INSTITUTE
OF ECONOMICS

Scuola Superiore
Sant'Anna

LEM | Laboratory of Economics and Management

Institute of Economics
Scuola Superiore Sant'Anna

Piazza Martiri della Libertà, 33 - 56127 Pisa, Italy
ph. +39 050 88.33.43
institute.economics@sssup.it

LEM

WORKING PAPER SERIES

Audit publicity and tax compliance: a quasi-natural experiment

Pietro Battiston [a]
Denvil Duncan [b]
Simona Gamba [c]
Alessandro Santoro [d]

[a] Istituto di Economia, Scuola Superiore Sant'Anna, Pisa, Italy
[b] School of Public and Environmental Affairs, Indiana University, USA
[c] FBK-IRVAPP, Trento, Italy
[d] Dipartimento di Economia, Metodi Quantitativi e Strategie di Impresa, University of Milano-Bicocca, Milan, Italy

2016/40 December 2016
ISSN(ONLINE) 2284-0400

Audit publicity and tax compliance: a quasi-natural experiment

Pietro Battiston[*], Denvil Duncan[†], Simona Gamba[‡], Alessandro Santoro[§]

November 2016

Abstract

We use confidential data on Value Added Tax payments at the sector level, in two large Italian cities, to estimate the effect of audits publicity on tax compliance of local sellers. By employing a Difference-in-Differences identification strategy, we find that such publicity has a positive effect on fiscal declarations made shortly after. The results suggest that increasing awareness on future audits via the media can be an important instrument in the hands of tax authorities.

Keywords: Tax evasion, Quasi-natural experiment, Audit publicity.
JEL classification: H26 K34 K40

[*]Istituto di Economia, Scuola di Studi Superiori Sant'Anna, Piazza Martiri della Libertà, 33, 56127 Pisa, Italy. me@pietrobattiston.it

[†]School of Public and Environmental Affairs, Indiana University - Bloomington, duncande@indiana.edu

[‡]FBK-IRVAPP, via Santa Croce, 77, 38122 Trento, Italy. gamba.simona@gmail.com

[§]Dipartimento di Economia, Metodi Quantitativi e Strategie di Impresa, Università degli Studi di Milano Bicocca, Piazza Ateneo Nuovo, 1, 20126 Milano, Italy. alessandro.santoro@unimib.it

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 tax rates, audit probability and fines (Allingham and Sandmo, 1972; Yitzhaki, 1974; Rincke and Traxler, 2011) in influencing the decision to evade, it is generally understood that tax evasion 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. These communication channels 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 evaders in the population, which in turn can 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 (Slemrod et al., 2001;

[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: the tax gap figure is taken from Table 1.1 in the HMRC's document *Measuring tax gaps tables 2015* while the deficit figure is from Table T4.35 in the Office for Budget Responsibility's *November 2015 Economic and fiscal outlook: Charts & Tables*. For US: the tax gap figure is from the Internal Revenue Service while the deficit figure is from Table 1.1 of the *Historical Tables* produced by the Office of Management and Budget. We refer to the 2006 budget deficit because the most recent tax gap estimates are for that year. For Italy: the tax gap figure is taken from *Scenari economici n. 25, Dicembre 2015* of the Centro Studi Confindustria, while the deficit figure is from Table 4 of the December 2015 "Bollettino Statistico" ("*Statistical Bulletin*") published by Bank of Italy.

Alm et al., 2009; Kleven et al., 2011), there is very little information on the extent to which media coverage influences the decision to evade. However, this can happen in several ways: media coverage can provide information about the magnitude of tax evasion, affect tax morale, and alter the perceived probability of audits or even just their salience. The impact of public disclosure of information on tax compliance has gained the attention of several governments around the world: for example, in Ireland, a list of tax defaulters is reported in national and local newspapers. According to the tax agency, this measure "aims to raise the profile of compliance and provide a continuous deterrent to other potential tax evaders" (Bø et al., 2015). However, so far, the attention of the literature has focused on the disclosure of information about the outcome of individual tax audits, rather than about audit campaigns themselves.

The present paper contributes to this stream of literature by identifying whether the publicity of an audit affects the propensity 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"). In this paper, we exploit this difference in order to identify the effect that publicity has on compliance, as detailed below.

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 had similar characteristics, including being unannounced and focusing on a similar set of industries. Third, they differed greatly in their media coverage, with the Milan blitz being extensively covered by news outlets due to an explicit decision of the Italian Revenue Agency. Finally, Genoa and Milan are part of the same Italian macro region (North-West), and are comparable in socio-economic terms: hence, even though the treatment assignment was not random, we compare two groups of taxpayers who are very similar at the baseline.

