

# Essays in Applied Economics



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# The Media and the Diffusion of Information in Financial Markets: Evidence with Economic Policy Uncertainty News

## Abstract

In this paper we investigate the causal role of the media in disseminating information among financial operators. For this scope we consider the coverage of economic policy uncertainty (EPU) by media outlets in the USA. As a proxy for this coverage we implement the daily news based economic policy uncertainty index introduced by Baker et al. (2016). This index builds on the number of articles about EPU - normalized by the total number of articles - coming from a large set of US newspapers ranging from national journals like USA Today to small local newspapers across the country. To identify exogenous swings in coverage of EPU, we exploit the crowding out effect of publishable stories brought about by newsworthy sport events. In particular, we show that the abovementioned index systematically goes down the same days of the Olympics, the Worlds Series and the NBA finals. We interpret this evidence as supportive of the idea that these important occasions lead newspapers to publish in proportion less news about EPU, regardless the policy uncertainty of the economy. Then, we use our instrument to evaluate the effects of variations in EPU coverage on the VIX, a popular measure for expected volatility of the US stock market. According to our results, an increase of the daily news based EPU index by one standard deviation leads to a positive variation of the VIX equal to 3.61 points on the same day. These results are in line with the theoretical framework presented by Pastor and Veronesi (2013), with the media playing a role in disseminating EPU news to investors.

**Keywords:** economic policy uncertainty, volatility, media

## 1. Introduction

This paper adds to the literature about the role of the media in spreading information around investors in financial markets<sup>1</sup>. Indeed, previous research shows evidence for a causal effect of media activity on several financial variables. Engelberg and Parsons (2011) report that for an earnings announcement by a given S&P 500 Index firm, trading in 19 mutually exclusive US local markets is strongly related to whether local papers cover the announcement. Peress (2014) describes a causal impact of the media on stocks' trading intensity and on the second moments of their returns (dispersion, intraday volatility, and autocorrelations). These authors argue that their results could be related to the concept of “limited attention” of investors put forward by researches in behavioural finance (e.g. Tetlock, 2011; Engelberg and Parsons, 2011; Peress, 2014; Barber and Odean, 2008; Hirshleifer et al., 2009; Fang et al., 2014; Da et al., 2011). Indeed, acquiring and processing information needs time and monetary, technological, and cognitive resources (Kahneman, 1973). On the other hand, news by the media are arguably one of the cheapest sources of information in terms of both monetary and acquisition/fruition costs. Therefore, it is reasonable to think that media outlets could have a role in spreading information among certain investors, especially if not professionals (Peress, 2014).

Proving empirically this linkage is problematic as a well known identification problem arises. In fact, implementing articles in the media as an explanatory variable of financial market data has a non negligible drawback. Media coverage is not random, but rather the product of profit maximization by newspapers, television, magazines, etc. (Gentzkow and Shapiro, 2010). Thus, any causal analysis is challeng-

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<sup>1</sup>In line with most of the literature about the topic, “media” refers broadly to the main means of mass communication (broadcasting, publishing and the Internet), regarded collectively. Accordingly, “media coverage” is the “attention” diverted by these mediums towards a specific topic (measured for example by minutes devoted to cover it, by the presence of one or more articles speaking about it, etc.). See for example Peress (2014), Eisensee and Stromberg (2007), Durante and Zhuravskaya (2018), Engelberg and Parsons (2011), Barber and Odean (2008), among others.

ing: a number of unobservable factors that influence these coverage decisions is very likely to affect also investors' behaviour; consequently, an endogeneity problem occurs.

In this paper we define a novel way to identify a causal role of media outlets in transmitting news to the financial market. Results coming from this approach not only suggest, in line with previous evidence, the existence of media coverage effects on investors' behaviour; they also expand this evidence over new dimensions. Moreover, the identification strategy we implement herein allows to overcome some weaknesses of preceding works on this subject.

Differently from previous authors, we start by focusing on the media coverage of economic policy uncertainty (EPU). Baker et al. (2016) build several indices which are intended to capture EPU. This is defined as unpredictability about who will make economic policy decisions, what economic policy actions will be undertaken and when, and the economic effects of these policy actions (Baker et al., 2016). Their variables capture both near-term concerns (e.g. when will the Fed adjust its policy rate) and longer term concerns (e.g. how to fund entitlement programs), as reflected in newspaper articles (Baker et al., 2016). In what follows we use one of these indices as a proxy for EPU coverage by US media: the daily news based EPU index (henceforth, EPU news index)<sup>2</sup>. We collect data of this index from 1990 to 2018. According to Pastor and Veronesi (2012, 2013) and Kelly et al. (2016),

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<sup>2</sup>Baker et al. (2016) also define a monthly version of their new news based EPU index, starting from the beginning of 1985. Their index reflects the scaled frequency of EPU articles in 10 leading US newspapers - USA Today, Miami Herald, Chicago Tribune, Washington Post, Los Angeles Times, Boston Globe, San Francisco Chronicle, Dallas Morning News, New York Times, and Wall Street Journal - that contain the following trio of terms: "economic" or "economy"; "uncertain" or "uncertainty"; and one or more of "Congress", "deficit", "Federal Reserve", "legislation", "regulation", or "White House" (Baker et al., 2016). Another component of this index draws on reports by the Congressional Budget Office that compile lists of temporary federal tax code provisions. Temporary tax measures are a source of uncertainty for businesses and households because Congress often extends them at the last minute, undermining stability and certainty about the tax code. A third component of the index draws on the Federal Reserve Bank of Philadelphia's Survey of Professional Forecasters. They measure dispersion in the individual-level data for three of the forecast variables directly influenced by government policy: Cochrane and Piazzesi return forecasting factors, purchases of goods and services by state and local governments, and purchases of goods and services by the federal government (see [www.policyuncertainty.com/us\\_monthly.html](http://www.policyuncertainty.com/us_monthly.html) for further details).

political uncertainty affects firms' expected profits and carries a "risk premium". Therefore, if the media are a source of genuine EPU information for investors - for example due to operators' "limited attention" and resource constraints - a greater coverage of EPU - namely, a greater probability of receiving news about EPU - should be associated with greater stock (or option) market volatility. In fact, according to Pastor and Veronesi (2013) as operators receive fresh updates about political uncertainty they revise their expectations about stock returns and react consequently. It follows that a neat hypothesis about a plausible effect of media coverage of EPU on financial markets can be tested; and this clearly helps identification. Thus, in our main econometric models we implement as dependent variable the Chicago Board of Exchange Volatility Index (VIX), a popular measure of US expected stock market volatility. Then, to overcome the endogeneity issue put forward by Gentzkow and Shapiro (2010) we proceed as follows. We first notice that our proxy for EPU coverage, namely the EPU news index, is based only on articles coming from a large sample of US newspapers<sup>3</sup>; this observation is crucial. Indeed, in their works about public opinion distraction and strategic behaviours of a government, both Eisensee and Stromberg (2007) and Durante and Zuhrovskaya (2018) document an existing competition among publishable stories, especially when newsworthy events are around. The mechanism is fairly simple: newsworthy events such as natural disasters, important sport events, summits, etc. get greater coverage by the media and crowd out other stories. With this intuition in mind we show that the EPU news index systematically lowers when either the Olympics, the World Series or the NBA finals approach. These results seem to point to the existence of a significant variation in coverage of EPU brought about by these important sport events<sup>4</sup>. Their timing is exogenously scheduled; as well, it seems fairly

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<sup>3</sup>This is important as it clarifies what type of information source is considered for constructing the EPU index: newspapers. Therefore, this variable allows us to study the effect of EPU media coverage variations on investors' behaviour. As previously clarified, the monthly version of the same index is not based on newspapers only.

<sup>4</sup>In the explorative stage presented later, we also test the impact of natural and technological disasters happening in the USA (since the EPU news index is based on US media outlets) with a number of fatalities above the 75<sup>o</sup> percentile for each category. However, no significant effect is found.

reasonable to think such events as not directly correlated with financial markets and their volatilities. Thus, we exploit them as an instrument for exogenous variability in EPU coverage to identify causal effects of the media on expected volatility as measured by the VIX.

According to our IV calculations, an increase of the EPU news index by one standard deviation leads to a positive variation of the VIX equal to 3.61 points on the same day<sup>5</sup>. Such a result seems in line with the aforementioned conclusions in Pastor and Veronesi (2012, 2013) and Kelly et al. (2016) of political uncertainty carrying a risk for investors; as well, it points to the media as a source of primary information about EPU. Robustness tests show that there is no systematic sign of anticipation by investors of other expected events - which could affect volatility - timed (strategically) when the newsworthy sport events in our first stage take place.

With this paper we contribute to several streams of research. Firstly, we add to the literature about the role of the media in financial markets. Previous studies show an association between media activity and stock market movements (e.g., Huberman and Regev, 2001; Hirsheifler et al., 2009; Tetlock, 2007, 2011; Fang and Peress, 2009; Fang et al., 2014). We relate more closely to Engelberg and Parsons (2011) and Peress (2014) which are the first to establish a more convincing causal effect of media coverage<sup>6</sup>. However, our evidence using EPU news and the VIX expands previous results and it sheds some light on a plausible mechanism - in line with the model in Pastor and Veronesi (2013) - be-

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<sup>5</sup>The average value of the VIX over the considered period is 19.35 points. See Table A.4 in Appendix for further details.

<sup>6</sup>Also Dougal et al. (2012) develop a causal analysis of the impact of newspapers on financial markets. These authors find that the identity of Wall Street Journal columnists is a good predictor of the next-day return on the Dow Jones Industrial Average. However, they appeal to a sentiment story whereby the bullish or bearish sentiment conveyed by columnists influences investors. The identification strategy of their article is based on two assumptions. The first is that authors of the Wall Street Journal's "Abreast of the Market" column exhibit persistent stylistic differences, such that even for the same set of facts, article content will vary. The second is that the selection of journalists is not systematically related to future returns (Dougal et al., 2012). All these details clearly suggest that Dougal et al. (2012) document a distinct causal phenomenon with respect to the role that the media have in disseminating information to investors - core instead of Peress (2014), Engelberg and Parsons (2011), as well as of this research.

hind the relation between the media and financial markets not explored before: in the event study by Peress (2014) there is no specification about news types; the evidence in Engelberg and Parsons (2011) comes only from coverage of earnings announcements by local media outlets. As well, our contribution to this literature is also methodological. Peress (2014) exploits newspaper strikes as an exogenous source of media coverage swings. According to the author, most of these strikes relates to the economic conditions of employees (e.g. employment rules, pay levels, pensions, tax breaks, state subsidies) and with fights for freedom of the press against censorship (Peress, 2014). It sounds plausible to think some of these nationwide newspaper strikes being correlated with the economic conditions of the country in which they set, especially during economic distresses. In turn, these events might correlate with financial markets' behaviour because of other unobservable reasons, leading to a biased estimation of the media coverage effect. On the contrary, exogeneity of the schedule of newsworthy sport events is arguably more convincing in this case. Furthermore, given that the EPU news index spans over pre-Internet and actual Internet era, this study overcomes also the main limitation of the identification procedure in Engelberg and Parsons (2011): the validity of their results for more recent years (i.e. with the predominance of the Internet) is questionable. Indeed, the two authors base their identification strategy on the assumption that, during their pre-Internet sample period between 1991 and 1996, investors near a big city such as Minneapolis were more likely to read the Minneapolis Star Tribune than local papers of other regions (Engelberg and Parsons, 2011). However, it is clear that nowadays - thanks to the Internet and many recent technology advancements such as the smartphones - this link between local papers and local investor activity is less convincing. By defining an identification strategy with a time horizon which spans until recent years, our research tries to solve the underlined limitation as well.

By focusing on EPU we also relate to the growing literature about the topic (e.g. Baker et al., 2019; Pastor and Veronesi, 2012, 2013; Bloom et al, 2018). Results reported here are among the few attempts to find causal evidence about the effects of EPU (through media cover-



age) on financial markets (in particular, volatility). Kelly et al. (2016) is another noticeable work which uses, however, a different approach. Scholars obtain a closed form solution for the prices of put options whose lives span a political event. Then, in order to isolate political uncertainty, Kelly et al. (2016) exploit its variation around major political events, namely, national elections and global summits: since the political event is often the main event that occurs during the option's short life, the option's price is informative about the value of protection against political risk (Kelly et al., 2016). According to their results, political uncertainty is priced in the option market, confirming their predictions about a risk premium associated with EPU. However, it is clear that in our work we are documenting a different phenomenon.

The remainder of this paper is organized as follows. In Section 2 we discuss why the media could have an effect on investors' behaviour and what can be testable implications when considering in particular EPU news coverage. In Section 3 we present in details the data and the empirical strategy used to identify an exogenous variation of EPU media coverage as well as the results about the impact of these swings on the VIX. In Section 4 we conduct a robustness exercise to check for an alternative explanation behind the results. Section 5 concludes.

## **2. Related literature**

### **2.1. Why could the media have an effect on investors?**

In textbook financial markets, arbitrage renders media contribution usually insignificant: public news is incorporated in asset prices quickly and fully, even before the media have time to report it. An implicit assumption in these models is that agents are able to continuously process all the new available information, without relevant costs. However, a large body of evidence criticizes this hypothesis of frictionless information spreading: news does not diffuse instantly through the investor population but it spreads gradually, affecting prices (Peress, 2014). In particular, the media seem to have a role in this dissemination:

«A Sunday New York Times article on a potential development of new cancer-curing drugs caused EntreMed's stock price to rise from 12.063 at the Friday close, to open at 85 and close near 52 on Monday. [...] The potential breakthrough in cancer research already had been reported, however, in various popular newspapers (including the Times) more than five months earlier». (Huberman and Regev, 2001; p.387)

We connect our main results to this stream of research. In particular, we relate to previous evidence that shows a causal role of the media in diffusing information to the financial markets. Engelberg and Parsons (2011) show that trades by investors located in various US cities respond to business news coverage by local newspapers distributed in these cities. After controlling for earnings as well as investor and newspaper characteristics, local press coverage increases the daily trading volume of local retail investors, from 8% to nearly 50% depending on the specification. Moreover, Engelberg and Parsons (2011) show that local trading is strongly related to the timing of local reporting, a particular challenge to non media explanations. To cope with endogeneity, the two authors use retail brokerage accounts to identify local nonoverlapping trading markets surrounding major US cities. Then, for each of these markets they identify a local information source: the daily newspaper of that city. Authors argue that during their pre-Internet sample period from 1991 to 1996, investors near, for example, Seattle were more likely to read the Seattle Post Intelligencer and not papers from other cities. This linkage and the fact that local media outlets often differ in their coverage timing of the same events afford a test of the media's effects on financial market participants (Engelberg and Parsons, 2011). Peress (2014) turns instead to national level data and relies on large scale strikes of journalists and other media related professionals to capture an exogenous reduction of press coverage. More in details, the author identifies strikes in the print media that prevent readers from receiving news: for this purpose, he searches for strikes that affect the press on a national scale, involve the media sector only, and occur on days during which stock markets are open (Peress, 2014). Over the period from 1989 to 2010 the scholar finds 52 eligible national

newspaper strikes which concentrate in France, Greece, Italy, and Norway. According to the study, in these countries trading volume falls by 12% on strike days. Furthermore, the difference between high and low daily prices, as well as intraday volatility, are reduced on average by 7%. According to the author, this evidence demonstrates that the media contribute to the efficiency of the stock market by improving dissemination of information among investors and its incorporation (Peress, 2014). In line with Engelberg and Parsons (2011), also in this case media most plausibly influence individual investors; on the contrary, professionals seem to be less affected. In fact, the above-mentioned effects are more pronounced among small stocks, which are predominately owned by retail investors (Peress, 2014).

