The Italian Blitz: audit publicity and tax compliance, evidence from a natural experiment

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Abstract

Tax evasion is a major problem faced by governments across the world, and many strategies have been used to minimize the level of evasion. One such strategy that has been widely implemented in Italy is the so-called audit blitz. The Italian blitzes in the last years shared many common features, but differed in their level of publicity. We use confidential data on sectorial-level Value Added Tax payments in two cities to estimate the effect of blitz publicity on tax compliance. Using a non-parametric Difference-in-Differences identification strategy, we find that blitz publicity has a positive effect on compliance in the month immediately after the blitz. The results suggest that increasing awareness on future blitzes via the media can be an important instrument in the hands of tax authorities.

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1 Introduction

Tax evasion is a worldwide phenomenon with significant budgetary, efficiency and equity implications. For example, it is estimated that closing the tax-gap would provide resources corresponding to approximately 60% of the UK's 2013 budget deficit, 155% of the US 2006 budget deficit and 180% of the 2015 Italian budget deficit.[1] There is also evidence that tax evasion affects allocative efficiency by influencing market prices (Kopczuk et al., 2016) and the elasticity of labor supply to tax rate changes (Doerrenberg and Duncan, 2014). While precise measures of tax evasion are not available for all countries, it is commonly accepted that tax evasion is widespread and that it is a major problem especially in developing countries. Given the implications of this phenomenon, a vast academic literature has focused on understanding its determinants (see Hashimadze et al., 2013 for a comprehensive review).

While many contributions highlight the importance of various "hard constraints" (such as the severity of penalties and the likelihood of detection - Allingham and Sandmo, 1972 - or the tax rate - Yitzhaki, 1974) in influencing the decision to evade, it is generally understood that such decision is also sensitive to other factors, such as the information taxpayers receive on the activities of tax authorities. Information can reach the citizen through three main channels: administrator-to-taxpayer communications, taxpayer-to-taxpayer communications, and media reports. Communication can provide information on audit frequencies and audit targets, which may affect taxpayers' perceived audit probability. Additionally, communication regarding an auditing event can influence an individual's perception of the proportion of tax evaders in the population, which may affect the individual's perception of the social norms governing tax evasion. Although administrator-to-taxpayer and taxpayer-to-taxpayer communication have been shown to affect tax evasion

[1]The tax gap is defined as the difference between the amount of tax that should, in theory, be collected by the revenue agency, and what is actually collected. For UK: tax gap data are taken from Table 1.1 in the HMRC's document *Measuring tax gaps tables 2015* and deficit data are from Table T4.35 in the Office for Budget Responsibility's *November 2015 Economic and fiscal outlook: Charts & Tables*. For US: tax gap data are from the Internal Revenue Service and deficit data are from Table 1.1 of the *Historical Tables* produced by the Office of Management and Budget. We use 2006 budget deficit because the most recent tax gap estimates are for the year 2006. For Italy: tax gap data are taken from *Scenari economici n. 25, Dicembre 2015* of the Centro Studi Confindustria, and deficit data are from Table 4 of the December 2015 "Bollettino Statistico" ("*Statistical Bulletin*") published by the Bank of Italy.

(e.g. Myles and Naylor, 1996, Slemrod et al., 2001, Alm et al., 2009), there is very little information on the extent to which media coverage influences the decision to evade.

The current paper contributes to the tax evasion literature by identifying the impact of media coverage on tax compliance. We are particularly interested in knowing whether audit publicity - through media coverage - affects the decision to evade. Our identification strategy is based on an evaluation of audit blitzes which recently took place in Italy. Blitzes are defined as a set of unexpected tax verification activities taking place within a short period of time, in a small area, and on some predefined business sectors. Importantly, in recent years, some blitzes in Italy were carried out in private, while others received significant media coverage (we will refer to them as "public blitzes"). This allows us to identify the effect that publicity has on compliance.

Our dataset is provided by *Agenzia delle Entrate* (the Italian Revenue Agency) and includes data for two blitzes which took place in the Italian cities of Milan and Genoa, covering 18 business-to-consumer (B2C) sectors. We focus on these for several reasons. First, both blitzes took place in January 2012. Second, they were both unannounced[2] and had similar characteristics, including focusing on a similar set of industries. Third, they differed greatly in their media coverage, with the Milan blitz being expressly and extensively publicized in the media. Finally, Genoa and Milan are part of the same Italian macro region (North-West), and are comparable in socio-economic terms.

We identify the effect of media-publicity through a Difference-in-Differences strategy which compares the behaviour of taxpayers in the two cities using a non-parametric approach.[3] We find evidence of a positive and significant effect of publicity on VAT payments in the month after the blitz[4]: VAT payments in Milan increased relative to payments in Genoa. We estimate this publicity effect to be at least 6 million euros only in the first post-blitz month and in the sectors considered. The findings are robust to a range of alternative specifications.

[2]As we explain in Section 2, an important public blitz took place in Cortina d'Ampezzo before those of Milan and Genua, but no information was then revealed about if and where other blitzes were going to be conducted.

[3]We adopt a non-parametric approach rather than the standard difference-in-differences approach for reasons related to the structure of our dataset and the limited support for normality assumptions. See Section 4 for more details.

[4]Notice that VAT payments done in February refer to purchases and sales of January.