We identify the effect of media coverage through a Difference-in-Differences strategy which compares the behavior of taxpayers in the two cities using a non-parametric approach.[2] The treatment group is composed of sectors in Milan, while the control group is composed of the corresponding sectors in Genoa: the treatment consists in being exposed to the news of the local blitz. We find evidence of a positive and significant effect of publicity on VAT payments for the month of the blitz: those in Milan increased relative to the ones in Genoa. We estimate the aggregate effect of publicity (on declarations for the month of the blitz and in the 18 sectors considered) to be more than 7 million euros. The findings are robust to a range of alternative specifications, and suggest that the public blitz increased the perceived probability of being audited for Milan taxpayers, thus increasing their compliance in the short run. Clearly, the sectors in Genoa might have also reacted to the news of the Milan blitz in the same direction: our estimates should be viewed as lower bounds if this is true.

Our findings are consistent with other 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.[3] This has important implications for both academics and policy-makers interested in understanding the determinants of tax evasion.

Our results also contribute to the policy debate concerning public blitzes. Shortly after the Milan blitz, 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 suggest that the publicity of the blitz had a positive effect on compliance also of *unaudited* shops, 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 direct and indi-

[2] We adopt a non-parametric approach for reasons related to the structure of our dataset and the limited support for normality assumptions. See Section 5 for more details.

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

rect costs would be needed;[4] but publicity of blitzes is an instrument which policymakers should take into consideration.

The remainder of the paper proceeds as follows. Section 2 provides institutional details that support our decision to focus on Italy and, in particular, on the cities of Milan and Genoa, while 3 provides background on the more general issue of tax compliance and information. The data are described in Section 4, our identification strategy in Section 5, and results in Section 6. Finally, we conclude in Section 7.

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. 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 nation-wide 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 the North of the country, and higher in the Center and especially in the South. For example, in the period 2007 to 2010, the estimated VAT gap was 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 for this paper are the North-Western regions of Lombardia, whose main city is Milan, and Liguria, whose main city is Genoa; the VAT gap for these two regions is estimated at 21.18% and 22.82%, respectively (D'Agosto et al., 2014). That

[4] 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, and in particular of an excessive "spectacularization" of the blitz, on the regular functioning of businesses.

these two cities have similar VAT gaps is important for our identification strategy; it suggests that our results are not 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 improving tax morale and public consciousness, the use of presumptive taxes, the increase in penalties for evaders, and of the frequency of tax verification activities, including 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 time span, and targeting some specific sectors of the economy. The OECD (2014) defines "tax verification activity" as an umbrella term 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. A blitz typically involves field audits.

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 Labor as well as local policemen. In order to ensure the unpredictability of the blitz, the authorities vary the time and day of each blitz. During blitzes, the agents check for the correct issuance of receipts,[5] 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. Failure to issue a receipt results in a fine of at least 150 euro plus the temporary closure for 15 days of the business if three such infractions are caught in a five years period. Additionally, verification activities can have other long term

[5]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.

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 negligible.

Although no complete data are available, blitzes are not an uncommon instrument among Revenue Agencies: 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 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 recently installed 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.[6]

[6]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, as well as a consequence of the end of the Monti government.

Cortina d'Ampezzo is a very famous winter holiday resort typically visited by celebrities and high income people. The Cortina blitz targeted not only businesses but also individuals directly. The Milan blitz, which started at 8:30 p.m. on a Saturday night, instead 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 informed and 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 noisy (see Section 4 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 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 a comparable geographic area, on the same sectors and at approximately the same time. As already mentioned, in 2012 a number of private blitzes were conducted in a number of Italian cities, such as Genoa, Turin, Bari and Cagliari, as well as in smaller towns. However, the blitz in Genoa took place in the same month as the public blitz in Milan, while this is not true for the other cities just mentioned. Moreover, most of these cities or towns are not comparable to Milan in several respects including size, wealth, geographic location, and pre-existing propensity to evade; the link between these observable variables and the reaction of taxpayers to a blitz being far from obvious, it is difficult to define an objective measure of similarity under this respect.[7] Therefore, we focus on the Genoa blitz, which took place in the same month and in-

[7] These are the two main motivations not to adopt a "synthetic counterfactual" approach (Abadie and Gardeazabal, 2003).

volved the same sectors as the Milan blitz. As already mentioned, Genoa, which is 120 km South-West 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.[8] 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 clothes 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 media coverage 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. While the Milan blitz received between 19 and 35 mentions, Genoa is only mentioned 6 to 9 times.

[8] Turin was not included in the analysis because no blitz happened there that could help answering our research question.

Table 1: Media coverage of different blitzes

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

Note: 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*.