The fact that mostly holders of small stocks rely on the press for obtaining news about the economy seems to support the behavioural finance concept of “limited attention” of investors. According to this stream of research, it is hard for humans to process multiple stimuli and perform multiple tasks at the same time (Hirsheifler et al., 2009). Furthermore, acquiring and processing information requires time as well as monetary, technological, and cognitive resources (Kahneman, 1973)<sup>7</sup>. Results in Peress (2014) and Engelberg and Parsons (2011) support this hypothesis and show that these frictions to information spreading could be binding, especially for individual investors. In fact, according to their evidence investors respond to a heterogeneous newspaper coverage of news. Thus, operators do not screen all sources of information every time  $t$ , but rely on a subset of them - according to their resources; secondly, also the media are among the channels used by investors to gather information. Then, information sources (media outlets, newswire services, institutional websites, data providers, etc.) seem not perfect substitutes. In particular, news provided by the media are arguably cheap, with low costs for acquisition and fruition; as (individual) investors have limited monetary and technological resources, it is likely the case that, on average, some of them are not

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<sup>7</sup>It seems reasonable to think individual operators suffering more of limited attention problems with respect to professional, well equipped ones. However, Fang et al. (2014) show that also mutual funds, a category of professional investors, seem to suffer of limited attention.

able to switch to other information providers without unsustainable efforts. And such a friction opens up room for the media to play a role in shaping investors' expectations. In line with the revised literature, also our results seem to confirm it.

In such a context it is reasonable to think that the more newspapers talk about a topic (e.g. EPU) - i.e. the more its coverage - the greater the probability it grabs the (limited) attention of (individual) investors.

## 2.2. EPU and stock market volatility

In our identification strategy we rely upon EPU news as covered by the media. In their seminal papers, Pastor and Veronesi (2012, 2013) interpret EPU as uncertainty about government's future actions. These actions have an impact on firms' profitability, as scholars specifically interpret policy changes broadly as government actions that change the economic environment (Pastor and Veronesi, 2013)<sup>8</sup>. EPU has become a relevant issue over recent years. As reported by Bloom et al. (2019), there is evidence of an expanding role of the government in the economy and, contemporaneously, an upward trend in policy-related economic uncertainty. In particular, for the case of United States authors underline a secular growth in government expenditures as a share of GDP, an increasing scale and complexity of the regulatory system, an increasing complexity of the federal tax code, a growing share of business "risk factors" that US firms attribute to government policy in their 10-K filings, a secular rise in the newspaper-based EPU index of Baker et al. (2016). Since these long-term developments show little sign of reversal, policy concerns are likely to remain a major source of uncertainty for many years (Bloom et al. 2019).

But how would investors react to EPU news? According to our IV estimations, the VIX rises when EPU gets more covered by US media, signalling a greater expected volatility of the stock market. Such a result seems in line with the theoretical framework put forward by Pastor

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<sup>8</sup>In their papers, Pastor and Veronesi (2012, 2013) refer to "political uncertainty" and not explicitly to "economic" policy uncertainty. However, it is clear from the revised definition that the two expressions refer to the same object, as economic aspects are involved in both cases.

and Veronesi (2013) for stocks, and extended to options by Kelly et al. (2016). According to Pastor and Veronesi (2013), when making its policy decisions the government is motivated by both economic and non-economic objectives: it maximizes investors' welfare, as a social planner would, but it also takes into account the political costs/benefits associated with adopting any given policy. Since these political costs are private information, operators do not know exactly what final policy the government is going to take. This in turn represents the source of political uncertainty in the model, which affects firms' expected profits and then stock returns. Over time, investors receive signals about the unknown political costs of the policies and update their beliefs about government's future actions. Among other predictions, the general equilibrium model in Pastor and Veronesi (2013) foresees a greater volatility of stocks in response to these political news. Kelly et al. (2016) extend the model in Pastor and Veronesi (2013) also for the case of options, and it is one of the very few papers attempting to find a causal relation between EPU and financial outcomes. More in details, they calculate a closed form solution for the prices of put options whose lives span a political event. Then, they calculate three option market variables: implied volatility of an at-the-money option, slope of the function relating implied volatility to moneyness, and variance risk premium (Kelly et al., 2016)<sup>9</sup>. The model implies that all three option market variables should be larger, on average, than the same variables calculated for options whose lives do not span a political event. To isolate political uncertainty, Kelly et al. (2016) exploit its swings around major political events, such as national elections and global summits: since the political event is often the main event that occurs during the option's short life, the option's price is informative about the value of protection against political risks (Kelly et al., 2016). Options provide valuable protection against the risk associated with major political events. In fact, political uncertainty is priced in the option market: option protection against price, variance, and tail risks is more expensive before a political event.

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<sup>9</sup>These variables capture the value of option protection against three aspects of risk associated with political events: price risk, tail risk, and variance risk, respectively.

In the light of the revised literature, assuming that the media could be one of the sources of genuine information for some investors (due to limited attention and resource constraints), a greater coverage of EPU translates into a greater probability of receiving news about EPU. *Ceteris paribus*, this should lead to an increase in stock (or option) market volatility because of a more probable updating process of expectations by agents (Pastor and Veronesi, 2012, 2013; Kelly et al., 2016). Such a neat prediction is at the heart of the identification strategy of this paper, as it makes clear that, if the media have a role in spreading EPU in the economic system, theory foresees a positive effect on volatility associated with it. The results we present in Section 3 are supportive of such a prediction.

### **3. Data & Empirical strategy**

#### **3.1. Media coverage and the EPU news index by Baker et al. (2016)**

In our paper we rely on the coverage of EPU by the media to study the role of these means of communication in spreading information to financial markets. For this scope we consider the daily news based economic policy uncertainty index introduced by Baker et al. (2016) for the USA. We implement it as a proxy for the coverage of EPU in the country. This index is freely downloadable from the authors' website<sup>10</sup>. Time span of the data goes from the 1<sup>st</sup> of January, 1990, till the 15<sup>th</sup> of April, 2018<sup>11</sup>. Descriptive statistics of the series can be found in Table A.1 in Appendix. This daily index is based on articles coming from newspaper archives of the Access World News's NewsBank service<sup>12</sup>. The NewsBank's Access World News database contains the archives of thousands of newspapers and other news sources from across the globe. While NewsBank has a wide range of news sources, from

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<sup>10</sup>[www.policyuncertainty.com](http://www.policyuncertainty.com).

<sup>11</sup>The sample starts in January 1990 in accordance with the availability of VIX data.

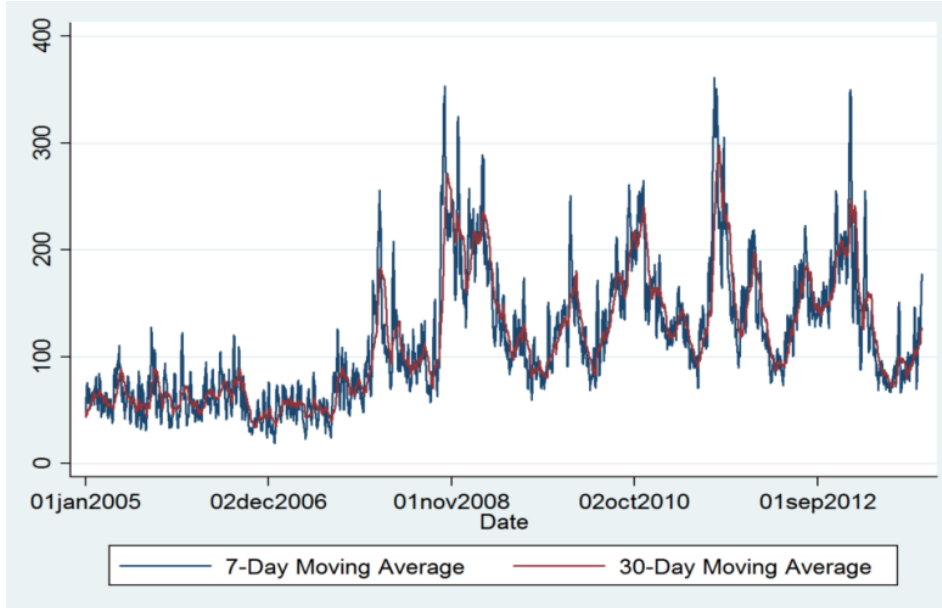
<sup>12</sup>[www.newsbank.com/libraries/schools/solutions/us-international/access-world-news](http://www.newsbank.com/libraries/schools/solutions/us-international/access-world-news).

newspapers to magazines to newswire services, authors conduct their analysis utilizing newspapers only. This is important, as the focus of our research is specifically on media coverage effects. Moreover, Baker et al. (2016) restrict the attention to newspapers in the United States, of which NewsBank covers well over 1000. These newspapers range from large national papers like USA Today to small local newspapers across the country. We think that this fact, plus the huge number of media outlets involved for the construction of the index, reassure about the plausibility of using this measure as a meaningful proxy for EPU coverage by the media in the US. The available data span from 1985 and are updated daily. Primary measure for this index is the number of articles that talk about EPU. For this purpose, Baker et al. (2016) look for articles that contain at least one term from each of the following three sets of terms. The first set is “economic” or “economy”. The second is “uncertain” or “uncertainty” . The third set is “legislation” or “deficit” or “regulation” or “congress” or “federal reserve” or “white house”<sup>13</sup>.

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<sup>13</sup>By considering the EPU news index of Baker et al. (2016) as a proxy for media coverage of EPU we somehow mimic the procedures in Durante and Zhuravskaya (2018) and Eisensee and Stromberg (2007) - although they focus on TV news broadcasts instead of newspapers. Indeed, these authors capture news coverage through indicator variables for whether a relevant news (about natural disasters or the Israeli-palestinian conflict) appeared in one of the top four TV news broadcast in the US. Similarly, the EPU news index is based on the number of journal articles talking about the topic. One could also argue that, since the EPU news index is based on many more media channels than only the top four TV broadcasters considered by Durante and Zhuravskaya (2018) and Eisensee and Stromberg (2007), it could be a more informative proxy for (EPU) coverage by the media sector. Thus, results in this research about effects of (EPU) media coverage swings (on financial markets) might be more reliable.

Fig. 1  
*The EPU news index*



*Notes:* moving average representations of a portion of the EPU news index. A 7-day moving average (current day and 6 days prior) and a 30-day moving average (current day and 29 days prior).

The number of newspapers that NewsBank covers has drastically increased over the sample period: from 18 in 1985 to over 1800 by 2008. To correct for this growth, authors normalize the number of economic policy uncertainty articles by taking daily counts of the total number of newspaper articles. However, as also noted by Baker et al. (2016), because papers enter and leave the Newsbank archive, and its count of newspapers expands greatly over time, compositional shifts potentially distort the longer term behaviour of the EPU news index. Moreover, in November 2012 the data change from being derived from the number of articles with the word “today” in their text; they are now based on the entire daily set of articles. However, data preceding the 2012 still use articles from the “today” series.

Therefore, we are aware of the main limitations of using, as in this study, a large portion of the EPU daily series; unfortunately, to the



best of our knowledge there are no more reliable alternatives. Thus, we assume that the compositional limits of the EPU news index do not compromise the final results.

### 3.2. Competition among news

Ideally, we would like to use the EPU news index to understand if shifts in the coverage of EPU by the media could have an impact on investors' behaviour. However, since this index is based on articles coming from journals, an endogeneity problem occurs. In fact, the decision of a media outlet to cover a certain topic is mostly the product of profit maximization; it does not happen at random (Gentzkow and Shapiro, 2010). Thus, causal analysis is challenging in this case: a number of unobservable factors that influence the decision to publish news about EPU may also affect investors' behaviour, leading to an identification problem. As well, correlation could result from reverse causality since the media may also report newsworthy market developments.

To overcome the underlined endogeneity issue, we rely upon the competition among publishable news documented by Eisensee and Stromberg (2007) and Durante and Zhuravskaya (2018). Eisensee and Stromberg (2007) study the influence of mass media on aid response of the US government to approximately 5000 natural disasters occurring between 1968 and 2002 all around the world. According to their evidence, reliefs by the US government in response to natural disasters strongly depend on whether the disaster occurs at the same time of other big events unrelated to need, such as the Olympics. To show this, the two authors start by noticing that if two equally newsworthy disasters occur, it is reasonable to assume that the one occurring when there is a great deal of other breaking news around would have lower chance of being covered<sup>14</sup>.

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<sup>14</sup>As authors suggest, this crowding out effect is probably stronger for television news broadcasts that are usually of a fixed length (half an hour for ABC, CBS, NBC and one hour for CNN). However, the explorative results here in this section suggest that this effect might be significant also for newspapers.

Fig.2

*The competition among news*



Notes: (left): Beijing Olympics and Russia-Georgia war; example 2 (center): FIFA World Cup and Israeli attack on Gaza; example 3 (right): FIFA World Cup and the “Save the Thief” Decree by ex Italian prime minister Silvio Berlusconi. Source: Durante and Zhuravskaya (2018).

To test this intuition, Eisensee and Stromberg (2007) rely on the Olympics since it is an event that is largely discussed by the media and it does not relate to politics and thus, to relief decisions<sup>15</sup>. News coverage is captured by an indicator variable for whether a disaster was covered in the major US national broadcast networks. More in details, they consider television news coming from the Vanderbilt Television News Archive which contains more than 30000 individual network evening news broadcasts and 700000 news stories from the major US national broadcast networks (ABC, CBS, NBC, and CNN) since 1968. According to their results, the Olympics are negatively correlated with the probability that networks cover a disaster<sup>16</sup>. Durante and Zhuravskaya (2018) investigate empirically whether politicians choose the

<sup>15</sup>According to Eisensee and Stromberg (2007), the World Series generates one-third as many network news stories as the Olympics on the day of, or after, the event. Both the Academy Awards and the Super Bowl generate less than one-twelfth as many stories.