Our findings have important implications for both academics and policy-makers interested in understanding the determinants of tax evasion. With respect to the academic literature, we find that tax evasion is affected not only by the magnitude of the audit probability (see Kleven et al., 2011, Section 6.2), but also by the publicity of the audit: the publicity of audits increases compliance keeping fixed the frequency, and hence costs, of audits. Our results are also consistent with empirical evidence on the publicity of audit strategies. For example, there is evidence that Germany experienced an increase in voluntary disclosure of evaded taxes after publicizing the purchase of CDs containing a list of potential tax evaders.[5]

Our results also contribute to policy debates regarding the Italian public blitzes. For example, the Italian government and tax officials released declarations stressing the fact that "public" blitzes were to be interpreted as a consequence of a "sharp change in the public attitude towards tax evasion", aimed at "enhancing compliance and decreasing the social scandal called tax evasion". This point of view was shared by some media analysts, who welcomed this "change of attitude" of the authorities, emphasized by an official press release (Agenzia delle Entrate, 2012) which mentioned the large number of taxpayers who were found to be non-compliant during the blitz (a share of 48%) and the strong increase in reported sales for audited shops (+44% on average, compared to the previous week). Our results confirm that the publicity of the blitz increased compliance, at least in the short term. This suggests that the publicity of the blitz had a positive effect on compliance, although policy-makers might need to run public blitzes continuously in order to produce a lasting effect. In order to evaluate the overall cost effectiveness of blitzes, precise estimates of their implementation costs would be needed. However, to the extent that publicizing a blitz is costless,[6] our results suggest that policy-makers should publicize all blitzes. It is then possible that the persistence of the effect depends on the frequency with which a specific area is blitzed.

The remainder of the paper proceeds as follows. Section 2 provides institutional details that support our decision to focus on Italy and, in particular,

[5]See `http://edition.cnn.com/2008/WORLD/europe/02/19/tax.evasion` and Langenmayr, 2015 and Bethmann and Kvasnicka, 2016.

[6]Of course, it is possible that publicizing a blitz imposes costs on economic actors in the market place over and beyond the additional tax liability. For example, some critics pointed at the alleged negative impact of the control procedures on the regular functioning of the economic activities, and on the alleged excess of "spectacularization" of the blitz.

on the cities of Milan and Genoa. The data are described in Section 3, our identification strategy in Section 4, and results in Section 5. Finally, we conclude in Section 6.

2 Tax evasion and blitzes in Italy

Italy provides a perfect context for testing the effect of media-publicity on tax compliance: tax evasion is widespread, the government regularly conducts blitzes, and some recent blitzes differed substantially in their media coverage and hence publicity. This section describes each of these features in detail.

Tax Evasion in Italy Italy is known to have one of the highest tax evasion rates among OECD countries. A consistent and relevant component of evasion is the VAT gap, i.e. the percentage difference between expected (without evasion) and actual VAT revenues. Italy is estimated to have the fifth highest value among European Union countries (CASE, 2015), despite displaying a decreasing trend in recent years (Pisani, 2014). Although evasion is a nationwide issue in Italy, the propensity to evade is well known to be heterogeneous across regions and sectors (Pisani, 2014; Marino and Zizza, 2012). According to estimates provided by the Italian Revenue Agency, the regional propensity to evade tends to be lower in Northern Regions and higher in Central and especially in Southern Regions. For example, in the period 2007 to 2010, the estimated VAT gap is below the national average of 26.04% in six of the eight Northern regions and above 32% in six of the eight Southern Regions (D'Agosto et al., 2014). Of particular interest to us in this paper are the North-Western regions of Lombardia, whose main city is Milan, and Liguria, whose main city is Genua; these two regions have VAT gaps of 21.18% and 22.82%, respectively (D'Agosto et al., 2014). That these two cities have similar VAT gaps is important for our identification strategy; it suggests that our results are not being driven by pre-intervention differences in evasion between the two cities.

As for heterogeneity across sectors, the available evidence indicates that B2C sectors are more prone to tax evasion: indeed, they are less exposed to the so-called "VAT paper trail", which is instead quite effective in reducing incentives to hide the true amount of business-to-business (B2B) transactions (Pomeranz, 2015).

Blitzes The Italian government has implemented many policies to address the widespread tax evasion that characterizes the Italian economy. These include campaigns aimed at increasing tax morale and public consciousness, the use of presumptive taxes, increasing in the penalties for evaders, and increasing the frequency of tax verification activities via blitzes.

A blitz can be defined as an unexpected round of tax verification activities conducted in a limited region (usually, a city or a set of cities), in a short timespan, and targeting some specific categories of economic activity. The OECD (2014) defines a "tax verification activity" as comprising "all the activities typically undertaken by revenue bodies to check whether tax liabilities were properly reported" including, in turn, "tax audit" or "tax controls", i.e. field, desk and correspondence audits, and other activities. These, in turn, range from in-depth fraud investigations to inspections of books and records. Blitzes can be included among these "other activities".

Blitzes in Italy are usually conducted by the *Agenzia delle Entrate* in collaboration with a specialized finance police force (*Guardia di Finanza*) and sometimes with the support of inspectors from the Ministry of Labour as well as local policemen. During blitzes, the agents check for the correct issuance of receipts,[7] the integrity of cash registers, the regular updating of books of accounts, the congruity of declarations previously made concerning several aspects of the shop (e.g., number of rooms and electrical appliances), the presence of workers not on the books, as well as some checks outside the shops themselves (e.g., presence of abusive sellers, sales of counterfeit goods). Failure to issue a receipt results in a fine of at least 150 euro plus the temporary closure for 15 days of the activity if three such infractions are caught in a five years period. Additionally, verification activities under an audit blitz can have longer term consequences; e.g., uncovered infractions may trigger a more intense audit of the affected firm. Blitzes not only lead to economies of scale in organizing audits, but they also provide the revenue agency with a comprehensive snapshot of fiscal compliance for a given geographic area or economic sector at a given point in time. Still, because they usually target only a few dozen to a couple hundred economic activities, the direct effect that a single blitz can have on the total amount of tax evasion is typically negligible.