We also searched for mentions of the blitzes on Youtube. A video that 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 Theoretical considerations

As already mentioned, there are many channels through which media coverage can affect taxpayers compliance decision. After a blitz, the perceived probability of an audit may change. On the one hand, it can increase either because the blitz is interpreted as a greater investment of the Revenue Agency in the fight against tax evasion, or due to an "availability-heuristic" effect (Tversky and Kahneman, 1973), with the increase in compliance being due to the subjective salience of audits and punishments. In this sense, media coverage can extend the effect to taxpayers sharing some characteristics with those involved in the blitz, the most obvious being the city of residence and the operating sector. On the other hand, it has been suggested that a rational taxpayer, aware of strict budget constraints affecting tax verification activities, would expect a lower audit intensity after the blitz and therefore evade more (Mittone, 2006).

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

The change in the perceived probability of audits is not the only channel through which media coverage can influence tax compliance. An alternative one is represented by the dissemination of information about the share of non-compliant taxpayers and the amount of evasion. After the Milan blitz, some media reported estimates of the percentage of non-compliant taxpayers and the amount of detected evasion, increasing the awareness of Milan-based taxpayers on the level of non-compliance, and possibly raising their propensity to evade.

Following the model by (Myles and Naylor, 1996), it is possible to show that even small variations in the share of non-compliance or in the perceived probability to be audited can generate large changes in the level of compliance. Thus, at least in the short run, the impact of the media coverage of the Milan blitz can be positive if the increase in the perceived probability outweighs the increase in the perceived share of non-compliant taxpayers, and vice-versa.

Finally, media reports can play an important role in shaping tax morale by stressing the negative consequences of non-compliance on the public budget or by highlighting the burden that non-compliance places on the shoulders of compliant taxpayers (particularly so in a country in which a former Prime Minister claimed that evading taxes was "morally justified"). Both these arguments are rooted in the public discussion on tax evasion in Italy, and both were mentioned in articles and declarations concerning the Milan blitz. Hence, in principle, they might have had opposite impacts on tax compliance in the days and months following the blitz.

It is worth mentioning that, in the case under analysis, some of the arguments exposed might have been relevant even for taxpayers living in Genoa. Interestingly, since these two cities belong to different regional directorates, and since budget and audit targets are allocated ex ante among these directorates, there is no reason why a rational taxpayer in Genoa should change her compliance after the blitz in Milan. Still, to the extent that Genoa taxpayers responded to the Milan blitz, the result of our difference-in-differences estimation would represent a lower bound of the effect on taxpayers based in Milan.

4 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. The deadline for presenting monthly IVA payments is the 15th of the following month: importantly, our database reports for each month m the amounts *due* for that month, and hence referring to earnings and costs in month $m-1$.

Because the 18 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", "coffeehouses/bars" and "clothes shops", were involved in the blitz in both cities, while the others were not involved in either city. For privacy reasons, the data provided to us are aggregated at the sector level for each city-month, so they do not allow an analysis at the firm level. For each city-sector-month, however, we know both the sum of IVA payments and the number of taxpayers. In total, our panel comprises 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 represents an act of 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

[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] 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 been 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. Importantly, such rates are applied uniformly in the whole Italian territory - i.e. changes affected at the same time Genoa and Milan.

selling a good or service without issuing a receipt[13] (Fabbri and Hemels, 2013; Battiston and Gamba, 2016) and hence under-reporting sales: indeed, for B2C activities this is much easier than fabricating evidence of non-existent purchases of inputs in order to over-report costs (this is the essence of the already mentioned "paper trail"). Tax audits in shops typically focus on non-compliance on the sales side, i.e. by checking or enforcing that receipts are regularly issued. The finance police has limited ability, during the blitz, to ensure that costs are recorded accurately (for instance because, in B2B transactions, invoices do not typically travel with the goods).

We deflate aggregated IVA payments using city-specific monthly price indexes released by the Italian National Institute of Statistics (ISTAT). We then divide them by the number of IVA-taxpayers in each city-sector-month: in other words, we look at *average* payments,[14] rather than at the total amount for the sector: this normalization allows us to take into account differences in size, and to control for changes in the population of IVA-taxpayers within a city over time.

Figure 1 shows IVA payments per taxpayer, averaged across sectors, for Genoa and Milan: they are clearly highly correlated, and strongly shaped by seasonal fiscal deadlines. This brings additional support to the soundness of using Genoa as a counterfactual for Milan, and at the same time highlights the importance of taking into account seasonal effects.