<sup>16</sup>Eisensee and Stromberg (2007) define the following model for their first stage equation. A latent variable  $news_i^*$  (newsworthiness) describes the benefits of covering disaster  $i$  from the TV network’s perspective,

$$news_i^* = \beta_1 news\ pressure_i + \beta_2 Olympics_i + \beta' X_i + \mu_i.$$

timing of their unpopular policies strategically to minimize news coverage and thus, the negative impact on public opinion. In particular, they focus on the timing of military operations during an ongoing conflict that attracts considerable media attention: the Israeli-Palestinian conflict. To test their strategic timing hypothesis, authors collect daily time-series data on the occurrence and severity of both Israeli military operations in the West Bank and Gaza strip and attacks by Palestinian militant groups on the Israeli territory between 2000 and 2011. Then, they identify all stories devoted to the Israeli-Palestinian conflict and construct a dummy for whether at least one conflict-related story appears on NBC, ABC, CNN or CBS and for the total length of conflict-related news stories appearing on these four networks on a given day. They combine these data with a measure of the presence of other newsworthy events in the US media. In particular, the two scholars compile a list of important predictable US political and sports events which are likely to increment news pressure. For example, among the political events there are lags and leads of the presidential elections as well as presidential caucuses. The FIFA World Cup on the other hand is the only big sport event that correlates significantly with

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This latent variable determines the news decision according to

$$\begin{aligned} news_i &= 1 \quad \text{if } news_i^* > 0 \\ news_i &= 0 \quad \text{if } news_i^* \leq 0 \end{aligned}$$

where  $news_i$  is an indicator variable for whether a disaster was covered in a news broadcast within a certain time span/window. The vector  $X_i$  contains disaster specific variables and fixed effects for disaster type, country, year, etc.  $\mu_i$  is unobserved newsworthiness. Other than *Olympics<sub>i</sub>*, Eisensee and Stromberg (2007) use also another instrumental variable for the availability of newsworthy material: the average of “daily news pressure” over the forty days following disaster  $i$  (called simply “news pressure” by the authors). “Daily news pressure” is calculated as the median (across broadcasts in a day) number of minutes a news broadcast devotes to the top three news segments in a day. The forty days averaging is motivated by the fact that crude “daily news pressure” could be endogenous to stories about disasters, since the airing of a disaster story will affect the amount of time devoted to the top three stories. In case we would be interested in defining a similar instrument for the case of EPU stories, this endogeneity issue is very likely to occur. Moreover, differently from disasters in Eisensee and Stromberg (2007) the frequency of episodes leading to EPU news is much higher and no reliable averaging trick is therefore applicable. Thus, in our identification strategy we do not consider “news pressure” as an instrument for the daily EPU index.

news pressure<sup>17</sup>. Using a dummy based on the occurrence of these major events, Durante and Zhuravskaya (2018) show that the likelihood of deadly military attacks by Israeli forces against Palestinians, as well as the number of casualties, is positively and significantly related to the level of news pressure by predictable newsworthy events on the day after the attack. In contrast, they find no evidence that attacks by Palestinian militant groups are timed to US news pressure. These results suggest that Israeli authorities may have strategically chose the timing of their attacks to minimize negative publicity in the United States<sup>18</sup>.

To sum up, both Eisensee and Stromberg (2007) and Durante and Zuhravskaya (2018) document an existing competition among publishable stories, especially intense when there are events worth of coverage. Both papers ground this consideration focusing on news television broadcasts that are usually of a fixed length (half an hour for ABC, CBS, NBC and one hour for CNN). This time constraint naturally leads to competition among stories for a spot. However, as argued by Durante and Zuhravskaya (2018), a very similar mechanism most likely extends to other medium of news such as newspapers. Indeed, when there are newsworthy events around, media divert their attention (and resources) towards them, at the expense of other reportable facts. Assuming that budgeting and timing issues - and also physical limits, especially for the paper versions of journals - bind the maximum number of articles a media outlet can produce each day, the aforementioned competition among stories may emerge also for the case of newspapers. In line with these conclusions, we claim that if there are other newsworthy events around, they either crowd out EPU stories or compete for a greater coverage with them. In both cases, the proportion of articles devoted by journals to EPU should decrease, leading the EPU news

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<sup>17</sup>Durante and Zuhravskaya (2018) also test the Olympics but its coefficient is not statistically significant.

<sup>18</sup>To make sure that the predictability of newsworthy events is key for their results, Durante and Zhuravskaya (2018) conduct a placebo experiment which uses the onset of disasters in the US as a driver of news pressure. Differently from sports and political events, disasters cannot be predicted with certainty. Strikingly, scholars show that news pressure driven by unpredictable disasters has no effect on the timing of Israeli attacks or their severity.

index to lower as well; a shift unrelated to the true level of uncertainty in the economic system as long as the considered newsworthy events do not affect EPU. In this regard, we want to identify exogenous swings of EPU coverage in the US media by exploiting the greater news pressure that big sport events and natural disasters trigger. Timing of these events is exogenously scheduled and not related with EPU; as well, it seems fairly reasonable to think such events as not directly correlated with financial markets.

To test our claim, we start by defining the following model:

$$EPU\ coverage_{i,t} = \beta'\theta_t + \pi Disasters_t + \delta Victims_t + \rho'\gamma_t + \vartheta_i + \varepsilon_{i,t} \quad (1)$$

where  $EPU\ coverage_{i,t}$  is the fraction of articles devoted to EPU by US newspaper  $i$  at time  $t$ .  $\theta_t$  is a vector of lead dummy variables equal to 1 if the corresponding newsworthy events take place in  $t+1$ , and 0 otherwise<sup>19</sup>. Vector  $\theta_t$  contains indicators for the following events: Olympics, FIFA World Cups, UEFA Euro Cups, World Series, March Madness, NBA Finals<sup>20</sup>. Similarly to Durante and Zuhravskaya (2018),  $Disasters_t$  is a dummy variable equal to 1 if either a natural or a technological disaster as defined in the International Disaster Database occurs at time  $t$  in the US<sup>21</sup>. Only those disasters of which the number of fatalities is above the 75<sup>o</sup> percentile in their categories are considered<sup>22</sup>.  $Victims_t$  is the natural logarithm of the number of victims (+1) of the corresponding tragedy in  $Disasters_t$ . Further details about the variables can be found in Tables A.2 and A.3 in Appendix.  $\gamma_t$  is a vector containing day of the week, monthly and yearly time dummies<sup>23</sup>;  $\vartheta_i$  are media specific fixed effects.  $\varepsilon_{i,t}$  is the error term.

<sup>19</sup>Anticipation by the media in covering a scheduled big event is documented, for instance, in Durante and Zhuravskaya (2018).

<sup>20</sup>Since the EPU news index is based on data coming from US media, most of the abovementioned dummies refer to sports which are popular in the US. Indeed, they are more likely to be covered by local media outlet (Durante and Zuhravskaya, 2018).

<sup>21</sup>Source: Center for Research on the Epidemiology of Disasters ([www.emdat.be](http://www.emdat.be)).

<sup>22</sup>There is an overlap between two events on the 4th of September, 2011. It counts as just one. Number of fatalities is summed together (15 + 4).

<sup>23</sup>Time span for this study goes from the beginning of the 90's until April, 2018, and it embraces both pre-Internet and Internet data. Over this period, the media industry has deeply changed, mostly because of the Internet. It seems reasonable to think the crowding out effect of stories

Unfortunately, data for each media outlet  $i$  are not available; namely, the daily coverage of EPU by each newspaper in the US is not directly observable; only a more aggregated proxy, the EPU news index as defined by Baker et al. (2016), is accessible. Thus, at this stage we assume that if the newsworthy events in the underlined Model 1 have on average a negative effect on  $EPU\ coverage_{i,t}$ , they have a negative impact also on the EPU news index. After these considerations, the resulting final specification is the more compact form of Equation 1 as specified below:

$$EPU\ Index_t = \alpha'\theta_t + \rho Disasters_t + \varphi Victims_t + \tau'\gamma_t + \mu_t \quad (2)$$

where  $EPU\ Index_t$  is the EPU news index as defined by Baker et al. (2016) observed in  $t$ .  $\theta_t$ ,  $Disasters_t$ ,  $Victims_t$  and  $\gamma_t$  are defined as in Equation 1;  $\mu_t$  is the error term at time  $t$ .

Table 1 below contains the results from different specifications of Equation 2. Results are striking. Indeed, especially from Specification 4 of the table is clear that the most significant impact on EPU coverage comes from very popular sport events in the US (again, we recall that the dependent variable is based on articles coming from US media outlets only). Also, the negative sign of the three coefficients reassures about the plausibility of the hypothesis behind the model: the daily proportion of articles about EPU is lowered by the presence of other newsworthy events during the same period because of news competition. Below in Table 2 we propose the same model, but considering only those days when stock markets are open – this being the baseline for our applications with financial variables. Again, the Olympics, the NBA finals and the World Series have a negative impact on the EPU news index even though coefficients are slightly smaller in magnitude and less significant with respect to Table 1.

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due to newsworthy events being stronger in the past, especially in the case of printed journals (physical limits imposed by the paper format could arguably have an analogous effect of TV news broadcast time limits). Year dummies in our models are intended to control also for these technological shifts over time.

Tab.1

*Crowding out effect of newsworthy events on the media coverage of EPU*

	<i>Dependent variable:</i>			
	EPU news index			
	(1)	(2)	(3)	(4)
Olympics (lead)	-15.670*** (5.912)		-15.663*** (5.914)	-15.973*** (5.892)
World Cup (lead)	-3.531 (6.680)		-3.483 (6.680)	
Euro Cup (lead)	16.324 (12.822)		16.325 (12.823)	
World Series (lead)	-13.013** (6.009)		-12.966** (6.013)	-13.026** (5.982)
March Madness (lead)	4.324 (4.985)		4.318 (4.987)	
NBA Finals (lead)	-8.856** (4.390)		-8.898** (4.387)	-8.726** (4.367)
Disasters		-12.262 (10.380)	-11.870 (10.512)	
Victims		3.534 (3.130)	3.310 (3.137)	
Year, Month, Dow FE	YES	YES	YES	YES
Observations	10,332	10,332	10,332	10,332
R <sup>2</sup>	0.243	0.240	0.243	0.242
Adjusted R <sup>2</sup>	0.239	0.236	0.239	0.238

*Notes:* regression results from different specifications of Equation 2. Dependent variable is the daily news based economic policy uncertainty index by Baker et al. (2016) for the USA. Time span: 1<sup>st</sup> of January, 1990 – 15<sup>th</sup> of April, 2018. Newey-West (1994) robust standard errors in parentheses. \* p<0.1; \*\* p<0.05; \*\*\* p<0.01.

Tab.2

*Crowding out effect of newsworthy events on the media coverage of EPU (only days when stock markets are open)*

	<i>Dependent variable:</i>			
	EPU news index			
	(1)	(2)	(3)	(4)
Olympics (lead)	-14.369** (7.028)		-14.355** (7.017)	-14.830** (7.001)
World Cup (lead)	0.130 (7.790)		0.354 (7.770)	
Euro Cup (lead)	17.725 (12.301)		17.800 (12.296)	
World Series (lead)	-12.570* (6.825)		-12.500* (6.845)	-12.577* (6.820)
March Madness (lead)	4.806 (5.314)		4.781 (5.335)	
NBA finals (lead)	-7.365* (4.014)		-7.364* (4.023)	-7.873* (4.022)
Disasters		-15.460 (12.871)	-14.517 (12.991)	
Victims		3.228 (3.751)	2.904 (3.769)	
Year, Month, Dow FE	YES	YES	YES	YES
Observations	7,128	7,128	7,128	7,128
R <sup>2</sup>	0.238	0.235	0.238	0.237
Adjusted R <sup>2</sup>	0.232	0.230	0.232	0.232

*Notes:* regression results from different specifications of Equation 2. Dependent variable is the daily news based economic policy uncertainty index by Baker et al. (2016) for the USA. Time span: 2<sup>nd</sup> of January, 1990 – 13<sup>th</sup> of April, 2018. Only days in which financial markets are open. Newey-West (1994) robust standard errors in parentheses. \* p<0.1; \*\* p<0.05; \*\*\* p<0.01.

### 3.3. First stage results

Similarly to Durante and Zuhravskaya (2018) we define a new dummy variable, *Newsworthy Events (lead)<sub>t</sub>*, which equals 1 in correspondence to the lead dummies for the Olympics, the World Series and



NBA finals<sup>24</sup>. Indeed, according to the models in Tables 1 and 2 they are the only events having a significant impact on the EPU news index.

The final first stage equation can be rewritten as:

$$EPU\ Index_t = \omega Newsworthy\ Events(lead)_t + \tau'\gamma_t + \mu_t \quad (3)$$

Table 3 below contains the results from Model 3, both for the entire sample and only when financial markets are open. In this table we also present the results from two F-tests intended to check for weak instruments problems.

Tab.3  
*First stage results*

	<i>Dependent variable:</i>	
	EPU news index	
	(1)	(2)
	All sample	Baseline
Newsworthy Events (lead)	-12.618*** (3.202)	-11.714*** (3.549)
Year, Month, Dow FE	YES	YES
Observations	10,332	7,128
F-test (for weak instruments)	15.521	10.890
R <sup>2</sup>	0.241	0.237
Adjusted R <sup>2</sup>	0.238	0.232

*Notes:* regression results of Equation 3. Dependent variable is the daily news based economic policy uncertainty index by Baker et al. (2016). Time span for Model 1: 1<sup>st</sup> of January, 1990 – 15<sup>th</sup> of April, 2018. Time span for Model 2: 2<sup>nd</sup> of January, 1990 – 13<sup>th</sup> of April, 2018 (only days in which financial markets are open). Newey-West (1994) robust standard errors in parentheses. \* p<0.1; \*\* p<0.05; \*\*\* p<0.01.

<sup>24</sup>There are no overlaps among the three sport events.

In both specifications the value of the F-test exceeds the usual threshold of 10 for the case of one endogenous regressor (Staiger and Stock, 1997). Therefore, the instrument does not seem weak in this case. It follows that, as long as *Newsworthy Events* ( $lead$ ) $_t$  is considered exogenous to the financial market we can plausibly use it as an instrument to study the effect on agents' behaviour of variations in EPU coverage by the media.