[7]This is done either by checking clients exiting a shop, or by agents in plain clothes inside the shop. Furthermore, once agents show up, their presence naturally enforces the release of receipts, and this allows a comparison of the amount of registered sales with the amount of registered sales in previous days, by analyzing the cash registers.

In order to ensure unpredictability of the blitz, the authorities vary the time and day of each blitz; blitzes can take place in the morning, in the evening or at night, and can occur on any day of the week. Although no detailed data are available, blitzes are not an uncommon instrument among Revenue Agencies, as they can be an occasion to also verify compliance with obligations to report the number of employees and their working hours. In Italy, at least 1,800 economic activities, located in almost all Regions, were inspected during blitzes which took place in the first half of 2012 alone; most of them were restaurants, discotheques and pubs. This corresponds to approximately 0.5% of all fiscal checks conducted yearly on Italian businesses, and 1% of those targeted at small and medium businesses (Italian Government, 2013).

Publicity The Italian blitzes in the last years varied sharply in the amount of media attention they received. The majority of blitzes are usually private, in the sense that they do not receive much media coverage, and often only shop sellers who are affected by the blitz are aware that one is taking, or has taken, place. On the other hand, two blitzes in 2011 and 2012 received extensive media coverage, so that every shop seller or business owner in the city of the blitz probably ended up being aware that a blitz was taking or had taken place. The first one took place in Cortina d'Ampezzo in December 2011, and the other one in Milan on January 28 and 29, 2012. The public nature of these blitzes was due to a choice of the Revenue Agency, presumably based on directions from the Italian Government. During winter 2011-2012, the Monti government was facing a major public finance crisis, and it had just passed a Budget Law including a number of tax increases and expenditure cuts. These policies were not welcomed by a vast part of the population, and several voices raised concerns that lower tax rates could be afforded, had Italy succeeded in reducing tax evasion. Thus, the decision to "go public" was probably motivated by the need to show that the fight to tax evasion was an organic component of consolidation efforts.[8]

Cortina d'Ampezzo is a very famous winter holiday resort typically visited by celebrities and high income people. There, the blitz targeted not

[8]It should be noted that the two experiments of "public blitzes" were unprecedented and unreplicated: nowadays, blitzes are rarely, if ever, discussed on the media. This in turn may be a consequence of the hot public debate which revolved around the alleged spectacularization of the Cortina and Milan blitzes.

only businesses but also directly individuals. The Milan blitz instead, which started on a Saturday night, around 8:30 p.m., focused on restaurants, night clubs and discotheques, and continued the following morning, when more restaurants, cafés and shops in the city center were subject to audits. Overall, the Milan blitz covered approximately 350 economic activities: agents mainly verified compliance of sales reports (including the regular release of receipts), and national and local TVs were allowed to broadcast these activities live.

Analyzing the effect of the blitz in Cortina would be difficult for at least three reasons. First, Cortina is a unique economic context, in which luxury goods and services represent an exceptionally large component of economic activities: comparing it to other towns, even in the same geographic area, would make little sense. Second, Cortina is a small town, and thus time series of aggregated tax payments are more irregular in time (see Section 3 for more details on the structure of our data). Third, the media coverage of the blitz in Cortina mostly focused on controls on individual possessions (which may not be relevant for the fiscal behavior of sellers), rather than on the shop audits. For these reasons, our analysis focuses on the public blitz which took place in Milan.

Our research question asks whether or not the public blitz had a different effect on compliance than a private blitz. In order to answer this question, we require data on public and private blitzes that occurred in the same geographic area, on the same sectors and at approximately the same time. As already mentioned, in 2012 a number of private blitzes were also conducted in a number of Italian cities, such as Genoa, Turin, Bari and Cagliari, as well as in smaller towns. However many of these private blitzes took place in cities or towns not comparable to Milan in several respects including size, wealth, geographic location, pre-existing propensity to evade. We hence focus on a blitz that took place in Genoa and in the surrounding area in the same month and on the same sectors as the Milan blitz. As already mentioned, Genua, which is 120 km South of Milan, is estimated to have a similar propensity to evade. Also, these two cities are similar in terms of socio-economic characteristics: together with Turin, they represent the three largest cities in the North West of Italy, and are often referred to as the "industrial triangle", due to the important role they share in the history of Italian manufacturing. Today, they are among the richest cities in Italy.

The Genoa blitz was held on January 6, 2012, covered approximately 150 businesses (including ice cream parlors, bars, discotheques, restaurants and

Table 1: Media coverage of different blitzes

Blitz	Google News*	Newspapers I**	Newspapers II***
Milan	326	35	19
Genoa	40	9	6

Reported is the number of articles on the blitz conducted in each city by data source. The Google News results are as of 26th of November 2015, selecting the category News, the date of the blitz and using "blitz + evasione+city" as search criterion. Newspapers I refers to "*Eco della Stampa*", Newspapers II to the web archives of *La Repubblica* and *Il Corriere della Sera*.

clothing shops) and lasted until late in the night. The difference in the number of activities involved in Milan and Genoa blitzes is roughly proportional to the difference in size of the two cities, and in both cases the audits were concentrated in less than 24 hours.

Evidence of Publicity We performed several checks in order to substantiate the difference in publicity between the blitz in Genoa and the one in Milan. These include archival searches on the following sources:

- the Google News search engine;
- the news database "*Eco della Stampa*", for a time window of 30 days before and after each blitz;
- the web archives of the two most important Italian newspapers (*La Repubblica* and *Il Corriere della Sera*), for a time window of 30 days before and after each blitz.