5 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,[15] the difference between $log(M_{m,y,s})$ and $log(G_{m,y,s})$ is larger than in other months.[16]

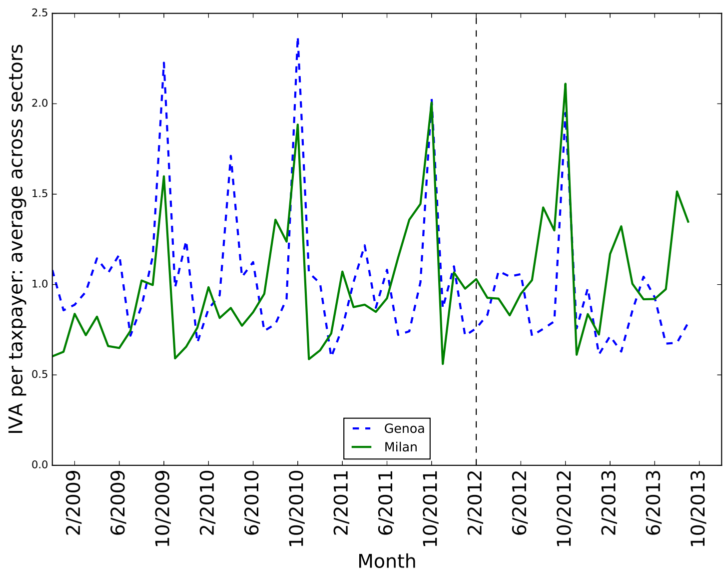
[13] Of each receipt released to a client, a copy is kept in the cash register.

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

[15] Recall from Section 4 that declarations made in month m refer to VAT due for month $m-1$.

[16] 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 regarding the month of the blitz. See Section 6.2 for a check

Figure 1: History of reported VAT payments



Note: average of VAT payments per taxpayer, across the 18 sectors analyzed. For ease of comparison, both series are normalized so that they average to 1. The effect of seasonal fiscal deadlines is clearly recognizable.

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 consumed daily (e.g. bakeries), others provide goods for which consumption may be more volatile (e.g. clothes 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.[17] 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 between 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 through OLS:

$$\delta_{y,m,s} = \sum_{y=2010}^{2013} \upsilon_y \xi_y + \sum_{m=2}^{12} \mu_m \gamma_m + \epsilon_{m,y,s}, \tag{1}$$

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.

of existence of long term effects.

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

Such fixed effects allow 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 on VAT declarations 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 of 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

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

follow the same distribution of 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.[18] 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 "$\mathbb{ALL}$"), hence including months after February 2012 in the control sample. The $\mathbb{PRE}$ specification is more reliable in the presence of any medium- or long-term trend, while the $\mathbb{ALL}$ specification can achieve higher precision by better exploiting available data. In what follows, results refer to the $\mathbb{ALL}$ specification unless stated otherwise (see Section 6.2 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,

[18] Although the MW test is often presented as a test of shift in distribution, in its simplest interpretation, related to the probability of a value in B being larger than a value in C, it does not require any distributional assumption on the two populations.

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: in Section 6.2 we present evidence that indeed, once controlling for month- and year-fixed effect, the unexplained difference between Milan and Genoa is uncorrelated across sectors.

6 Results

6.1 Main Findings

The MW test on the null hypothesis of equality between values in $\mathcal{B}$ and values in $\mathcal{C}$ rejects it 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 for the month of the blitz is statistically different from that in Genoa.

In order to estimate the magnitude of the effect, we look at the values of the unexplained component $\epsilon_{y,m,s}$ across sectors:[19] 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 (i.e. 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,[20] that is $7,690,899$ € in Milan, for the month and sectors considered (corresponding to 23.7% of the average monthly payments in Milan).

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

[19] The values of ϵ for each sector are shown in Figure 4, Appendix A.

[20] Let R be the observed ratio between payments in Milan and Genoa (averaged over sectors) in the month of the blitz: since the estimated effect is 31.1%, the counterfactual ratio (i.e. estimated in absence of the blitz) is $\hat{R} = \frac{R}{1+31.1\%}$. From such value and the observed payments in Genoa, we can easily calculate the counterfactual value of payments in Milan, and subtract it from the *observed* value, obtaining the given estimate.

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 minor, 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. This measure, however, 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: the data we used does not include their payments, because of their coarser temporal accuracy.

In general, our approach is based on the assumption of no externalities, i.e. that the publicity of the blitz in Milan had no effect on compliance decisions in Genoa. However, this does not hinder the significance of our result: if some news of the blitz in Milan reached Genoan taxpayers, increasing compliance levels, then our estimates are *downward* biased.[21] 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 the results presented so far concern the estimated increase of IVA payments only. If a shop evades IVA by reporting incorrect 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.[22]

Because we focus on VAT payments, which are the difference between IVA

[21] 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.