### 3.4. Second stage results

Our second stage regression is based on the theoretical models by Pastor and Veronesi (2013) and Kelly et al. (2016): if the media have a role in disseminating signals about EPU to investors, as the probability of receiving EPU news increases due to greater EPU coverage, *ceteris paribus* (expected) volatility of the stock market should increase. To test this hypothesis, similarly to Pastor and Veronesi (2013) we use the VIX as a proxy for the expected stock market volatility in the USA<sup>25</sup>. The VIX is provided by the Chicago Board of Exchange and it uses S&P 500 bid/ask quotes of options with more than 23 days and less than 37 days to expiration and weights them to yield a constant measure of the expected volatility of the US stock market over the coming 30 days<sup>26</sup>. Given its definition, the VIX sounds appropriate to capture swings in investors' expectations about future market developments. We turn our attention to a forward-looking variable since EPU mostly relates to uncertain economic scenarios in the future. For our analysis we implement VIX closing values spanning from the 2<sup>nd</sup> of January, 1990 until the 13<sup>th</sup> of April, 2018. Further details about our time series can be found in Table A.4 in Appendix.

Table 4 presents the main results of this study and it reports different specifications of the following second stage regression equation:

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<sup>25</sup>We recall that we use the EPU news index by Baker et al. (2016) as a proxy for EPU coverage by US media. This explains the focus on the VIX data, measure of expected volatility of the US financial market.

<sup>26</sup>Data are freely downloadable from the Chicago Board of Exchange's website ([www.cboe.com/micro/buywrite/dailypricehistory.xls](http://www.cboe.com/micro/buywrite/dailypricehistory.xls)).

$$VIX_t = \beta \widehat{EPU\ Index}_t + \delta' X_t + \rho' \gamma_t + \mu_t \quad (4)$$

$VIX_t$  is the VIX daily closing value observed at time  $t$ .  $\widehat{EPU\ Index}_t$  is the EPU news index at time  $t$  instrumented via Equation 3 in our first stage.  $X_t$  is a vector of controls (description of the variables can be found in Table A.5 in Appendix).  $\gamma_t$  is a vector containing day of the week, monthly and yearly time dummies.  $\mu_t$  is the error term. Columns 1 and 2 show OLS and IV estimations for the baseline model. In line with the predictions, as the coverage of EPU by media channels increases, expected stock market volatility increases accordingly. In particular, referring to the IV calculation in Column 2, an increase of the EPU news index by one standard deviation leads to a positive variation of the VIX equal to 3.61 points on the same day. The effect is sizeable, as the average of VIX closing values over the considered time period equals 19.35 points. Columns 3 and 4 report results from the same specification; however, we also add quarterly federal government spending over GDP to control for the effects of greater public spending on volatility<sup>27</sup>. Indeed, according to Baker et al. (2016), as the government increases spending the stocks of those firms depending more on public expenditures appear less volatile on average. The negative sign of *Federal gov spending/GDP<sub>t</sub>* seems to confirm Baker et al. (2016) - although in our case only a more aggregate measure of expected volatility is used as dependent variable. We also notice that the coefficients for the media coverage of EPU remain stable in both columns. Following Pastor and Veronesi (2013) and Kelly et al. (2016), we add to Specifications 5 - 8 two different proxies to control for the conditions of the economic system. Indeed, as these authors make clear stock markets respond to the state of the economy: more volatile during recessions and crises, more stable otherwise. In line with these conclusions, both *GDP % change<sub>t</sub>* and *S&P 500 index return<sub>t</sub>* lower the VIX when they increase and signal better economic circumstances<sup>28</sup>.

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<sup>27</sup>Source for the data is the Bureau of Economic Analysis, US Department of Commerce ([www.bea.gov](http://www.bea.gov)). Descriptive statistics can be found in Tab.A.5 in Appendix.

<sup>28</sup>Source for GDP data is the Bureau of Economic Analysis, US Department of Commerce ([www.bea.gov](http://www.bea.gov)). Source for the data used to calculate the S&P 500 index returns is Yahoo

Furthermore, coefficients for the media coverage of EPU remain again stable, even though the IV estimation in Column 8 is not statistically significant anymore. Finally, Columns 9 and 10 reports estimations with all controls together. Overall, the coefficients of  $\widehat{EPU\ Index}_t$  appear stable in magnitude and significant, strengthening the robustness of the results.

It is important to stress that these results not only support a role of the media in diffusing EPU news to investors. They are also among the very few causal results on the effect of economic policy uncertainty on financial markets. Indeed, the fact that greater coverage of EPU is associated with higher levels of expected stock market volatility sounds as fresh evidence supporting the effects of policy uncertainty formalized by Pastor and Veronesi (2012, 2013) in their general equilibrium models.

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Finance ([www.finance.yahoo.com](http://www.finance.yahoo.com)). Descriptive statistics of both series can be found in TabA.5 in Appendix.

Tab.4

*Second stage regression results*

		<i>Dependent variable:</i>									
		VIX (daily)									
		OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
EPU News Index (daily)		0.025*** (0.001)	0.054* (0.032)	0.025*** (0.001)	0.053* (0.032)	0.020*** (0.001)	0.054* (0.030)	0.020*** (0.001)	0.051 (0.032)	0.020*** (0.001)	0.049* (0.029)
Federal Gov. Spending/GDP (quarterly)				-236.767*** (48.359)	-205.333*** (62.525)					-282.134*** (46.801)	-242.105*** (64.616)
GDP % change (quarterly)						-0.978*** (0.092)	-0.783*** (0.206)			-0.997*** (0.089)	-0.824*** (0.206)
S&P500 Index returns (daily)								-75.045*** (10.734)	-77.337*** (10.466)	-71.518*** (8.977)	-74.689*** (9.232)
Year, Month, Dow FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	6,801	6,801	6,801	6,801	6,801	6,801	6,801	6,801	6,801	6,801	6,801
R <sup>2</sup>	0.600	0.553	0.606	0.561	0.646	0.583	0.612	0.617	0.576	0.664	0.617
Adjusted R <sup>2</sup>	0.598	0.550	0.603	0.558	0.643	0.581	0.609	0.602	0.573	0.662	0.615

*Notes:* regression results of Equation 4. Dependent variable is the VIX daily closing values by the Chicago Board of Exchange. Time span for all models: 3<sup>rd</sup> of January, 1990 – 30<sup>th</sup> of December, 2016. Heteroskedasticity - autocorrelation robust standard errors in parentheses. \* p<0.1; \*\* p<0.05; \*\*\* p<0.01.

## 4. Robustness

Stability of the coefficients in the specifications of Table 4 in part reassures about the robustness of the results. According to our evidence, a variation in media coverage of EPU triggered by factors reasonably exogenous to the financial system (namely newsworthy sport events that are popular in the US) leads to an increase of the VIX, measure for the expected volatility of the US stock market. However, our instrument for the EPU news index is based on predictable sport events only. We make this choice in accordance with the results coming from our explorative regressions in Section 3. We also tested if major natural and technological disasters - events which are more unpredictable - crowded out EPU news but we could not find any significant effect. This fact raises a criticality. For example, Israeli et al. (2020) underline that managers seem to disclose different types of information strategically. Authors find that earnings announcements made on days with few concurrent announcements are significantly more likely to be losses than earnings announcements made on days with many concurrent announcements<sup>29</sup>. As well, Israeli et al. (2020) show evidence for more reported losses on days with predictable important sport events such as the March Madness: in fact, managers may try to “hide” bad earnings news on these days.

Given this evidence, we cannot rule out the possibility that economic agents might take strategic decisions when predictable important events are - or are not - around, and which in turn affect the VIX systematically. Thus, not (only) swings in media coverage of EPU - instrumented as in our case by popular sports - but instead other factors would be among the possible causes behind the increase in expected stock market volatility we documented in Section 3.

With the data in hand we are not able to distinguish between the effects caused by either media coverage of EPU or possible strategic

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<sup>29</sup>The fact that managers disclose fewer losses on days with a high number of earning announcements may initially seem counterintuitive. However, according to Israeli et al. (2020), these days are associated with more retail investor attention per unit of earnings news. They are, in other words, salient days for individual traders.

behaviours of market operators. However, to circumvent this issue we propose a placebo test with three lags and leads of the instrumented EPU news index to check if investors anticipate some predictable strategy we cannot control for. The intuition is the following: if the results obtained from our second stage regressions depend on operations whose timing rely on the schedule of the sport events in our instrument, (professional) investors are likely to anticipate them and react beforehand. Thus, leads of an increase in  $t$  of the instrumented EPU news index might be correlated with above average expected volatility. Results of this test are shown in Table 5 below.

Tab.5  
*Placebo test*

	<i>Dependent variable:</i>						
	VIX (daily)						
	IV regressions						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
EPU News Index (three lags)	0.061* (0.035)						
EPU News Index (two lags)		0.057* (0.033)					
EPU News Index (one lag)			0.051* (0.021)				
EPU News Index				0.049* (0.029)			
EPU News Index (one lead)					0.043 (0.028)		
EPU News Index (two leads)						0.044 (0.031)	
EPU News Index (three leads)							0.037 (0.031)
Year, Month, Dow FE	YES	YES	YES	YES	YES	YES	YES
Full set of controls	YES	YES	YES	YES	YES	YES	YES
Observations	6,798	6,799	6,800	6,801	6,800	6,799	6,798
R <sup>2</sup>	0.554	0.574	0.604	0.617	0.637	0.629	0.638
Adjusted R <sup>2</sup>	0.551	0.571	0.601	0.615	0.635	0.626	0.636

*Notes:* IV regression results. Dependent variables are VIX daily closing values by the Chicago Board of Exchange. Time span for all models: 3<sup>rd</sup> of January, 1990

– 30<sup>th</sup> of December, 2016. In line with the regressions in the second stage, the full set of controls contains: federal government spending over GDP (quarterly); S&P 500 index returns (daily); % GDP variations (quarterly). Heteroskedasticity - autocorrelation robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

In the first three columns we can notice the persistency over three days of a positive change in expected volatility brought about by an increase in EPU coverage. In fact, coefficients remain positive and fairly similar in their magnitudes throughout the considered specifications. This persistency is something to be expected as the VIX is a forward looking measure of volatility; this makes the results and our empirical approach more plausible. However, more important for us are the last three specifications: coefficients of the lead variables are in fact not statistically significant. Thus, no evidence of an anticipating behaviour of investors is found over the three days before an increase of EPU coverage as instrumented by newsworthy sport events. We interpret these results as a sign against the idea that strategic moves of some operators - and not swings in media coverage of EPU - drive the results in our second stage. This is further support for the validity of our identification strategy.

## 5. Conclusion

There is a widespread idea that the media influence financial markets in several ways. Yet, such claims are often based on anecdotal associations, as making causal inference in this regard is a difficult task. Focus of this paper was on the role of the media in spreading news around investors and for this scope, we put forward and tested a novel identification strategy. First of all, we diverted our attention on a precise type of news, namely news about EPU. If the media have a role in spreading genuine EPU signals among operators (as implied for example by the literature in behavioural finance on investors' limited attention), in line with Pastor and Veronesi (2013) a plausible hypothesis about media coverage of EPU can be tested, helping identification: the more EPU



coverage, the greater the probability it grabs the (limited) attention of (individual) investors; *ceteris paribus*, this should lead to an increase in volatility because agents are more likely to update their beliefs about future government's actions and expected returns of stocks. Secondly, to circumvent the usual endogeneity problem when using data based on media reporting, we took advantage of variations in media coverage brought about by newsworthy events. Indeed, both Eisensee and Stromberg (2007) and Durante and Zuhravskaya (2018) document the existence of a competition among publishable news. After an exploration of our data, the Olympics, the World Series and the NBA finals seem significant sources of news pressure able to crowd out stories about EPU. In fact, the daily news based EPU index by Baker et al. (2016) - implemented here to proxy for media coverage of EPU in the US - systematically lowers in those days of one of the mentioned sport events. As long as these sports are considered exogenous to the true level of political uncertainty in the system - and to other determinants of stock market volatility - they can serve as an instrument for variations in the media coverage of EPU. Results from several specifications of a second stage regression confirm the theoretical implications: as the media coverage of EPU increases, namely as the probability of (individual) investors receiving news about EPU increases, the VIX also increases. We interpret this as evidence for a change in expectations due to a greater spread of EPU news among investors. Therefore, in line with Peress (2014) and Engelberg and Parsons (2011) this paper finds evidence supporting a causal role of the media in the diffusion of news among investors. However, with its focus on EPU it adds on their outcomes by underling a new mechanism through which media outlets affect the markets. Moreover, their identification strategies present some limitations that in this paper we tried to solve with our novel procedure. Considering as in Peress (2014) national newspapers strikes a source of media coverage variation exogenous to the financial market is questionable. In fact, it sounds reasonable to think some of these events being related to the cycle of the economic system. News-worthy scheduled sports are, in this sense, arguably more reliable. As well, causal evidence in Engelberg and Parsons (2011) comes mainly

from an assumption which does not sound very robust nowadays, given the large spread of the Internet in more recent years: operators should respond mainly to business news coverage by local newspapers distributed in those cities in which they live in. Differently from the two authors, for the identification procedure proposed here we use data coming from both pre- and the Internet eras. Thus, in our opinion more general and conclusive results are obtained.

According to the revised literature about limited attention and information frictions, retail investors are more affected by such problems with respect to professional categories. It follows that the latter group should be less sensible to the activity of media outlets since it may rely on other sources of information more easily. An intriguing development of this research would be to find evidence about this difference among operators also when considering EPU coverage. Proving this dissimilarity would be further evidence in favour of the behavioural theories on investors' cognitive limitations and their implications for the role of the media in disseminating news to the financial markets.

## Appendix

Tab.A.1

*Daily Economic Policy Uncertainty Index - descriptive statistics*

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
EPU news index	10,332	101.160	68.279	3.320	53.850	130.572	719.070

*Notes:* descriptive statistics of the daily news based economic policy uncertainty index by Baker et al. (2016). Time span: 1<sup>st</sup> of January, 1990 – 15<sup>th</sup> of April, 2018.