The results presented in Table 1 confirm that the Milan blitz had a much wider media coverage than the Genoa blitz. A *Google News* search for "*blitz*", "*evasione*" (evasion), "*Milano*", selecting the dates 28th-29nd of January 2012, yields 203 entries reporting 326 articles on the topic. On the other hand, the search of "*blitz*", "*evasione*", "*Genoa*", selecting the dates 6th-7th of January 2012, yields 5 entries and 40 articles. This disparity is confirmed by a search in the newspaper archives (see Table 1). While the Milan blitz received between 19 and 35 mentions, Genoa is only mentioned 6 to 9 times.

We also searched for mentions of the blitzes on the Youtube website. A video which can be found on Youtube by searching "*blitz*", "*evasione*",

"*Milano*"[9] scored 7,240 views as of 26th of November 2015, while no video referring to Genoa blitz can be found on Youtube. A search on Google Trends for the word "evasione", limited to January 2012 and to Italy, features a clear peak (the maximum for all the month) on Sunday January 29 (the day of the blitz in Milan), while nothing similar is present for the day of the Genoa blitz. Together, these findings provide evidence that the two blitzes differed significantly in their media coverage.

3 Data

Our empirical analysis is based on a confidential database provided to us by the *Agenzia delle Entrate*. The database includes a panel of monthly IVA (the Italian VAT) payments for 18 B2C sectors over the period January 2009 to November 2013, for Milan and Genoa.[10] As a general rule, Italian firms are required to pay IVA to the Italian Revenue Agency monthly.[11] Each IVA payment consists of the (declared) difference between IVA collected on sales and IVA paid on purchases.[12] Such payments are reported on Form "F24", which is submitted electronically and thus not affected by transcription mistakes.[13]

Because these sectors are mainly involved in B2C transactions, they have similar evasion opportunities; namely, they can omit or falsify receipts. Four of these sectors, "restaurants", "discotheques", "coffehouses/bars" and "clothing shops", were involved in the blitz in both cities, the others where not involved in neither of them. For privacy reasons, the data provided to us are aggregated at the sector level for each city-month, so they do not

[9] `http://www.youtube.com/watch?v=Dryed9MvGJU`

[10] A complete list of the 18 sectors is provided in Table 2 of Appendix A.

[11] Firms whose yearly turnover is below some specified thresholds are allowed to pay VAT quarterly.

[12] For any sale, the amount of IVA is calculated as the product of the price of the good being sold times the applicable VAT rates. In Italy, there are three VAT rates: standard (22%), reduced (10%) and super-reduced (4%). The standard rate has been changed at two occasions in recent years. It was 20% until the 16th of September 2011, then it was raised to 21% until the 1st of October 2013 and it has finally been set equal to 22% since then. The super-reduced rate applies, among others, to basic food items, while the reduced rate applies to restaurants and to hotel accommodation. Notice that such rates are applied uniformly in the whole Italian territory.

[13] When presenting monthly IVA payments, the deadline is the 15th of the following month.

allow us to conduct an analysis at the firm level. For each city-sector-month, the variables included in the dataset are aggregated IVA payments and the number of taxpayers. In total, the size of our panel is 2124 observations (59 months, 2 cities, 18 sectors).

Firms must remit the amounts declared on the F24 form to the Italian Revenue Agency - and the correspondence is trivial to verify. Therefore, non-compliance in the act of filling the F24 form corresponds to tax evasion. Notice that in the eventuality of tax audit controls, the tax police can verify that the payments correspond to the difference between IVA on sales and IVA on costs: each must correspond to the sum of amounts reported on receipts issued and received, respectively. Typically, evading IVA involves selling a good or service without issuing a receipt[14] (Fabbri and Hemels, 2013, Battiston and Gamba, 2016) and hence underreporting sales: indeed, for B2C activities this is much easier than to fabricate evidence of non-existent input purchases in order to overreport costs.[15] Tax audits in shops typically focus on non-compliance on the sales side, i.e. by checking or enforcing that receipts are issued regularly. The finance police has limited ability, during the blitz, to ensure that costs are recorded accurately (e.g. because in B2B transactions, invoices do not typically travel with the goods).

Aggregates of IVA payments at the sector level are deflated using city-specific monthly price indexes released by the Italian National Institute of Statistics (ISTAT), and then normalized by the number of IVA-taxpayers in each city-sector-month: in other words, we look at *average* payments,[16] rather than at the total amount for the sector. This normalization allows us to take into account the difference in size between the two cities, and to control for changes in the population of IVA-taxpayers within a city over time.

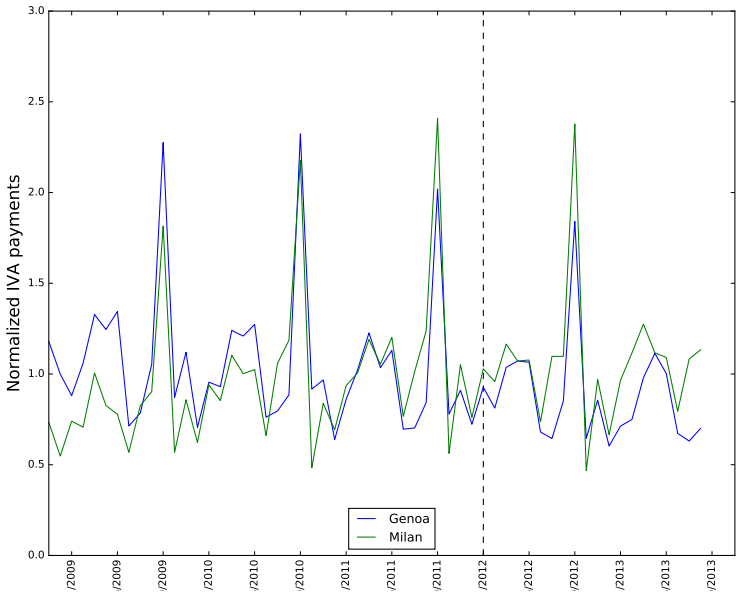
Figure 1 shows total IVA payments for Genoa and Milan during the period covered by the sample: payments are clearly highly correlated between the two cities, and strongly shaped by seasonal fiscal deadlines. This brings additional support to the soundness of the counterfactual, and, on the other hand, highlights the importance of taking into account seasonal effects.