[22] If we consider the highest income tax rate (43%, which in Italy is applied on yearly 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 $100/21 \times 0.43 \approx 200\%$.

on sales and IVA on purchases, our estimated effect can be driven by changes in either of the two components. For example, sellers might respond to the blitz by stopping hiding part of the revenues, but it is also possible that some firms reduce their recourse to inflated costs. Distinguishing between these channels is out of the scope of the present study. Similarly, in this context we disregard the distinction between psychological motives (e.g. guilty or social stigma) and strategic decisions (higher perceived probability of auditing) on behalf of shops owners/sellers: we limit ourselves to the estimation of the effect which the media coverage of the blitz has on fiscal declarations, which *per se* has important policy implications.

6.2 Robustness

Our approach treats residuals from the different sectors as independent information. We hence check that they are not correlated.[23] We find that pairwise Pearson correlations between pairs of different sectors are concentrated around 0, and the average correlation is very small (0.001), and not significantly different from 0.

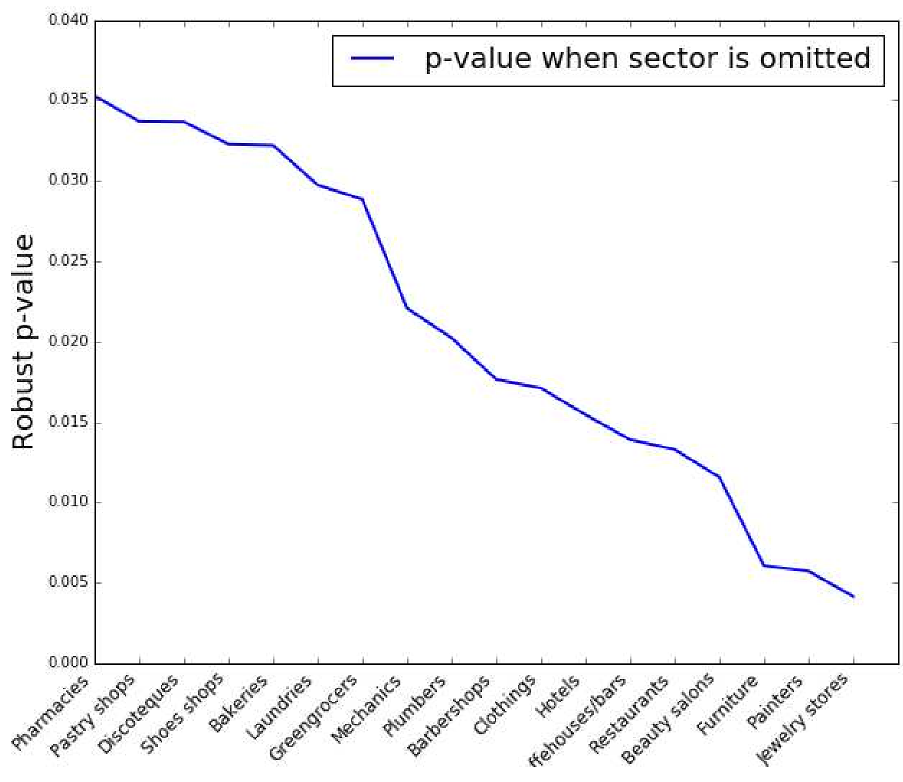
In order to check the robustness of our results to the selection of sectors, we first rerun the MW test by removing one sector at a time, and looking only at the other 17. The effect always remains significant at the 5% level, as shown in Figure 2. It also remains significant ($p = 0.030$) if we remove sectors known to be involved in blitzes (restaurants, coffeehouses/bars, discotheques, clothes 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 are indeed expected to 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 is ran on few observations only,[24] and hence will have low power;

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

[24] More precisely, in each of those MW tests, the "treated" set contains exactly one element (the residual for February 2012), and the resulting p-value is then simply r/T,

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



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 heterogeneity (in the distribution of residuals) among sectors, and would have much lower power instead in case the sectors were identically distributed: the MW test is a combinatorial test, and performs worse if the sample is split in sub-samples for the analysis. The fact that the p-value is slightly larger than the one found with the pooled MW test suggests that the heterogeneity across sectors is not large enough as to overcome the inefficiency of the aggregation procedure.

Finally, the "$\mathbb{ALL}$" specification, in which the control sample includes months *after* February 2012, is coherent with the assumption 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. 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 neutralize it, 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 the aforementioned business cycle concerns.