Tab.A.2

*Disasters in the USA – descriptive statistics*

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Disaster (dummy = 1)	10,332	217 (2.1%)					
Tot. deaths	10,332	1.006	21.029	0	0	0	1,833

*Notes:* descriptive statistics of a dummy equal to 1 if either a natural or a technological disaster as defined in the International Disaster Database ([www.emdat.be](http://www.emdat.be)) occurs at time  $t$  in the US. *Tot. deaths* is the natural logarithm of the number of victims (+1) of the corresponding tragedy. Only those disasters of which the number of fatalities is above the 75<sup>o</sup> percentile in their categories are considered. Time span is the 1<sup>st</sup> of January, 1990 – 15<sup>th</sup> of April, 2018.

Tab.A.3

*Newsworthy sport events – descriptive statistics*

Statistic	N	N. of days
Olympics	10,332	258 (2.5%)
World Cup	10,332	217 (2.1%)
Euro Cup	10,332	165 (1.6%)
World Series	10,332	196 (1.9%)
March Madness	10,332	589 (5.7%)
NBA Finals	10,332	340 (3.3%)

*Notes:* descriptive statistics of dummies for newsworthy sport events in the USA. Time span is the 1<sup>st</sup> of January, 1990 – 15<sup>th</sup> of April, 2018.

Tab.A.4  
*VIX – descriptive statistics*

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
VIX	7,124	19.354	7.857	9.140	13.660	22.780	80.860

*Notes:* descriptive statistics of VIX closing values by the Chicago Board of Exchange. Time span: 2<sup>nd</sup> of January, 1990 – 13<sup>th</sup> of April, 2018.

Tab.A.5  
*Second stage control variables – descriptive statistics*

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
S&P500 index return (daily)	6,804	0.0003	0.011	−0.090	−0.005	0.006	0.116
Federal spending as % of GDP (quarterly)	6,805	0.181	0.012	0.154	0.171	0.189	0.204
GDP % variation (quarterly)	6,805	2.484	2.448	−8.4	1.4	4	7.5

*Notes:* descriptive statistics for the control variables used in the second stage regressions. Source for the data about the federal spending is the Bureau of Economic Analysis, US Department of Commerce ([www.bea.gov](http://www.bea.gov)). Source for GDP data is the Bureau of Economic Analysis, US Department of Commerce ([www.bea.gov](http://www.bea.gov)). Source for the data used to calculate the S&P 500 index returns is Yahoo Finance ([www.finance.yahoo.com](http://www.finance.yahoo.com)). Time span: 2<sup>nd</sup> of January, 1990 - 30<sup>th</sup> of December, 2016.

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# Law Enforcement Efficiency and Generalized Trust

## Abstract

In this paper we ask if law enforcement efficiency might have an impact on generalized trust. To answer this question we propose a novel empirical strategy in which we first document the existence of a negative relationship between the performance of Italian tribunals and lawyer density. Next, we report a connection between this density and the number of law degree holders in a given territory. This allows us to exploit as an instrument the systematic changes in the distribution of law graduates over the Italian peninsula brought about by the creation of mass universities in 1969 due to the Codignola reform. We collect data about the number of law degrees issued by all Italian public law schools from 1958 to 2014 to keep track of the effects of the reform over time and across faculties. In turn, this allows us to understand in which areas of the country we should observe underperforming tribunals in the early 2000s. As measures of trust, we then employ an individual level proxy based on the 2001, 2008 and 2013 ITANES (Italian National Election Studies) surveys as well as the number of nonprofit associations per inhabitant at a municipal level in 2001. According to the estimates coming from our main reduced form equations, those citizens living in areas with a less efficient enforcement seem on average less trusting.

**Keywords:** trust, law enforcement efficiency, culture, social capital



## 1. Introduction

Several authors show that higher levels of trust among individuals have positive externalities for the functioning of the economy. More precisely, according to the literature, diffuse generalized trust, namely a belief in the reliability, truth, or ability of other members of society (Carl and Billari, 2014), should foster collaborative solutions among agents in many real world situations in which coordination problems arise (e.g. Bigoni et al., 2015; Bigoni et al., 2018; Guiso et al., 2004; Guiso et al., 2009; Tabellini, 2010, Nannicini et al., 2013; Gorodnichenko and Roland, 2017). It is important to stress that generalized trust has to be distinguished from a more “limited” one, which it is instead applicable to a narrow circle of friends or relatives (Tabellini, 2008)<sup>1</sup>. Emblematic about the role of generalized trust are the results in Guiso et al. (2004) for the case of financial transactions, the ultimate trust-intensive contracts. In fact, financing is nothing but an exchange of a sum of money today for a promise to return more money in the future. Whether such an exchange can take place also depends on the extent to which the financier trusts the financee (Guiso et al., 2004). *Ceteris paribus*, authors show that in areas with, on average, higher level of interpersonal trust, households are more likely to use checks, invest less in cash and more in stocks, have higher access to institutional credit, and make less use of informal credit.

Tabellini (2008) describes a theoretical framework in which cultural norms of high trust and law enforcement efficiency - defined as the probability that a crime goes undetected - interact over time. In a model with imperfect monitoring and private information, the scholar describes possible complementarities between enforcement effectiveness and pro-social behaviours in prisoner’s dilemma type of games. Indeed, by making endogenous the share of trustworthy and not-trustworthy individuals of an economy, Tabellini (2008) shows that the fraction of trustworthy players in the population is an increasing function of the efficiency of law enforcement due to greater incentives to collaborate.

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<sup>1</sup>This form of trust is likely to be less influenced by the legal enforcement and its performance, object of this study.

Moreover, agents are also more willing to transmit values of higher interpersonal trust to their offspring: this can potentially lead norms of greater cooperation to flourish over time in a society. According to another stream of research, trust can be seen as emerging from heuristics or “rules-of-thumb” used to interact with others in complex environments (Durante, 2010; Nunn and Watchenkon, 2011; Nunn, 2012, 2014). These rules-of-thumb embody, among other things, relevant characteristics of the environment in which they are used (Binmore and Samuelson, 1994). For example, according to Nunn and Watchenkon (2011) poor formal institutions permit poor behaviour, which engenders mistrust. Authors show that trust levels in Africa depend not only on interiorized cultural factors of respondents but also on proxies for the quality of the institutional environment in which they currently live (e.g. how many locally elected councillors were found corrupt; whether local council members listen to citizens’ concerns).

In the light of these results we want to test if law enforcement efficiency might be one of the drivers of trust. In particular, we attempt to find empirical evidence supporting a positive impact. To do this, we collect information about individual level trust coming from three waves of a periodic survey held in 2001, 2008, 2013 by ITANES (Italian National Election Studies). Moreover, as a further indicator for the level of cooperation we follow Putnam (2000) and we implement the number of nonprofit associations per individual - in 2001 - in each Italian municipality. These data come from ISTAT (Italian National Institute of Statistics). However, problems of endogeneity may occur when answering econometrically to our inquiry. Indeed, unobservable variables proper of a territory could drive both the existing level of generalized trust and the performance of legal enforcement, leading us to obtain inconsistent estimates. Moreover, it sounds plausible to think the existence of feedback effects in this case, with the functioning of monitoring being improved by higher level of trust as well. We try to overcome these problems by proceeding as follows. We first notice that law enforcement efficiency in Italy seems to be related with the density of operating lawyers (Buonanno and Galizzi, 2014; Carmigniani and Giacomelli, 2010). Main reason appears to be a higher litigation rate

driven by an asymmetric information advantage of attorneys on clients as well as strong rigidities to compete on prices; in fact, minimum fees for legal services were in place until recent years (Finocchiaro Castro and Guccio, 2014; Buonanno and Galizzi, 2014)<sup>2</sup>. High levels of litigiousness lead to clog up courts and increase the length of proceedings, mining their effectiveness (Carmigniani and Giacomelli, 2010). Thus, we look for a source of variations in the density of lawyers over the Italian territory to instrument swings in law enforcement efficiency as well. According to Buonanno and Galizzi (2014), Giacomelli and Carmigniani (2010) and Mora-Sanguinetti et al. (2015), density of legal practitioners is an increasing function of law graduates in a given area<sup>3</sup>. Then, similarly to these authors we argue that in municipalities closer to a law school the density of operating lawyers should be on average higher. However, we also show evidence supporting the claim that this density should be systematically higher in proximity of law schools active since the promulgation of the so called Codignola reform in 1969. Indeed, the Italian government lowered the entry barriers to access any type of public university, including law. Using data about the yearly number of degrees issued by all Italian public law schools over 56 years, from 1958 to 2014, we document an increase in these figures right after the new legislation<sup>4</sup> <sup>5</sup>. Moreover, such an

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<sup>2</sup>Over the time span considered by our research, the existence of such minimum compulsory fees for legal services has been challenged only starting from 2006, by the decree 223 (the so-called Bersani decree) aimed at their elimination. Although the national council of lawyers opposed the decree and tried to limit its applicability, the decree came into effect in 2008. Notice that we also consider the ITANES survey of 2013 as a source of individual level data on trust. However, we do not think the Bersani decree alone could have eliminated the relation between lawyer density and efficiency of Italian tribunals (at least, not already by 2013), key for our identification strategy. On this, results from Models 5 and 6 in Table A.13 in Appendix seem to confirm our supposition: yet in 2012, OLS coefficients from regressing civil trial length - a measure for law enforcement efficiency in accordance with Guiso et al. (2004) - on a proxy for lawyer density are positive, significant and similar in magnitude to those obtained by equivalent models for the years 2001 and 2008.

<sup>3</sup>To access the legal profession in Italy a degree in law is compulsory.

<sup>4</sup>Data have been manually collected from bulletins reporting annual statistics of the Italian schooling system (*Annuario statistico dell'istruzione*), which can be found in ISTAT library in Rome. For years from 2007 to 2014, data are downloadable from MIUR (Italian Ministry of University Education and Research) website.

<sup>5</sup>It is important to point out that the Italian university system is mostly public. In the academic year 2014/15 for example, only 9.3% of the students was enrolled in private institutions

increase characterized every faculty already established at that time. We also show how a proxy for the density of law degree holders - right before the years of our dependent variables for trust - is on average higher for provinces with a law school in activity since 1969. We claim that this fact might be due to the push to the number of law degrees brought about by the reform since its promulgation. Results from several placebo tests further support this idea. Then, we run a battery of balance tests to check if an instrument for law graduate density - and in turn lawyer density - based on the distance of a municipality from its closest law school active at the time of the Codignola reform is correlated with other socioeconomic characteristics. The results indicate that our instrument seem fairly exogenous. However, we are cautious about this point and in what follows we underline some limitations of our empirical approach. We then report evidence which supports the connection between the instrument and changes in lawyer density.

We obtain our main results by estimating several specifications of two reduced form equations - one for the individual level generalized trust and the other for the nonprofit association density - with the indicator variable described above intended to capture variations in lawyer density across Italian municipalities in the early 2000s. According to our estimations, in those areas with a higher density of attorneys people seem characterized by lower levels of interpersonal trust: the probability of answering that most people can be trusted in one of the three ITANES surveys decreases by around 8%; when considering the sample of municipalities of these respondents, nonprofit associations drop by 1 unit per 1000 inhabitants. We believe that the channel in action might be a lower efficiency of law enforcement in proximity of cities with more lawyers (which in turn leads to more misbehaviour of agents and then, on average, less trust). We provide insights in support of this statement.

Our empirical results contribute to the debate of how trust is affected by the external environment, law enforcement efficiency in particular.

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(source: *Studenti e bacini universitari*, ISTAT, 2016). Thus, it seems reasonable to believe that reforms of the public university system in this country could have large impacts on the structure of the graduated population.

They are supportive of theoretical results of Tabellini (2008) in which increases in monitoring effectiveness rise incentives to be more trustworthy and to transmit values of cooperativeness. Our evidence also extends the results by Nunn and Watchenkon (2011) about negative effects on trust of both internal factors associable with a culture of low cooperation and poorly functioning formal institutions. By focusing on law enforcement efficiency, we are able to shed further light on the last channel. In particular, our evidence speaks in favour of complementarities between environments that sustain more cooperation and believes engendering higher levels of generalized trust.

Furthermore, we contribute to the research on the determinants of law enforcement efficiency in Italy. In particular, for the years between 1995 and 2012 we offer fresh evidence on the link between density of lawyers and the performance of tribunals put forward by previous literature: our estimations seem to support the thesis of an opportunistic behaviour of legal practitioners whose the incentive is to inflate their emoluments by more judicial activity (e.g. Finocchiaro Castro and Guccio, 2014; Buonanno and Galizzi, 2014; Giacomelli and Carmignani, 2010). This inflated demand for justice not explainable by fundamentals translates into a greater workload for tribunals, increases in the time needed for the resolution of disputes and thus, in less effective monitoring (Finocchiaro Castro and Guccio, 2014). Moreover, in line with this stream of research we make use for identification of the link between density of law degree holders and attorneys. However, differently from before our collected longitudinal data on law degrees issued by the Italian public universities allow us to explicitly test this claim. As well, this novel panel set permits us to monitor the effects of the aforementioned Codignola reform on graduates in law and gain some evidence of its repercussions also for attorneys. Therefore, we think our procedure to indentify variations in the distribution of lawyers (through swings in law graduates) being more reliable with respect to previous works on the topic.

In the reminder of the article we proceed as follows. In Section 2 we describe insights from previous literature supporting the research hypothesis. In Section 3 we present our data about generalized trust

and law enforcement efficiency in Italy. In Section 4 we discuss the empirical strategy and we report evidence in favour of its usage as well as its main limitations. We devote Section 5 for our main results coming from a battery of reduced form equations and we report evidence for the channel in action in Section 6. In Section 7 we further test our estimations with two different robustness exercises intended to check for other possible explanations behind the results. Section 8 concludes.

## 2. Related literature

In this paper we show evidence in accordance with a positive relation between law enforcement efficiency and generalized trust. To do so, we exploit long lasting consequences on monitoring effectiveness of an arguably exogenous reform of the Italian university system, promulgated in 1969. But what might be the explanation behind our results? And why we can think of a culture of high interpersonal trust as not being substitutable by formal laws and their enforcement?