[14]Of each receipt released to a client, a copy is kept in the cash register.

[15]This is the essence of the already mentioned "paper trail".

[16]The 2124 observations become 2056 once taking into account observations with no payments (see Table 3, Appendix A.

Figure 1: History of total reported VAT payments



Note: for ease of comparison (Milan is larger than Genoa), both series are normalized so that they average to 1.

4 Identification Strategy

We adopt a Difference-in-Differences identification strategy comparing the effect of the public blitz in Milan to the private blitz in Genoa. Let $M_{m,y,s}$ denote per capita VAT payments in Milan in month m of year y, for sector s, and $G_{m,y,s}$ the same for Genoa. We are interested in testing the hypothesis that, in the month following the blitz, the difference between $log(M_{m,y,s})$ and $log(G_{m,y,s})$ is larger than in other months.[17]

A crucial feature of our data is the presence of multiple time series, one for each of the 18 sectors. Although the sectors are unified by a theoretical opportunity to evade, they differ in many other respects. Some sell services or goods which are used routinely (e.g. bakeries), others provide goods for which consumption may be more volatile (e.g. clothings shops); they also differ in the typical size of the firm and in the average value of goods or services they offer. In addition, there may be a different response to information about blitzes between sectors in which the customer goes to the service provider (e.g., restaurants) and sectors in which the service provider goes to the customer (e.g., plumbers). More generally, there is very limited support for the assumption that VAT payments are similarly distributed across sectors. Even within sectors, there are no hints that the distribution of VAT payments over time should be normal; moreover the number of available observations *at the sector level* is relatively small (59 per city), making asymptotic assumptions inappropriate.[18] The aforementioned issues are only partly solved by including sector- or time-fixed effects in the specification, since the assumption of similarity of distributions across sectors is still required for coefficients to be valid. This leads us to adopt a non-parametric approach, which does not rely neither on the normality assumption, nor on the assumption of equal distributions across sectors. Indeed, in consideration of the intrinsic differences across sectors, together with the fact that seasonal fiscal deadlines affect different sectors in different ways, we also abstain from *directly* testing our main hypothesis via a pooled test on all sectors.

Instead, we define $\delta_{m,y,s} = log(M_{m,y,s}) - log(G_{m,y,s})$ (the difference be-

[17] We work on the logarithm of VAT payments because we expect the effect, if any, to be proportional to the pre-blitz level. Notice that we focus on the *immediate* impact of the blitz, i.e. on the payments of the month following the blitz. See Section 5.2 for a check of (absence of) long term effect.

[18] We checked whether residuals of Equation 1 are normally distributed. Normality is rejected for most sectors - see Appendix A, Table 3.

tween log of deinflated VAT payments per taxpayer in Milan and in Genoa) and we regress it, for each sector s separately, on year (ξ_y) and month (γ_m) specific dummies. More precisely, we estimate the following model using an OLS:

$$\delta_{y,m,s} = \sum_{y=2010}^{2014} \upsilon_y \xi_y + \sum_{m=1}^{12} \mu_m \gamma_m + \epsilon_{m,y,s}, \tag{1}$$

where $\epsilon_{m,y,s}$ is hence the component of the difference in payments between Milan and Genoa that cannot be explained by month and year fixed effects. Notice that such fixed effects are necessary to control for fiscal deadlines (the effect of which is evident in Figure 1) and macroeconomic trends, respectively.

The residual component $\epsilon_{m,y,s}$ may be shaped by a multitude of unobservable factors which affect the two cities in different ways. This makes it challenging to distinguish any medium or long term effect of the blitz from confounding factors (e.g. difference of the business cycle between the two cities). We hence exploit the discontinuity represented by the blitz by focusing our attention on its *immediate* effect; i.e., the effect in the month immediately following the blitz. Our identification strategy thus relies on the assumption that, with the exception of the blitz, the probability of such factors being exceptionally strong precisely in the month after the blitz is very low.

We pool all the residuals $\epsilon_{y,m,s}$ from each of the 18 sectors and run a non-parametric Mann-Whitney (MW) test (Mann and Whitney, 1947) on the null hypothesis that values in the set

$$\mathcal{B} = \{\epsilon_{y,m,s} | (y,m) = (2012,2)\}$$

are distributed like the values in the set

$$\mathcal{C} = \{\epsilon_{y,m,s} | (y,m) \neq (2012,2)\}$$

against the alternative hypothesis that the values in $\mathcal{B}$ are larger. This test is ran both for the period from January 2009 to February 2012 (specification "$\mathbb{PRE}$"), and for the entire sample period (January 2009 - November 2013: specification "$\mathbb{ALL}$").[19] The $\mathbb{PRE}$ specification is more reliable in the presence of any medium- or long-term trend, while the $\mathbb{ALL}$ specification allows us to exploit all of the available data. In what follows, results refer

[19]Notice that months after February 2012 are part of the control.

to the $\mathbb{ALL}$ specification unless stated otherwise (see Section 5.2 for more evidence supporting this choice); results for the $\mathbb{PRE}$ specification do not differ significantly.