Both if the effect is short lasted, or if it has a medium term component which is hidden by confounding factors affecting Milan and Genoa differently, we would expect its evidence to fade out gradually. In this spirit, we look at

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

payments made *two months* after the blitz, that is, in March 2012: indeed, if we run a test on such month (excluding February 2012 from the analysis), we again find a positive effect on declarations (3,601,279 €, roughly half the estimated effect for February), significant at the 10% level ($p = 0.081$). Results become non-significant instead starting with April 2012 (3 months after the blitz: $p = 0.784$). Fiscal data with higher temporal resolution (e.g. receipts released by shops are timestamped) would be needed to estimate more detailed response curves.

Given the importance of seasonal deadlines, we run placebo tests by assuming that the blitz took place in January of other years covered by the sample (2009, 2010, 2011, 2013) rather than in 2012: in no case we can reject the null hypothesis of equality between values in $\mathcal{B}$ and $\mathcal{C}$ ($p > 0.23$). These findings confirm the causal interpretation of our results. Figure 3 presents the p-values resulting from placebo tests on *each* month in our sample: notice that the 2 months with lower p-values roughly correspond to the expected number of false positives in a sample of 58 independent tests.

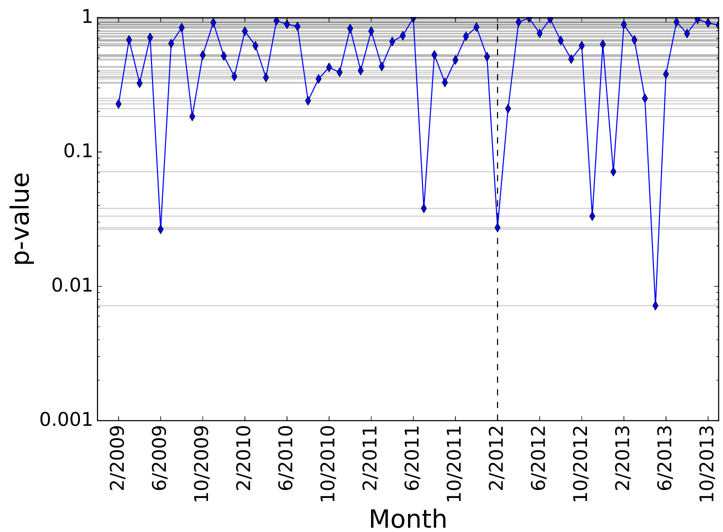
We also replace the nonparametric approach by regressing, through an OLS, the difference $\delta_{y,m,s}$ over sector, month and year fixed effects, with an additional dummy variable "blitz" (therefore, again adopting a Difference-in-Differences identification strategy). The coefficient for such variable has a positive sign, and is significant ($p = 0.035$). If rather than a single dummy variable we add a set of sector-blitz interactions and we aggregate their p-values using the Fisher method, the result is still significant ($p = 0.019$).

7 Conclusions

Recent empirical studies based on administrative data provide abundant evidence on the specific deterrence effect of audits (Slemrod, 2016). However, the channels through which information about tax enforcement and evasion opportunities spread are relatively understudied: the existing literature focuses on the role of taxpayer-to-taxpayer and administrator-to-taxpayer communication. This study focuses on the role that information provided by the media can have in shaping tax compliance decisions.

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: they can also be important instruments for sending signals to taxpayers about the willingness to

Figure 3: Results of placebo tests



Note: reported are the p-values of a series of placebo tests. Each placebo test assumes that the blitz took place in a month other than the true blitz period (January 2012, reflecting in VAT payments of February 2012). The selected month of each placebo is indicated on the x axis.

fight tax evasion. However, the extent to which these signals can be effective entirely depends on the level of attention that a blitz receives by 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.

In order to study the effect of the publicity of the blitz on local taxpayers, we exploit a confidential database of VAT declarations at the sector level provided by the *Agenzia delle Entrate* (Italian Revenues Service). Our data consists of multiple time series, possibly presenting very different characteristics: the non-parametric approach we adopt allows us to exploit the independent information they convey without resorting to distributional assumptions (neither between nor within sectors).

We analyze the compliance effect of the variation in blitz publicity by comparing VAT payments in Milan with VAT payments in Genoa for the month of the blitz. Our Difference-in-Differences analysis provides evidence of a positive, strong and robust effect of publicity. The estimates, based on 18 categories of B2C commercial activities, suggest that the public blitz in Milan increased VAT compliance by more than 7 million euros in such sectors alone. The effect is robust to the selection of the sectors analyzed: interestingly, it remains strongly significant even when discarding sectors not directly involved in the blitz. This represents additional evidence in favor of the importance of media reports about fiscal verification activities, rather than just of the activities themselves, in shaping tax compliance, hence further reinforcing our hypothesis on the importance of media coverage. A more detailed analysis of sector-specific and peer effects would require the availability of micro data on individual payments: this is a promising venue for future research.