Our evidence could connect to theoretical results in Tabellini (2008) which shows how law enforcement effectiveness and pro-social behaviour might interact over time. In particular, Tabellini (2008) describes possible complementarities between cultural norms of high trust and the quality of domestic institutions. In an economy which lasts forever, agents take extra psychological utility from collaborating with others in prisoner's dilemma type of games<sup>6</sup>. However, some subjects prefer to collaborate more (trustworthy agents) whilst other individuals prefer to collaborate less (not-trustworthy agents). By making the share of trustworthy and not-trustworthy individuals endogenous, according to Tabellini (2008) the fraction of good players in the population is an increasing function of the efficiency of law enforcement, defined as the probability of detecting players who choose to defect, especially

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<sup>6</sup>This formulation wants to capture the idea that individuals might be motivated by more than just material payoffs. They could also value the act of cooperating per se. These “warm glow preferences” could reflect religious or moral principles or other values that induce individual self-regulation in social interactions (Tabellini, 2008).

in case of more “distant” games (in the spirit of the model, not those prisoner’s dilemma games played with more “similar” individuals such as family, friends, etc., for which monitoring effectiveness is considered a less important driver influencing the final outcome). Moreover, in the model a shock to legal enforcement effectiveness at time  $t$  not only immediately changes the incentives for agents to be trustworthy or not, but it also propagates over time to the next generations since agents rationally choose - by sustaining a cost - what values to transmit to their offspring by looking at the pattern of the efficiency of law enforcement and of likely future transactions. However, Tabellini (2008) assumes “imperfect empathy” which implies that the equilibrium is both forward- and backward-looking. It is backward-looking because values of the parents influence their educational choices. Thus, values evolve gradually over time and during the transition they reflect historical features of the external environment. But the equilibrium is also forward looking, because parents adapt their educational choices to the future environment of their children<sup>7</sup>.

As we make clear in what follows, we suppose that the Codignola reform of the Italian university system had indirect but persistent negative effects on law enforcement efficiency of some territories (observable around those years covered by our dependent variables of generalized trust). Therefore, in accordance with the revised model by Tabellini (2008), for these areas we should witness systematic lower levels of trust due to the incorporation of information about poorer monitoring performances. Our empirical evidence seems in line with these conclusions.

According to another stream of research, a further possible explanation for our results could be connected to the fact that in their daily life, people need some heuristics or “rules-of-thumb” to handle complex decisions and save time (Kanheman et al., 1982). More or less trust can then be considered as emerging from different heuristics used

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<sup>7</sup>In the model, if the quality of legal enforcement is chosen under majority rule, there is also path dependence: adverse initial conditions may lead to a unique equilibrium where legal enforcement remains weak and individual values discourage cooperation (Tabellini, 2008). Thus, the model describes a self-enforcing mechanism by which low levels of trust and weak institutions persist among future generations.

to interact with others (Nunn, 2014). Indeed, formal institutions such as contracts - together with their enforcement - seem not sufficient in many cases to allow people to get rid of the need of such “rules-of-thumb” because of technological constraints. On the contrary, it sounds plausible to postulate the existence of some complementarities between the quality of formal institutions and the emerging of informal rules sustaining different levels of generalized trust (Durante, 2010; Guiso et al., 2004; Putnam, 1993).

The issue can be summarized in game theoretic terms. When agents interact among one another, they can be thought as playing games in which there are rules, strategies, payoffs and preferences over these payoffs. In some cases, the structure of the game makes the interests of each wealth maximizing individual and society (namely the players as a whole) overlapping; cooperation among players is “naturally” sustained because there are incentives to do so. Examples are when the game is repeated, when players possess complete information about others’ past behaviours (namely there is reputation), and when there are small numbers of players. Instead, cooperation is difficult to sustain in many other real world situations: when the game is not repeated (or there is an endgame), when information about the other players is lacking, and when there are large numbers of players (North, 1991). In such situations, a defect by other players is a real threat and cooperative solutions do not emerge. To free up the potential gains from trade, human societies put in place formal (and informal) devices such as constitutions, laws, contracts as well as mechanisms to enforce them (North, 1991)<sup>8</sup>. However, in the real world these mechanisms are often not sufficient to always and fully protect agents by defecting problems. Indeed, many economically relevant exchanges are not object of explicit laws or contracts. As well, contracts are often incomplete, especially

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<sup>8</sup>In particular, institutions can be thought as humanly devised constraints that structure political, economic and social interaction. They consist of both informal constraints - sanctions, taboos, customs, traditions, and codes of conduct - and formal rules - constitutions, laws, property rights (North, 1991). However, in this section we are interested in showing why interpersonal trust per se might be an important element for an economy. Thus, we focus on (the limitations of) its main substitutes in solving coordination problems: formal institutions. Indeed, they are the only ones protected by a law enforcement system (police, etc.). In other words, they are the only ones guaranteed by a “credible” control system.



employment contracts, as it is very hard to predict all possible caveats and duties that can arise during a collaboration. Moreover, also in the case of exchanges which are fully regulated by formal rules, monitoring is imperfect. Therefore, again we would need to understand, by investing a certain amount of resources, if law enforcement is a credible threat which leads all agents towards collaborative solutions. In fact, an inefficient law enforcement, namely a system not able to detect and convict criminals with sufficiently high probability, opens up rooms for strategic uncooperative behaviours: expected losses might be too low to discourage egoistic decisions (Tabellini, 2008). To sum up, the simple fact that there exists monitoring is not sufficient to dissipate all risks when an exchange with third parties takes place. Then, in line with our game theory framework, in virtually all economic relevant situations (both protected and unprotected by law), we would need to collect and process some information to understand if cooperation is the optimal strategy. For individuals, applying this rationale to understand always the right thing to do is costly, both in terms of resources and time. And in our highly connected societies, prisoner's dilemma type of games, for example, happen several times, on a daily base, with different levels of complexity. Since we are finitely lived agents, with a finite amount of resources, it could be that looking for the optimal strategy (in game theoretic terms) is not always the best (feasible) option among all possible actions (Simon, 1972). Learning is one of the mechanisms we have to not perform every time such calculations: once we discover the optimal behaviour in one situation, we should be able to apply it also to other (enough) similar cases (Camerer et al., 2003). However, in our daily lives it is common to face novel scenarios<sup>9</sup>; thus, we would be often back to the need of calculating the best thing to do. And this task can be computationally demanding, especially in games

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<sup>9</sup>Nowadays, this observation seems particularly true. In our “technology driven” societies, innovations continuously change the actual way of interacting with others. Moreover, they create novel forms of interaction. In this sense, notable examples are the recent radical disruptions brought about by social medias.

with information failures<sup>10 11</sup>

As human beings, we are both resource and time constrained; thus, it is likely the case we need sometimes “efficient” approaches to go through the high number of novel and potentially complex exchanges we face over time (both protected and unprotected by law). Something “fast-and-frugal”, some heuristics that allow us to save resources, and they are applicable to several contexts (Gigerenzer and Gaissmaier, 2011)<sup>12</sup>. In line with the literature on the topic, we can think interpersonal trust as resulting from heuristics or “rules-of-thumb” used to interact with others (Durante, 2010; Nunn and Watchenkon, 2011; Nunn, 2012, 2014). An heuristic can be defined as a strategy that ignores part of the information, with the goal of making decisions more quickly, frugally, and/or accurately than more complex methods (Gigerenzer and

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<sup>10</sup>In a game with “imperfect” information, players are unaware of the actions chosen by other players. However they know who the other players are, what their possible strategies/actions are, and the preferences/payoffs of these other players. Examples are card games where each player’s cards are hidden from other players but objectives are known as in poker, bridge, etc. A further step of complexity are games of “incomplete” information. These are games where the players do not have common knowledge of the game being played. This idea is important in capturing many economic situations, where a variety of features of the environment may not be commonly known. Among the aspects of the game that the players might not have common knowledge of, there are: payoffs; who the other players are; what moves are possible; what opponent knows, and what he knows we know, etc. (Levin, 2002).

<sup>11</sup>Artificial intelligence literature shows that the task of finding an optimal solution to  $n$ -player imperfect information games is NP-hard (nondeterministic polynomial-time hardness) for any value of  $n$ . It follows that no time efficient algorithm exists for optimally solving imperfect information games in general (Blair et al., 1993). In summary, according to author’s results, an heuristic approach will be necessary to solve in a reasonable amount of time such games. A problem is assigned to the NP (nondeterministic polynomial-time) class if it is solvable in polynomial time by a nondeterministic simple abstract computational device called Turing machine. This device is intended to investigate the extent and limitations of what can be computed by algorithms. NP-hard problems are at least as hard as the hardest problems in NP (source: [www.mathworld.wolfram.com/NP-Problem.html](http://www.mathworld.wolfram.com/NP-Problem.html), [www.mathworld.wolfram.com/NP-HardProblem.html](http://www.mathworld.wolfram.com/NP-HardProblem.html); Weisstein, Eric W., "NP-Problem" And "NP-Hard Problem", Wolfram Web Resource).

<sup>12</sup>It is well-known from laboratory experiments that, on average, agents make use of heuristics to solve computational problems (see for example the seminal paper by Kahneman et al., 1982). Indeed, in line with Kahneman (2003; pag.1469), <<intuition and reasoning are alternative ways to solve problems, that intuition resembles perception, that people sometimes answer a difficult question by answering an easier one instead, that the processing of information is often superficial, that categories are represented by prototypes>>.

Gaissmaier, 2011)<sup>13</sup> <sup>14</sup>. Related to this, culture can be described as a set of long lasting decision-making heuristics, which typically manifest themselves as values, beliefs, or social norms employed in uncertain and complex environments (Nunn and Watchenkon, 2011). This view builds on insights from evolutionary anthropology about the micro-foundations behind the emergence of culture, invoked by Nunn (2012, 2014) for the case of generalized trust. Boyd and Richerson (1985, 1995, 2005) show that if information acquisition is either costly or imperfect, it can be optimal for individuals to develop simple heuristics

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<sup>13</sup>These observations are based on the classical “accuracy-effort” trade-off: people save effort with heuristics, but at the cost of accuracy. In this view, humans and other animals rely on heuristics because information search and computation cost time and effort; heuristics trade-off some loss in accuracy for faster and more frugal cognition (Shah and Oppenheimer, 2008). However, in some situations – e.g. when part of the relevant information is unknown or it has to be estimated from small samples – heuristics can lead to more accurate inferences than strategies that use more information and computation. Even when information and computation are entirely free, there is typically a point where less is more. Gigerenzer and Gaissmaier (2011) reports several real world situations in which very simple heuristics (“hiatus heuristic”, “ $1/N$  rule” in investment choice, etc.) outperform, according to several metrics, sophisticated algorithms which use more data for their calculations. To similar conclusions also Dosi et al. (2017) in which agents have to cope with a complex evolving economy characterized by deep uncertainty resulting from technical changes, imperfect information and coordination hurdles. In these circumstances, they find that neither individual nor macroeconomic dynamics improve when agents replace myopic expectations with less naive learning rules. In fact, more sophisticated expectations (e.g. expectations based on recursive least squares) produce less accurate individual forecasts and also considerably worsen the performance of the economy. According to the authors, results suggest that fast and frugal robust heuristics are not a second-best option: rather they are “rational” in macroeconomic environments with heterogeneous, interacting agents and changing “fundamentals” (Dosi et al., 2017). For the aim of our work, embracing either the “accuracy-effort” trade-off or the “less-is-more” view (or both) to justify the usage of heuristics by agents does not make any big conceptual difference. The important take out point here is the following: in both cases, for resource constrained agents it can be overall optimal to take decisions in complex environments by developing simple rules-of-thumb, which are based on partial information sets. In accordance with the revised literature (e.g. Nunn, 2012, 2014), we consider trusting others as emerging from these rules, able to quickly guide us through many different (and novel) bargaining situations.

<sup>14</sup>Lacetera et al. (2012) show that also in reasonably competitive, high-value durable-goods market, agents use simple heuristics (e.g. “sold right before a 10000-mile threshold”). Analyzing over 22 million wholesale used-car transactions, authors find evidence of left-digit bias in the processing of odometer values, whereby individuals focus on the number’s leftmost digits. The bias leads to discontinuous drops in sale prices at 10000-mile odometer thresholds, along with smaller drops at 1,000-mile thresholds. These findings reveal that information-processing heuristics matter even in markets with large stakes and easily observed information (Lacetera et al., 2012).

or “rules-of-thumb” in decision making. By relying on general beliefs about the “right” thing to do in different situations, individuals may not behave in a manner that is optimal in every instance, but they save on the costs of obtaining the information necessary to always behave optimally (Nunn, 2014). Experience equips people with heuristics that they use to settle coordination problems that arise in real-life bargaining situations. These rules-of-thumb embody, among other things, salient or focal features of the environment in which they are used (Binmore and Samuelson, 1994). In fact, these rules do not develop in a vacuum but rather evolve according to which yield the highest payoff (Nunn and Watchenkon, 2011).

We believe that, in line with these observations, the capacity a system demonstrates to punish defections in everyday life situations is a relevant variable for the choice, by individuals, of heuristics reflecting higher levels of trust (especially towards neither family nor friends). According to Tabellini (2008), a more efficient law enforcement raises the costs of uncooperative behaviours in all exchanges guaranteed by law. For the average individual, this means a larger number of “games” in which cooperation is optimal. Thus, it sounds reasonable to think that he/she is going to develop heuristics of higher levels of generalized trust. Indeed, the risk of a loss by choosing to cooperate without performing a “costly” optimal calculation, is now lower. Our empirical results could represent fresh evidence that speaks in favour of such mechanism.

Values, beliefs and social norms that favour adaptability to the external environment will become more prevalent in the population because embraced over time by most individuals. For example, in situations in which large-scale cooperation increases fitness, norms that facilitate fruitful interaction (such as norms of higher mutual trust) will be particularly valuable and will become the prevalent cultural traits (Durante, 2010). Thus, by focusing on the relationship between justice efficiency and the level of generalized trust, we also try to offer some empirical evidence on a broader topic: the way formal institutions and culture interact and, in particular, how cultural traits are shaped. In many occasions, researchers have underlined the economic

relevance of a culture of generalized trust<sup>15</sup>. However, empirical research on what precisely influences cultural traits and how they both emerge and change over time is still needed. On this, some causal insights are offered by Nunn and Wantchekon (2011) about how the slave trade caused a culture of mistrust to develop within Africa. Nunn and Wantchekon(2011) shows that current differences in trust levels within some regions of Africa can be traced back to the transatlantic and Indian Ocean slave trades. Combining contemporary individual-level survey data with historical data on slave shipments by ethnic group, the researchers find that individuals whose ancestors were heavily raided during the slave trade are less trusting today. Initially, slaves were captured primarily through state organized raids and warfare, but as the trade progressed, the environment of ubiquitous insecurity caused individuals to turn on others (including friends and family members) and to kidnap, trick, and sell each other into slavery (Nunn and Wantchekon, 2011). Scholars hypothesize that in this unsecure environment, a culture of mistrust may have started and evolved, in line with the aforementioned evolutionary anthropology models put forward by Boyd and Richerson (1985, 1995, 2005). In a similar fashion, in our setting a lower level of enforcement efficiency could have led to more misbehaviour in some territories, and then to the emerging of cultural traits based on more mistrust.