Our empirical approach is parsimonious in terms of data for two reasons. The first is that introducing other explanatory variables which are roughly constant either across time (e.g. average income, number of shops in the city) or across cities (e.g. fiscal deadlines) would bring no benefit to the analysis, as in any Difference-in-Differences setup. The second is that, among variables that are city- and month-specific, those that would in principle be interesting in explaining evasion (e.g. total reported sales) are obviously distorted by evasion itself. Residuals from different sectors are implicitly treated as independent information: this is baked by evidence provided in Section 5.2.

5 Results

5.1 Main Findings

The MW test on the hypothesis that values in $\mathcal{B}$ are equal to the values in $\mathcal{C}$ rejects the null hypothesis of equality with a p-value of $p = 0.015$ ($p = 0.019$ in the $\mathbb{PRE}$ specification). This means that the increase in tax payments in Milan in the month following the blitz is statistically different from that in Genoa.

In order to estimate the magnitude of the effect, we then look at the values of the unexplained component $\epsilon_{y,m,s}$, across sectors: [20] we find that such residual is on average 0.271 (the average across all months being zero by definition) for February 2012. An effect of 0.271 in logarithmic terms (on $\delta_{m,y,s}$) corresponds to an increase by $e^{0.271} - 1 = 31.1\%$ of the ratio $e^{\delta_{m,y,s}} = \frac{M_{m,y,s}}{G_{m,y,s}}$. This in turn translates in $4,060$ € of extra VAT payment per taxpayer,[21] that is $7,690,899$ € in Milan, for the month and sectors considered.

[20] The values of ϵ for each sector are depicted in Figure 3, Appendix A.

[21] The counterfactual (in absence of the blitz) ratio between payments in Milan and Genoa is obtained by dividing the *observed* ratio (averaged over sectors) by $1 + 31.1\%$. From such counterfactual ratio and the observed payments in Genoa, the counterfactual value of payments in Milan is calculated. This is then subtracted from the observed value of payments in Milan, obtaining this number.

This is the most appropriate estimate if we expect that differences in the effect across sectors are random, and we aim at extrapolating an average effect of a generic publicized blitz on the whole population of shops in Milan. If we expect instead that different sectors may have intrinsically different propensities to react to news of the blitz, it is more appropriate, in order to estimate the total effect of publicity, to calculate the absolute effect in each sector, and then sum up the results: this yields a total of $6,222,934$ € extra VAT payments. The fact that this estimate is smaller than the previous one means that sectors with higher declared revenues per taxpayer tended to react less. Although the difference is small, it matches the intuition that smaller shops (e.g. shops in which the owner is typically also a seller) have a relatively larger tendency to evade. Notice however that this measure, differently from the previous one, is heavily dominated by the results for bigger sectors in terms of VAT payments.

Estimates of prevented VAT evasion presented so far obviously focus only on the 18 sectors under analysis: the total effect could in principle be much larger. Also excluded from the analysis are economic activities which, due to their small size, are allowed to file VAT payments once every three or twelve months, rather than every month: their payments are excluded from the data we used, because of their coarser temporal accuracy.

In general, our approach is based on the assumption that the publicity of the blitz in Milan had no effect on compliance decisions in Genoa in the same month. However, this does not hinder the significance of our result: if some news of the blitz in Milan had reached Genoan taxpayers, increasing compliance levels, then our estimates would be *downward* biased.[22] Moreover, as we mentioned before, the North of Italy is estimated to have lower levels of evasion compared to the Center and to the South: if the repressive effect goes hand in hand with the level of evasion, our estimates will be conservative for what concerns the impact of public blitzes in other Italian cities.

Finally, it should be noticed that results presented so far concern the estimated increase of IVA payments only. If a shop evades IVA by reporting uncorrect revenues, evasion of income taxes (due by the owner) is also necessarily taking place. The total effect of the blitz in terms of prevented tax evasion could hence be much larger.[23]

[22] An exception to this line of reasoning is the possibility of a "bomb crater effect" (Mittone, 2006). However, we do not see such an effect in sectors of Milan not concerned by the blitz.

[23] If we consider the highest income tax rate (43%, which in Italy is applied on yearly

Because we focus on VAT payments, which is the difference between IVA debts and IVA credits, it is possible that our estimated effect is driven by changes in IVA debts and/or IVA credits. For example, sellers might respond to the blitz by stopping hiding part of the revenues and thus reporting more IVA debts. It is also possible that some firms reduce their use of inflated costs, which would lower IVA credits. Distinguishing between these channels is out of the scope of the present study.

5.2 Robustness

Our approach treats residuals from the different sectors as independent information. We hence check that they are not correlated.[24] We find that pairwise Pearson correlations between different sectors are concentrated around 0, and the average correlation between any two series is very small (equal to 0.001), and not significantly different from 0.

In order to check the robustness of our results to the selection of sectors, we first rerun the MW test by removing one sector at a time, and looking only at the other 17. The effect always remains significant at the 5% level, as shown in Figure 2. It also remains significant ($p = 0.030$) if we remove sectors known to be involved in blitzes (restaurants, coffehouses/bars, discoteques, clothing shops), confirming that publicity about the blitz reached all sectors, regardless of their direct involvement. The effect is even more significant ($p = 0.008$) if we drop from the analysis sectors in which the service provider goes to the customer, rather than the opposite. It is worth observing that such sectors (i.e. plumbers and painters, in our sample) pose specific challenges to tax verification authorities, and might be less concerned by blitzes.

