	16,476**
	(4,662)	(7,327)	(6,479)	(6,455)
Observations	20,819	20,819	18,109	18,109
Controls for employment	NO	NO	NO	YES
Controls for wages	NO	NO	YES	YES
Cubic monthly trend	NO	YES	YES	YES

Notes: The dependent variable is the amount of evaded contributions for each inspected firm. The baseline period is January to October 2016. The Pre-SMS period goes from October 2016 to March 2017, while the Post-Abolition period starts in March 2017. Standard errors are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

A Appendix

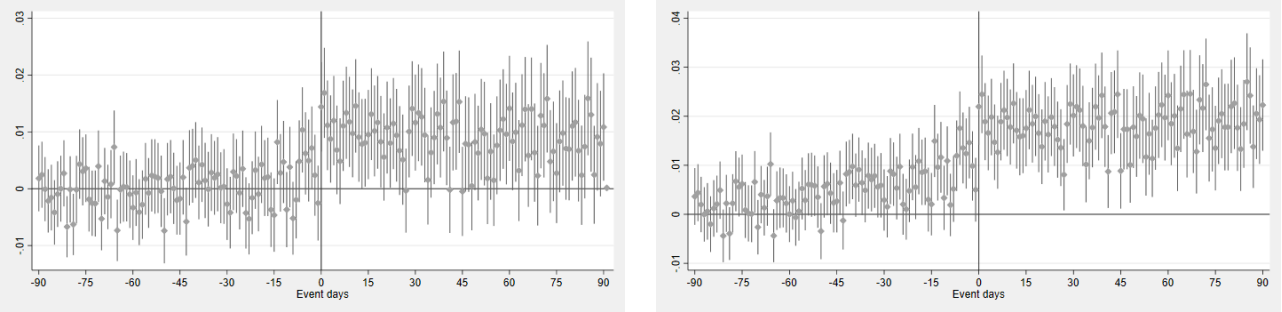

Figure 12: Event Study pre-SMS

Notes: The figure plots event study coefficients, where the event is a labor inspection. The excluded time period is between 180 and 90 days prior to the inspection. Standard errors are clustered at the firm level. The figure on the left controls for calendar time t (in Eq. 11, $f(t) = \alpha t$), the figure on the right has no additional controls (in Eq. 11, $f(t) = 0$).

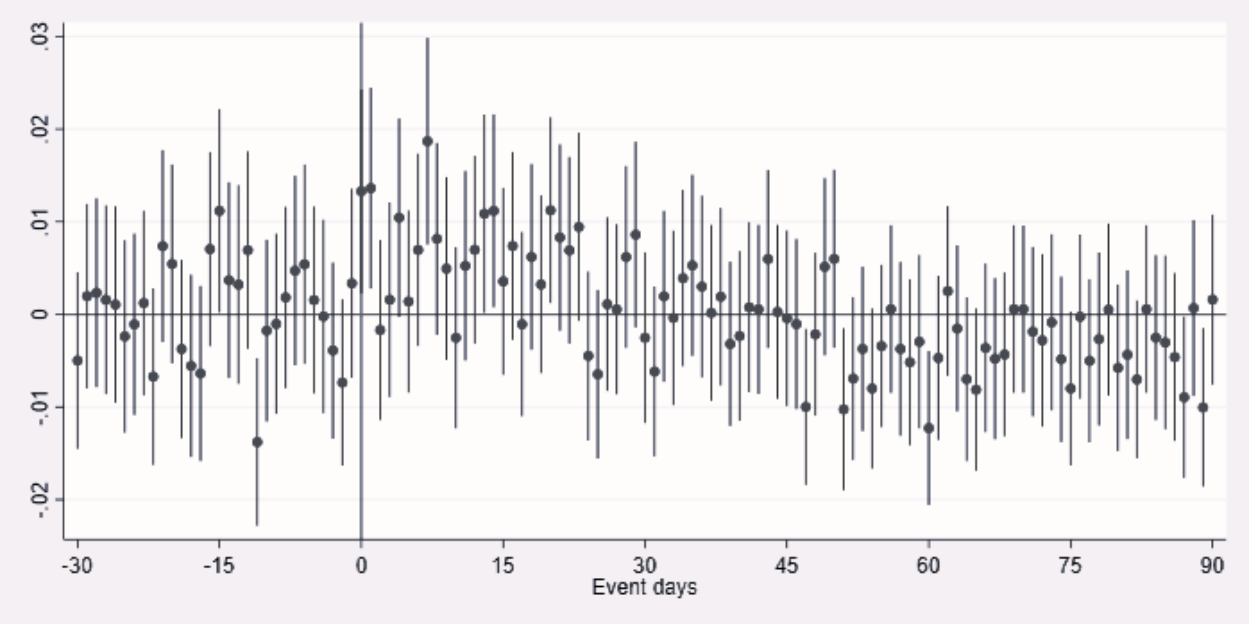

Figure 13: Event Study post-SMS

Notes: The figure plots event study coefficients, where the event is a labor inspection. The excluded time period is between 60 and 30 days prior to the inspection. Standard errors are clustered at the firm level.

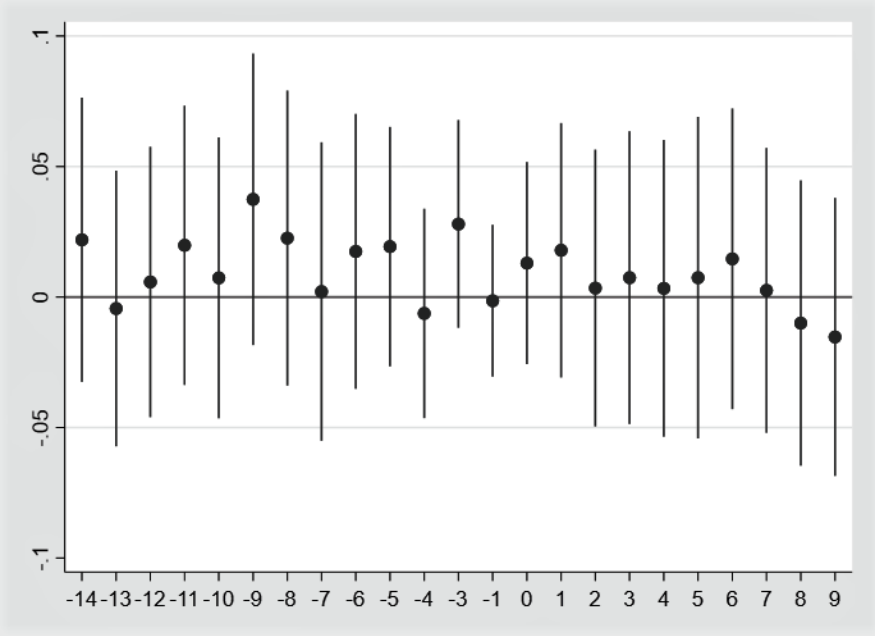

Figure 14: Log-Total Wage Bill Around the Abolition of Vouchers

Notes: The figure plots differences in the total wage bill between firms that on average "mis-used" vouchers and those that did not, 10 months before and 9 months after the abolition of vouchers. Standard errors are clustered at the firm level.