By focusing on movers, Nunn and Wantchekon (2011) disentangle if trust is influenced by cognitive biases, external stimuli or both: when individuals relocate, their cultural beliefs, norms, and values move with them, but their external environment is left behind. Therefore, if the slave trade primarily affects trust through internal factors, then mistrust should be most strongly correlated with the extent to which individuals' ancestors were affected by the slave trade. On the other

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<sup>15</sup>Using European level data, Tabellini (2010) shows that trust is associated to current economic development. To isolate the exogenous variation in trust, the author relies on two historical variables used as instruments: the literacy rate at the end of the 19th century, and the political institutions in place over the past several centuries. The political and social history of Europe provides a rich source of variation in these two variables at a regional level. The exogenous component of trust due to history is strongly correlated with current regional economic development, after controlling for contemporaneous education, urbanization rates around 1850, and other national level effects.

hand, if the slave trade affects trust primarily through external factors - like the deterioration of domestic institutions which may lead to a decline in the trustworthiness of others due to more misbehaviour - then mistrust should be most strongly correlated with the slave trade's impact on the environment in which the individual lives today (Nunn and Wantchekon, 2011). According to authors' results, both factors are important to explain the difference in trust level among movers in the sample. However, it is not clear what institutional elements of the new environment where movers relocated drive the results. We connect to this and we try to shed further light by focusing on law enforcement and its performance. In particular, our empirical results speak in favour of law enforcement efficiency as one of the important elements of the environment which could affect trust. In Gorodnichenko and Roland (2017), the key conduit of economic growth and productivity enhancements is technological innovation. In line with this, they argue that an individualistic culture, with its emphasis on personal freedom and achievement (thus awarding social status to personal accomplishments such as important innovations) plays a key role in stimulating innovations and, hence, in explaining long-run economic growth. Related to our inquire, they also examine the interaction between individualism and institutional features such as the average protection against expropriation risk: scholars cannot exclude a two-way interaction, with individualistic cultural traits affecting institutions and institutions affecting these traits as well. Similarly, also our evidence speaks in favour of a relation between cultural traits and the institutional setting. In particular, we document the existence of complementarities between generalized trust and law enforcement, with efficiency of the latter sustaining more trust.

### **3. Data**

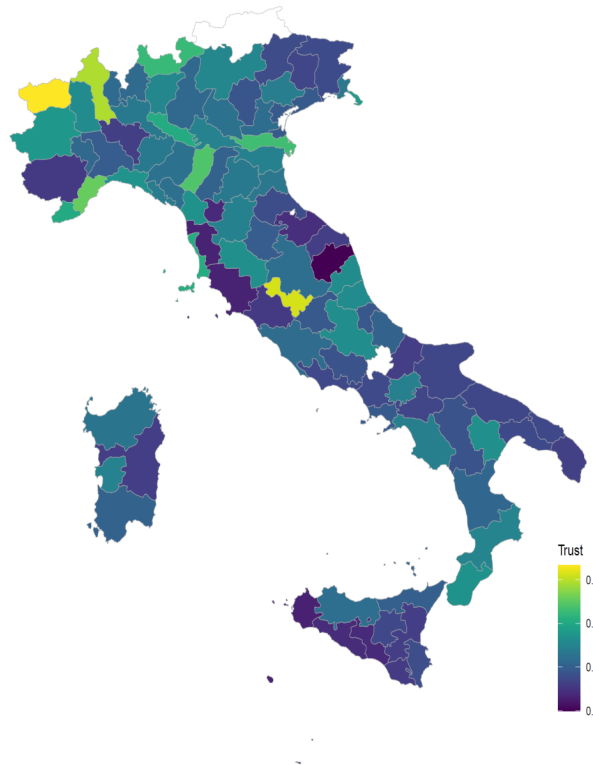
#### **3.1. Measures of generalized trust**

In line with the revised literature, generalized trust is considered an im-

portant determinant of many economically relevant transactions (e.g. Guiso et al., 2004; Nunn, 2012). Discovering if it is affected by changes in monitoring effectiveness is the object of our research. For this scope, we collected individual level data coming from three waves of a periodic survey held in 2001, 2008, 2013 by ITANES, which map answers to the question “generally, do you think most people can be trusted or that you cannot be too careful in dealing with people?” into a binary variable. This variable takes a value of 1 if the respondent says that most people can be trusted, 0 otherwise. Pooling the three surveys together gives us a total of 6971 usable observations. Figure 1 shows the distribution of our dependent variable across Italian provinces: in line with previous results (e.g. Putnam, 1993; Guiso et al., 2004; Bigoni et al., 2015), higher level of generalized trust can be found in the North-Central area of the peninsula, reassuring about the plausibility of our measure. Indeed, several papers have shown systematic social capital differences between Southern and North-Central Italian regions which can trace back to institutional dissimilarities between the two areas of the country during the Middle Ages (Putnam, 1993; Guiso et al., 2004): inclusive *repubbliche* (republics) and *signorie* (lordships) in the North and Centre, in which more social capital flourished; extractive and authoritarian monarchies in the South. In our sample, 23.03% of the respondents answered that most people can be trusted. For every respondent we also collected information on age and gender, together with information about study level and employment status. Similarly to Nunn and Wantchekon (2011), many of the controls are intended to proxy for individual income, which has been shown to be correlated with trust. Although we do not have a direct measure of income, occupation and education level can be considered good proxies (Nunn and Wantchekon, 2011). Descriptive statistics of the collected variables can be found in Table A.1 in Appendix.

Fig.1

*Trust in the Italian provinces*



*Notes:* average generalized trust by province calculated using 2001, 2008 and 2013 ITANES (Italian National Election Studies) individual level survey data. For each individual, the variable takes a value of 1 if the respondent says that most people can be trusted, 0 otherwise. Provinces in white colour are missing from the pooled sample.

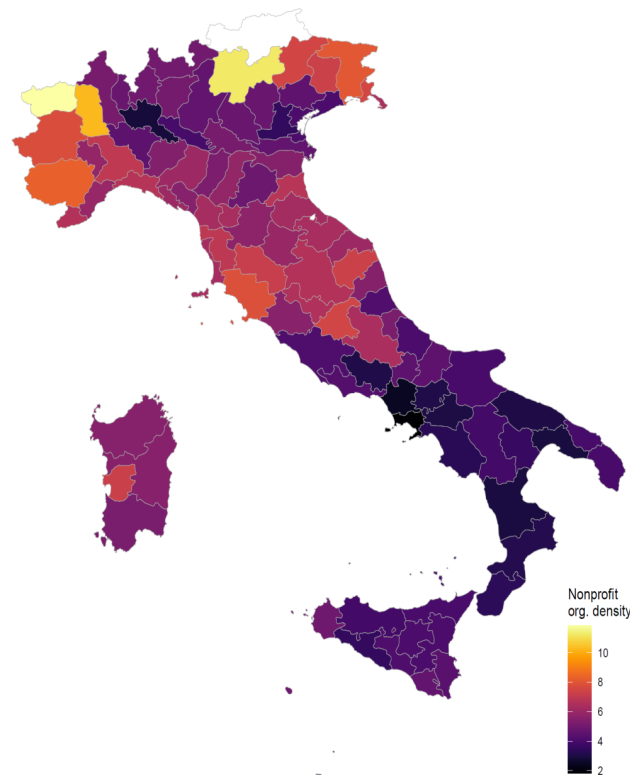
Survey data may suffer of measurement errors. For this reason, as a further proxy for generalized trust in a given municipality we also implement the number of nonprofit associations per individual of each Italian city; data source for our calculations is ISTAT. We consider 2001 as the reference year in order to make these observations time comparable with individual level data coming from the three ITANES surveys. According to Putnam (2000), nonprofit association density can be considered as one of the indicators measuring social capital in



a territory: it is a proxy for volunteerism level in a community and in turn, for the strength of the bonds between fellow citizens. Thus, in line with the literature, it should also connect to the trust level of the area (Putnam, 2000; Guiso et al., 2004). In our sample we have a total of 7313 data points, counting an average of 5 nonprofit associations every 1000 people. Figure 2 below reports calculations for each Italian province. Similarly to our individual level data about generalized trust, also in this case the difference between North-Central and Southern territories emerges. This fact makes us more confident that both the two proxies implemented in this study might be informative about the same underlying phenomena generating trust. Further details about the data can be found in Table A.3 in Appendix.

Fig.2

*Density of nonprofit associations in the Italian provinces*



*Notes:* number of nonprofit associations per 1000 inhabitants at a provincial level,

in 2001. Data source for calculations is ISTAT (Italian National Institute of Statistics). The province in white colour is missing from the sample.

### 3.2. Measure of law enforcement efficiency

The force under investigation which in our opinion might affect trust is law enforcement efficiency. Tabellini (2008) relates it to the probability a crime is not detected. Since this probability is not directly observable, the literature has put forward alternative methods to proxy for it. For the purpose of this paper, similarly to Guiso et al. (2004) and Tabellini (2010) we choose the average number of days it takes to complete a first degree civil trial in each Italian *distretto di corte d'appello* (Italian district courts)<sup>16</sup>. In particular, these data are intended to proxy for the performance of the tribunals in each *distretto* of the Italian territory<sup>17</sup>. We collected data from 1995 to 2012 in order to picture the behaviour of law enforcement efficiency over a time span that precedes and comprehends the years of our two measures of generalized trust. However, to the best of our knowledge average civil trial durations are not readily available for the case of Italy when considering less recent years. To overcome this problem, for every *distretto* we collected data on the number of pending (*pendenti*), definite (*definiti*) and appeared (*sopravvenuti*) trials at the end of each year, in order to apply the so called “disposition time formula” defined by the CEPEJ (European Commission for the Efficiency of Justice)<sup>18</sup><sup>19</sup>. Indeed, this formula aims to give a reliable approximation of the

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<sup>16</sup>The Italian territory is divided into 26 districts called *distretti di corte d'appello*. Each of them is competent to decide on appeals against judgments handed down at first instance by the ordinary court and the court for minors. There are also 3 special sections (*sezioni distaccate di corte d'appello*) - Bolzano (*sezione distaccata corte d'appello di Trento*), Sassari (*sezione distaccata corte d'appello di Cagliari*) and Taranto (*sezione distaccata corte d'appello di Lecce*) – which we are not going to consider independently from their correlative *distretto* due to unavailability of these finer data.

<sup>17</sup>The total number of tribunals have been fluctuating over time due to several legislative reforms. Nowadays, the total number of tribunals is 145. See [www.giustizia.it](http://www.giustizia.it) – ministry of justice website (*giustizia* map).

<sup>18</sup>Data have been manually collected from bulletins reporting annual statistics of the Italian justice system (*Annuario di statistiche giudiziarie*), which can be found in ISTAT library.

<sup>19</sup>The disposition time formula is:  $365 * (\text{pending trials at the end of the year} / \text{definite trials at$

variable of interest. Table A.6 in Appendix presents descriptive statistics of law enforcement efficiency calculated with CEPEJ formula for 6 reference years between 1995 and 2012. Table 1 reports the average durations calculated with the CEPEJ formula for each of the 26 *distretti di corte d'appello*, over four reference years preceding the timing of our ITANES data (1995, 1997, 1999, 2001)<sup>20</sup>. It is important to notice the high degree of heterogeneity across the Italian districts, regardless the fact that law enforcement is regulated at a national level. This makes Italy a suitable case for the aim of our paper, as it allows us to exploit differences within the same country. We hope it could lead to more reliable inference on the relation between monitoring performance and interpersonal trust. In Figure 3 we report law enforcement efficiency as calculated using the CEPEJ formula for the years 1995 and 2001.

Tab.1  
*Average civil trial length*

District	Average duration (in days)			
	1995	1997	1999	2001
Ancona	1,361.620	1,252.191	1,255.113	544.710
Bari	1,348.989	989.405	1,294.763	1,023.625
Bologna	941.056	721.960	736.865	632.027
Brescia	893.412	1,063.777	673.981	687.024
Cagliari	1,142.401	1,081.535	960.083	800.785
Caltanissetta	1,390.929	1,077.141	1,020.105	723.980
Campobasso	1,357.762	812.060	1,178.524	647.927
Catania	1,073.792	986.557	842.265	718.869
Catanzaro	1,446.180	1,735.563	1,609.606	1,044.634
Firenze	903.296	746.456	746.221	631.462
Genova	787.571	777.862	821.060	605.895
L'Aquila	980.334	1,026.422	1,050.523	670.299
Lecce	1,227.814	1,062.715	796.184	929.385
Messina	1,777.657	1,651.616	1,845.981	1,406.063
Milano	630.564	525.655	530.471	475.305
Napoli	1,095.790	940.539	982.490	856.877
Palermo	1,063.533	785.494	743.258	728.997
Perugia	882.982	859.564	980.852	783.904
Potenza	2,751.813	783.905	705.524	1,060.491
Reggio Calabria	1,753.147	1,866.877	1,359.417	843.902
Roma	1,128.822	990.996	873.575	654.380
Salerno	1,366.287	787.397	1,199.693	896.919
Torino	713.997	474.427	528.692	383.849
Trento	831.312	571.598	582.965	430.247
Trieste	866.161	549.067	702.474	528.908
Venezia	971.465	718.780	823.027	659.215

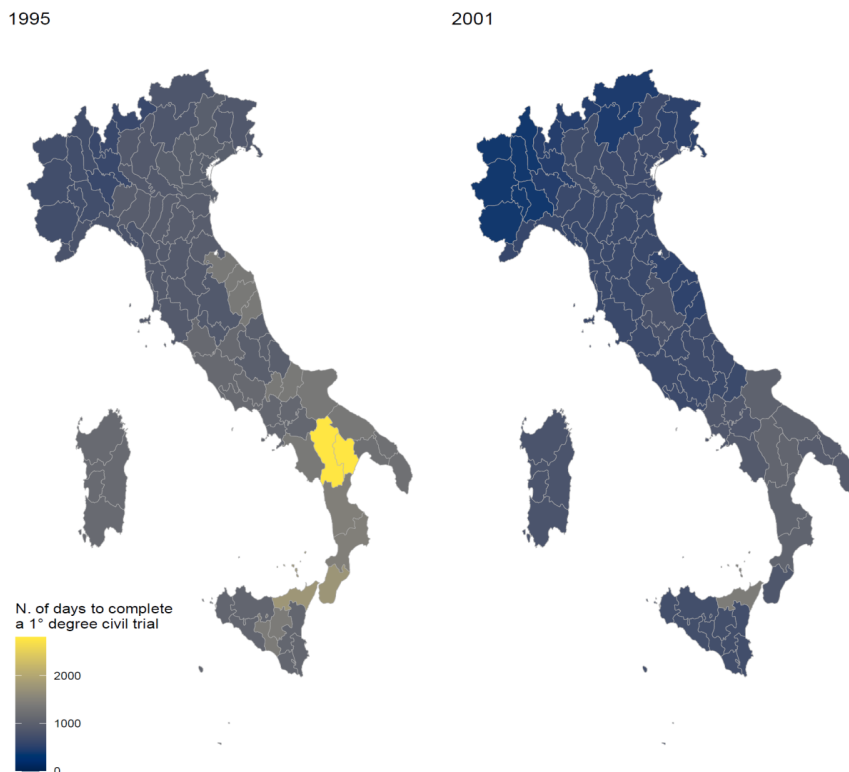
the end of the year). See [www.webstat.giustizia.it](http://www.webstat.giustizia.it) - ministry of justice website (in *procedimenti civili, durate*).