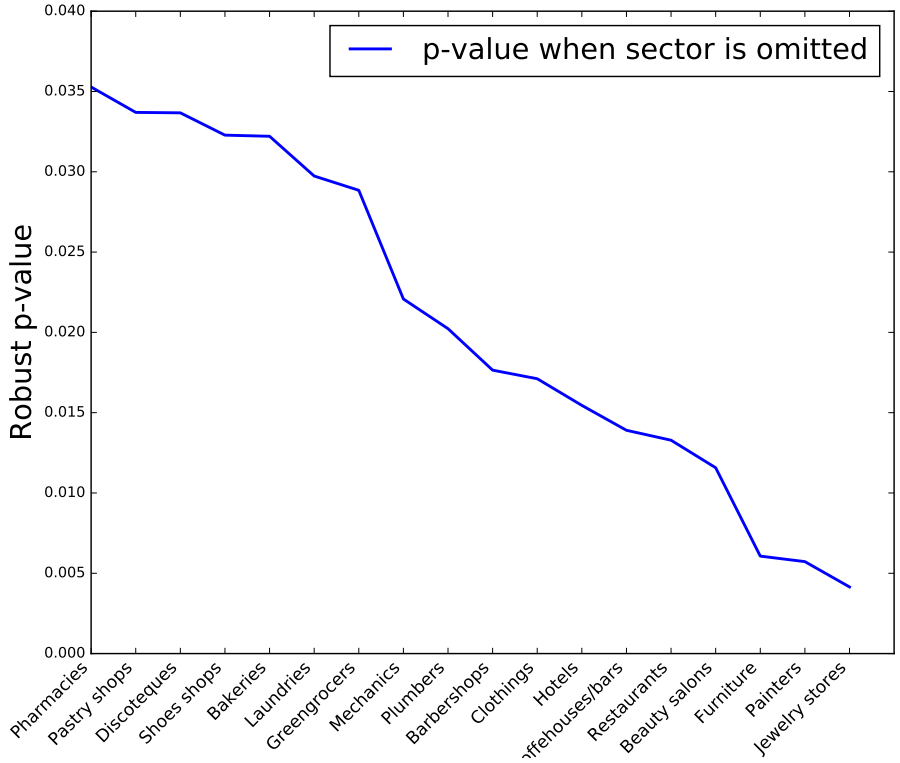
Secondly, we replace the pooled MW test with 18 independent MW tests ran on the residuals obtained from each sector separately. Notice that each of such tests will be ran on few observations only,[25] and hence will have low

incomes over 75 000 €), the amount of prevented income tax evasion can be roughly estimated to lie between 50% and 200% of the prevented VAT evasion, depending on factors such as the markup level and whether input costs are also being incorrectly reported. For instance, if the incorrect reporting concerned exclusively sales, an IVA rate of 21% would correspond to a multiplicative coefficient of $100/21 \times 0.43 \approx 200\%$.

[24] Our p-values would be underestimated if we considered correlated information as independent: with this test, we ensure instead that the original correlation between the different sectors is explained by seasonal and yearly fixed effects.

[25] More precisely, in each of those MW tests, the "treated" set contains exactly one element (the residual for the month of the blitz), and the resulting p-value is then simply

Figure 2: P-value obtained by runnning the MW test omitting one sector at a time.



power; however, the p-values obtained can be aggregated for instance with the use of the Fisher method (Fisher, 1925) for meta-analysis. This method guarantees that each sector is attributed the same importance, independently from the sector-specific variance: it results in a p-value of $p = 0.027$. Notice that this test, compared to the pooled MW test, should have higher power the larger the difference between sectors, and would have significantly lower power in the case in which the sectors were identically distributed: the MW test is a combinatorial test, and performs worse if the sample is split in subsamples for the analysis. The fact that the p-value is only slightly higher hence suggests that heterogeneity across sectors is present, although not so large as to make the pooled MW test disadvantageous. The fact that the p-values is higher than the one found with the pooled MW test suggests that the heterogeneity (of the distribution of VAT payments in time) across the different sectors is not large enough as to overcome the inefficiency of the aggregation procedure.

Finally, the "$\mathbb{ALL}$" specification, in which the control sample also includes months *after* February 2012, is coherent with the hypothesis that the blitz only had a temporary effect (or that, if a medium or long term effect is present, it cannot be observed due to confounding factors, i.e. to medium- or long-term business cycle differences between Milan and Genoa). Although we did verify that results are analogous in the "$\mathbb{PRE}$" specification, it is still worth checking if the data in our observation window exhibit some long term variation. For instance, if a permanent change between the business cycles in Milan and Genoa *had* taken place at some time in our observation window, the results of the "$\mathbb{ALL}$" specification could be biased by such a change. In particular, a permanent negative (positive) change just before (after) the blitz would bias our results downwards, making the estimates conservative. But, a permanent positive (negative) change just after (before) the blitz would have the opposite effect, possibly limiting the validity of the specification. This risk is taken into account to a large extent by the presence of year fixed effects in Equation (1). Still, in order to completely exclude this eventuality, we first run MW tests on the hypothesis that after the blitz there was a permanent increase/decrease in the unexplained terms: in both cases, we are unable to reject the null of no difference (notice that, had we found a significant effect, its attribution to the public blitz would have been implausible anyway, for

r/T, where r is the rank of such element, and T is the number of observations available for that sector.

the aforementioned business cycle concerns).

We also run two placebo tests by replacing the months of the blitz (January 2012) with January 2011 and January 2013, respectively: in both cases, we cannot reject the null hypothesis of equality between values in $\mathcal{B}$ and $\mathcal{C}$ ($p > 0.7$):[26] these findings confirm the causal interpretation of our results.