What we measure is a short term effect on the declarations immediately following the blitz and, to a lower extent, on declarations made one month later. Still, our method allows us to identify a positive response of taxpayers to information on fiscal verification activities, and to exclude in this context the presence of the "bomb crater effect" hypothesized by Mittone (2006). It is also worth mentioning that the effect of audits publicity is a proof of the presence of tax evasion (a phenomenon widely acknowledged as difficult to measure *per se*) in a specific selection of business sectors. The fact that the blitz publicity was a sporadic initiative leaves then open the question as to what would happen if blitzes, and more in general tax verification activities, routinely benefited of such a media coverage.

This paper focuses on the positive effect of media coverage on compliance: making a comprehensive assessment and comparison of the benefits and costs related to such publicity is out of its scope. In particular, it should be taken into account that some of the costs might have a *political* nature, i.e. influencing the popularity of politicians, and hence can be difficult to quantify. For reference, we can still present a back of the envelope calculation concerning the cost of *running* a blitz (as opposed to the cost of *publicizing* it). Although no public information is available on the number of tax auditors involved in the Milan blitz, we know that approximately 1,200 tax officers were employed and that the blitz lasted approximately 20 hours. Since, according to the Revenue Agency estimates, the operational cost of each hour of audit is 55 euros per officer involved,[25] this means that, under the extreme assumption that each of the officers worked for the entire duration of the blitz, the total cost of the operation was around 1.3 million euros. That is, the upper bound to the cost of the blitz is approximately five times lower than the lower bound to the effect of publicity we provide. Although we are not able to quantify the political costs of the blitz publicity, there is some evidence that these costs are high. This is suggested by the fact that a change in strategy was publicly advocated in the direction of strengthening the "mutual trust" between the taxpayer and the Revenue Agency after the Milan blitz. In fact, the Italian Revenue Agency has not conducted public blitzes since then.

[25] These figures, which we received privately from the Revenue Agency, are consistent with those provided to the OECD Tax Administration Database: `www.oecd.org/site/ctpfta/taxadministrationdatabase.htm`.

References

Abadie, A. and J. Gardeazabal (2003). The economic costs of conflict: A case study of the basque country. *The American Economic Review 93*(1), 113–132.

Agenzia delle Entrate (2012, January 30). *Operazione "movida" a Milano. Gli incassi crescono di oltre il 44% rispetto al sabato precedente*. Press Release. `http://lombardia.agenziaentrate.it/sites/lombardia/files/public/File/comunicati/2012/cs300112_operazione_movida_milano.pdf`.

Allingham, M. and A. Sandmo (1972). Income tax evasion: A theoretical analysis. *Journal of Public Economics 1*(3-4), 323–38.

Alm, J., R. Jackson, Betty, and M. Mc Kee (2009). Getting the word out: Enforcement dissemination and compliance behavior. *Journal of Public Economics 93*(1), 392–402.

Battiston, P. and S. Gamba (2016). The impact of social pressure on tax compliance: A field experiment. *International Review of Law and Economics 46*, 78–85.

Bethmann, D. and M. Kvasnicka (2016). International Tax Evasion, State Purchases of Confidential Bank Data and Voluntary Disclosures. *Institute of Economic Research, Korea University, Working Paper Series* (1603).

Bø, E. E., J. Slemrod, and T. O. Thoresen (2015). Taxes on the internet: Deterrence effects of public disclosure. *American Economic Journal: Economic Policy 7*(1), 36–62.

CASE (2015). Study to quantify and analyse the VAT Gap in the EU Member States. Technical report, Center for Social and Economic Research, Warsaw. `http://ec.europa.eu/taxation_customs/resources/documents/common/publications/studies/vat_gap2013.pdf`.

D'Agosto, E., M. Marigliani, and S. Pisani (2014). Asymmetries in the territorial VAT gap. Discussion Topics 2, Italian Revenue Agency.

Doerrenberg, P. and D. Duncan (2014). Experimental evidence on the relationship between tax evasion opportunities and labor supply. *European Economic Review 68*, 48–70.

Fabbri, M. and S. Hemels (2013). 'Do You Want a Receipt?' Combating VAT and RST Evasion with Lottery Tickets. *Intertax: international tax review 41*(8/9), 430–443.

Fisher, R. A. (1925). *Statistical methods for research workers.* Oliver and Boyd, Edinburgh.

Hashimadze, N., G. Myles, and T. Binh (2013). Applications of Behavioural Economics to Tax Evasion. *Journal of Economic Surveys 27*(5), 941–977.

Italian Government (2013). Rapporto concernente i risultati conseguiti in materia di contrasto dell'evasione fiscale. Nota di Aggiornamento al DEF, Allegato II, Senato della Repubblica.

Kleven, H., M. Knudsen, C. Kreiner, S. Pedersen, and E. Saez (2011). Unwilling or Unable to Cheat? Evidence from a Tax Audit Experiment in Denmark. *Econometrica 79*(3), 651–692.

Kopczuk, W., J. Marion, E. Muehlegger, and J. Slemrod (2016). Does Tax-Collection Invariance Hold? Evasion and the Pass-through of State Diesel Taxes. *American Economic Journal: Economic Policy 8*(2), 1–36.

Langenmayr, D. (2015). Voluntary disclosure of evaded taxes — increasing revenue, or increasing incentives to evade? *Journal of Public Economics*, forthcoming. doi:10.1016/j.jpubeco.2015.08.007.

Mann, H. B. and D. R. Whitney (1947). On a test of whether one of two random variables is stochastically larger than the other. *The Annals of Mathematical Statistics 18*(1), 50–60.

Marino, R. and R. Zizza (2012). Personal income tax evasion in italy: An estimate by taxpayer's type. In M. Pickhardt and A. Prinz (Eds.), *Tax Evasion and the Shadow Economy*. Elgar.

Mittone, L. (2006). Dynamic behaviour in tax evasion: An experimental approach. *The Journal of Socio-Economics 35*(5), 813–835.

Myles, G. and R. A. Naylor (1996). A model of tax evasion with group conformity and social customs. *European Journal of Political Economy 12*, 49–66.

OECD (2014). Tax administration 2013. Forum on tax administration.

Pisani, S. (2014). An Approch to assess how the activity of the Italian Revenue Agency affects compliance. Discussion Topics 1, Italian Revenue Agency.

Pomeranz, D. D. (2015). No Taxation without Information: Deterrence and Self-Enforcement in the Value Added Tax. *American Economic Review 105*(8), 2539–69.

Rincke, J. and C. Traxler (2011). Enforcement spillovers. *Review of Economics and Statistics 93*(4), 1224–1234.

Slemrod, J. (2016). Tax compliance and enforcement. new research and its policy implications. *SSRN Working Paper 2726077*.

Slemrod, J., M. Blumenthal, and C. Christian (2001). Taxpayer response to an increased probability of audit: evidence from a controlled experiment in Minnesota. *Journal of Public Economics 79*(3), 455–483.

Tversky, A. and D. Kahneman (1973). Availability: A heuristic for judging frequency and probability. *Cognitive psychology 5*(2), 207–232.

Yitzhaki, S. (1974). A note on income tax evasion: A theoretical anlysis. *Journal of Public Economics 3*(2), 201–202.

A Additional material

Table 2: List of sectors included in the database

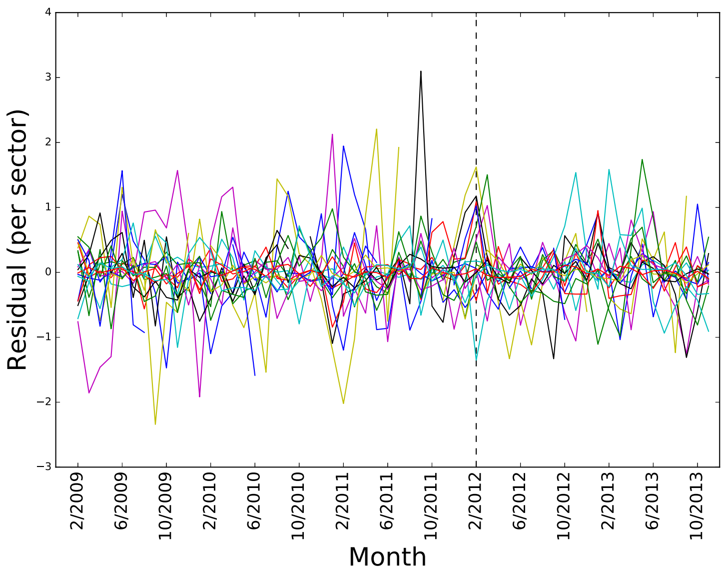
ISTAT code	Original name	Category
31	Fabbricazione di mobili	Furniture
43.22.01	Installazione di impianti idraulici, di riscaldamento e di condizionamento dell'aria (inclusa manutenzione e riparazione) in edifici o in altre opere di costruzione (idraulici)	Plumbers
43.3	Completamento e finitura di edifici (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
Clothing	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
Coffeehouses/bars	58	0.010	0.018
Discotheques	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 4: 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).