6 Conclusions

Fiscal blitzes, which cluster together a large number of unexpected tax audits, do not just represent a possibility for revenue agencies to achieve organizational economies of scale in running audits. In principle, they can also be important instruments for sending a signal to taxpayers about the willingness to fight tax evasion. However, the extent to which this signal can be effective entirely depends on the level of attention that a blitz receives in the public opinion. In Italy, a fiscal blitz, ran in Milan in January 2012, achieved an exceptionally broad resonance, as a consequence of an explicit choice of the Italian Revenue Agency to release information about it to the media. The tax authorities also conducted a private blitz in Genoa around the same time as in Milan.

We analyze the compliance effect of the variation in blitz publicity by comparing VAT payments in Milan with VAT payments in Genoa in the month after the blitz, in order to determine the extent to which media coverage affects compliance. Our Difference-in-Differences analysis shows evidence of a strong and robust positive publicity effect. The estimates suggest that the public blitz in Milan increased IVA compliance by approximately 7.7 million euros just in the 18 B2C sectors which our data cover.

Our data consists of multiple time series which are in principle differently distributed among them: the non-parametric approach we adopt allows us to exploit the independent information they convey.

The policy implications of this study point towards the role that information provided by the media can have in shaping tax compliance decisions, and hence towards the importance for revenue agencies to wisely use information about blitzes, and, more generally, available communication channels.

[26] Results for January 2013 do not involve discotheques, for which declarations of February are missing.

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A Additional material

Table 2: List of sectors included in the database

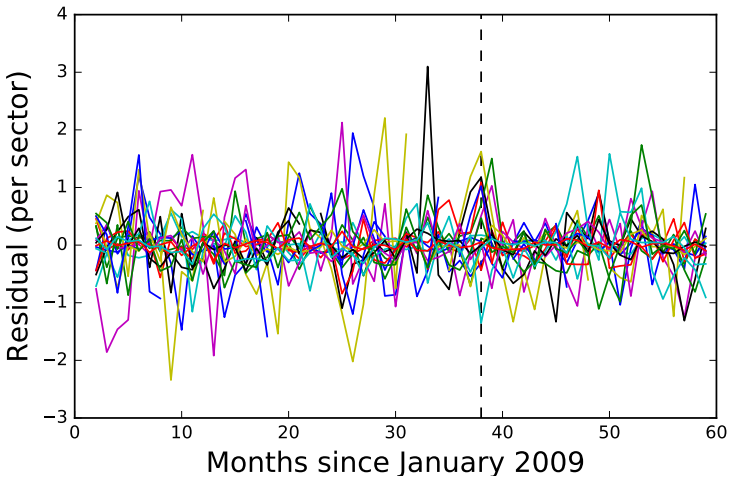
ISTAT code	Original name	Category
31	Fabbricazione di mobili	Furniture
43.22.01	Installazione di impianti idraulici, di riscaldamento e di condizionamento dell'aria (inclusa manutenzione e riparazione) in edifici o in altre opere di costruzione (idraulici)	Plumbers
43.3	Completamento e finitura di edifici (imbianchini)	Painters
45.2	Manutenzione e riparazione di autoveicoli (meccanici)	Mechanics
47.21	Commercio al dettaglio di frutta e verdura in esercizi specializzati (fruttivendoli)	Greengrocers
47.24.1	Commercio al dettaglio di pane	Bakeries
47.24.2	Commercio al dettaglio di torte, dolciumi, confetteria	Pastry shops
47.71	Commercio al dettaglio di articoli di abbigliamento in esercizi specializzati	Clothings
47.72	Commercio al dettaglio di calzature e articoli in pelle in esercizi specializzati	Shoes shops
47.73	Commercio al dettaglio di medicinali in esercizi specializzati	Pharmacies
47.77	Commercio al dettaglio di orologi e articoli di gioielleria in esercizi specializzati	Jewelry stores
55.1	Alberghi e strutture simili	Hotels
56.1	Ristoranti e attività di ristorazione mobile	Restaurants
56.3	Bar e altri esercizi simili senza cucina	Coffehouses/bars
93.29.1	Discoteche, sale da ballo night-club e simili	Discoteques
96.01.2	Altre lavanderie, tintorie	Laundries
96.02.01	Servizi dei saloni di barbiere e parrucchiere	Barbershops
96.02.02	Servizi degli istituti di bellezza	Beauty salons

Table 3: Results of normality tests on residuals

Sectors	Obs	Shapiro-Francia	Shapiro-Wilk
Furniture	58	0.127	0.251
Plumber	58	0.069	0.166
Painters	58	0.201	0.352
Mechanics	58	0.001	0.002
Greengrocers	58	0.805	0.934
Bakeries	51	0.977	0.978
Pastry shops	57	0.000	0.000
Clothings	58	0.007	0.018
Shoes shops	58	0.002	0.002
Pharmacies	58	0.012	0.019
Jewelry stores	58	0.113	0.201
Hotels	58	0.024	0.053
Restaurants	58	0.000	0.000
Coffehouses/bars	58	0.010	0.018
Discoteques	34	0.958	0.992
Laundries	58	0.713	0.946
Barbershops	58	0.049	0.176
Beauty salons	58	0.017	0.021

Note: p-values from normality tests on residuals from Equation 1. Both Shapiro-Francia and the similar Shapiro-Wilk test are performed, rejecting the null hypothesis of normality ($\alpha = 10\%$) respectively in 11 and 9 out of 18 sectors.

Figure 3: Sector-specific residuals



Note: Residuals from equation 1: each line corresponds to one of the 18 sectors. Although one might notice a prevalence of peaks for month 38, the effect of publicity can not be singled out by looking at a single time series (sector).