<sup>20</sup>Tab.A.7 in Appendix takes 1995, 2001, 2012 as reference years to picture the data behaviour for each district over all the considered time span.

*Notes:* average number of days it takes to complete a first degree civil trial in each Italian district court using the “disposition time” formula defined by the CEPEJ (European Commission for the Efficiency of Justice). Four reference years: 1995, 1997, 1999, 2001. Data were manually collected from annual statistics bulletins of the Italian justice system (*Annuario di statistiche giudiziarie*), which can be found in ISTAT (Italian National Institute of Statistics) library.

Fig.3

*Law enforcement efficiency in the Italian provinces*



*Notes:* average number of days it takes to complete a first degree civil trial in each Italian province according to the relative district court performance. Calculation is based on the “disposition time” formula defined by the CEPEJ (European Commission for the Efficiency of Justice). Two reference years: 1995, 2001. Data were manually collected from annual statistics bulletins of the Italian justice system (*Annuario di statistiche giudiziarie*), which can be found in ISTAT (Italian National Institute of Statistics) library.

By comparing the maps in Figure 3 we can notice two facts: from 1995 to 2001 law enforcement efficiency rose, with the average number of days to complete a first degree civil trial dropping from 1180 to 745 in 2001 at a national level. However, similarly to our proxies for trust, also this visual analysis underlined a systematic difference between Northern and Southern parts of Italy, persistent over time. Despite this analogy, it is very likely that problems of endogeneity exist in case one wants to study the effects of law enforcement efficiency on generalized trust using these data. Indeed, unobservable variables proper of a given area could drive both the existing level of generalized trust and enforcement performance, leading to inconsistent estimates. Moreover, it seems reasonable to think the presence of a two way relationship in this case, with the functioning of justice being improved by higher level of trust as well. To overcome these problems, in what follows we propose a novel identification strategy. Scope is to define a credible reduced form which is able to give us some insights about the relation under study.

## **4. Identification strategy**

### **4.1. Enforcement efficiency, lawyer density and the Codignola reform of the Italian university system**

First step of our identification strategy is to notice that law enforcement efficiency in Italy seems to be related with the density of lawyers operating in a given territory. For example, Buonanno and Galizzi (2014) explore the causality relationship between litigation and the number of lawyers for the case of Italian first instance courts of justice, between 2000 and 2007<sup>21</sup>. More in details, the debate on the causes of civil justice inefficiency in Italy identifies the high level of litigiousness as one of its main factors, together with shortcomings on

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<sup>21</sup>Litigation describes proceedings initiated between two opposing parties to enforce or defend a legal right. Litigation is settled by agreement between the parties but may also be heard and decided by a jury or judge in court.

the supply side and the inadequacy of legal rules (Finocchiaro Castro and Guccio, 2014). Indeed, the high level of litigiousness leads to clog up courts and increases the length of proceedings, generating side effects for the economy (Carmignani and Giacomelli, 2010). In Buonanno and Galizzi (2014), several measures of litigation are positively correlated with lawyer density: using an instrumental variable approach, they find that a 10% increase in the number of lawyers over population is associated with an increase between 1.6% and 6% in civil litigation rate. Their empirical analysis supports the supplier-induced demand hypothesis for the case of attorneys in Italy: following a sharp increase in the number of competitors over the last decades, and in the impossibility of competing on price because of a minimum fee regulation, some lawyers may have opportunistically used their informational advantage to induce clients to bring lawsuits into court more often than would have been optimal. Similar results can be found in Giacomelli and Carmignani (2010). In their paper, researchers investigate the relationship between lawyers and civil litigation across Italian provinces over the period 2000-2005. Authors document the existence of a positive correlation between lawyer density in a province and litigation, and employ a 2SLS approach to verify the existence of a causal effect. Their results show that an increase in lawyer density leads to more litigation and that the magnitude of this effect is sizeable.

Although - through litigation - lawyer density and law enforcement efficiency in Italian provinces seem intimately correlated, using the former as an instrument for the latter does not solve the problem of endogeneity we are worried about. In fact, it is reasonable to think the existence of some unobservable variables behind their correlation, which in turn might also affect interpersonal trust. Nevertheless, focusing on lawyer density has a useful advantage, as it helps us to define a plausible identification strategy. Indeed, according to Buonanno and Galizzi (2014), Giacomelli and Carmignani (2010) and Mora-Sanguinetti et al. (2015), lawyer density in a given province depends (also) on its distance from law schools. Rationale is lawyer density being an increasing function of law graduate density in a given area, which in turn depends on the costs to study law. It sounds reasonable to as-

sume the distance from a law school being a relevant determinant of these costs, as it affects for example commuting expenses as well as the decision to rent a place to move in (Carmignani and Giacomelli, 2010). Thus, in theory we might use this distance to map the distribution of lawyers - and in turn, of law enforcement efficiency - over the national territory. However, this distance could also relate to justice efficiency and our measures of trust in many ways other than through lawyer density only. It is usually the case that law schools are not randomly localized over a territory, but they are created in specific urban areas with predetermined social, demographic and institutional characteristics, which allow these faculties to function properly. For example, some of these unobservable attributes can be correlated to the number of people living in that city and the existence of a public transport service to reach the campus, accommodations and facilities for students, etc. To overcome the problem, previous authors focus their attention on the relationship between distance from “historical” law schools and density of active lawyers today. In particular, they argue that such a variable can be considered exogenous to the determinants of litigation in a given province nowadays. However, it should be correlated to lawyer density since proxy for historical costs to study law. Carmignani and Giacomelli (2010) use as instruments some proximity measures of the Italian provinces to public law schools active in 1975. Similarly, Mora-Sanguinetti et al. (2015) construct two groups of instrumental variables: one related to the number of law schools founded in Spain by 1968 in each province; the second, related to the distance between the current provincial capitals to the historical capital of their university district (where it was possible to study law) in 1845.

Similarly to the revised literature, in what follows we exploit for identification the link between law graduates and lawyer density. However, we do it in a novel way. In fact, we keep track of the effects over time on the yearly number of new law graduates of an important reform which made Italian universities more accessible: the Codignola reform in 1969. We do so by collecting data about the number of law degrees issued by all Italian public law schools over 56 years, from

1958 to 2014. Thanks to the temporal span of our panel set, we can monitor the augmentative effects on yearly graduates of the new legislation, and understand in which law faculties they were, on average, more pronounced. Our aim is to use the information coming from this unique dataset to realistically determine which areas of the country should have more law graduates in recent years due to the proximity to universities hit by the reform. In force of the connection between law degree holders and attorneys documented by the revised research, our goal is to map differences in lawyer density across Italian municipalities as well. Thanks to our dataset, we think our procedure to identify shifts in this distribution being more reliable with respect to previous works on the topic. In this way, we aim to obtain more defensible results. As long as this mapping is considered exogenous to unobservable determinants of trust and law efficiency, we can exploit it in our econometric models. Moreover, our approach allows us to escape from the issue of defining what are historical and not historical law faculties; in fact, this is a decision containing an element of arbitrariness which could affect the final results. Differently from the revised literature, it is a choice we do not have to make: we let the data speak for us.

The Codignola reform was promulgated in 1969<sup>22</sup>. With this new legislation the Italian government created the so called “mass university”: any type of 5 year high school degree grants the access to any public university. Indeed, before 1969 access to the university was conditioned by the type of high school attended. For instance, classical high school degree gave access to all faculties; scientific high school degree to all faculties, except for literature and philosophy; the artistic high school degree to architecture; accounting degree allowed to enrol in economics. Then, after 1969 there was a switch in the regime of the Italian public faculties, which became more accessible than before. Figure 4 gives suggestive evidence about the overall effect of the reform on both national law efficiency and lawyer density (see also Table 4 in Section 4.3 below for more formal results based on panel models): re-

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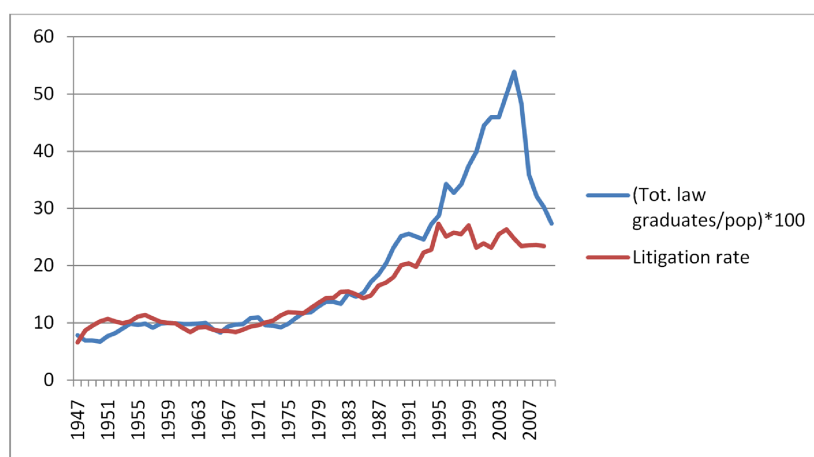
<sup>22</sup>More precisely, the Codignola reform is the law 11 December 1969, n.910. Its text can be freely consulted here: [www.gazzettaufficiale.it/eli/id/1969/12/13/069U0910/sg](http://www.gazzettaufficiale.it/eli/id/1969/12/13/069U0910/sg).



ardless an economic boom during the 1950s and the 1960s, as well as a population which steadily increased over the considered years (until the 2000s), the ratio of law graduates over population stayed constant, starting its climb right after the Codignola reform, and doubling its value within 20 years. However, as we make clear in what follows, the reform did not have homogenous effects on all Italian territories: in fact, it is very likely the case some areas of the country have witnessed a greater and systematic increase in the number of law degree holders - and then, of lawyers - with respect to others. In what follows, we exploit these differences for identification.

Fig.4

*National litigation rate and the number of law graduates over the population*



*Notes:* time series of the Italian litigation rate between 1947 and 2009, together with the ratio between total number of law graduates and national population, over the same years. Correlation is equal to 0.92. Data source is the *Serie Storiche* database by ISTAT (Italian National Institute of Statistics).

## 4.2. Identification and balancing tests

We now explore more in details the intuition behind our strategy. In accordance with the revised literature, distance from a law school – as an important factor influencing the costs of studying law – correlates

with density of law degree holders. For the case of Italy, this in turn seems to affect close tribunals' efficiency. In fact, an higher presence of graduates in law translates into more operating lawyers and then, into more litigation clogging up courts (Buonanno and Galizzi, 2014; Carmignani and Giacomelli, 2010; Mora-Sanguinetti et al., 2015). Before the Codignola reform of 1969, each Italian town was located to a certain distance from the existing closest public law school. According to ISTAT data as well as accessible online sources, 26 public law schools were active at that time. These faculties were located in Turin, Milan, Pavia, Teramo, Padua, Trieste, Genoa, Parma, Modena, Bologna, Ferrara, Florence, Pisa, Siena, Perugia, Urbino, Camerino, Macerata, Rome, Naples, Bari, Palermo, Messina, Catania, Sassari, Cagliari. After the law was promulgated, there was an increase in the demand for legal studies brought about by milder conditions to access the faculty. Indeed, the entire system of Italian public universities switched its regime: from a public one, but with constraints to access some types of faculties (law schools included), to a mass university regime. Over the next years, other public law schools were founded, all functioning under the new regime. According to ISTAT and MIUR (Italian Ministry of University Education and Research), in 1997 - last foundation year considered by our research<sup>23</sup> - the number of these public institutions was 34 instead<sup>24</sup>. The new faculties were also located in Brescia, Caserta, Alessandria, Campobasso, Foggia,

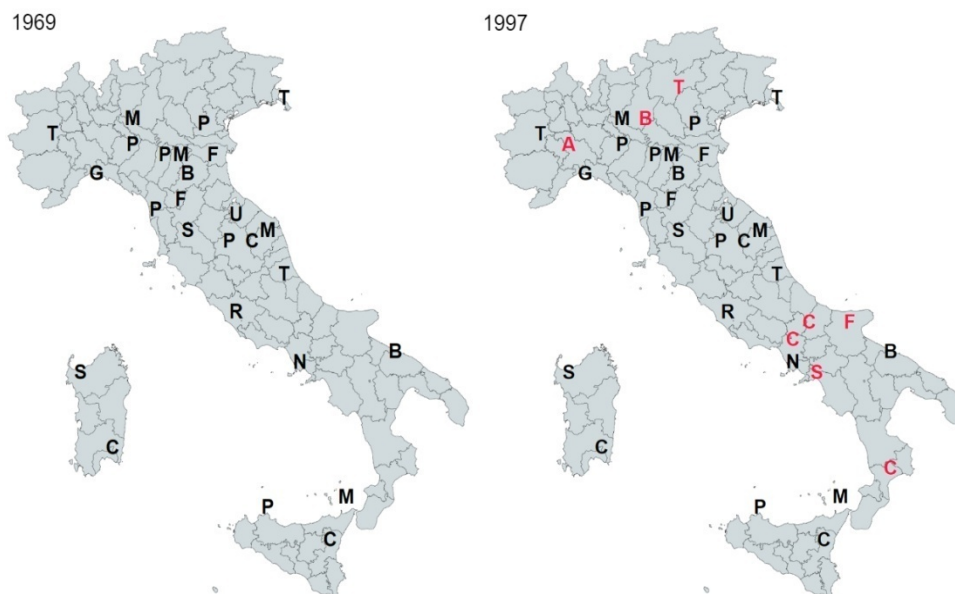
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<sup>23</sup>We chose 1997 as the last reference year (thus, not considering law schools created afterwards) for the following reasons: (i) these law schools were already active before another important reform of the Italian university system known as the Bologna reform of 1999; we hope to limit in this way confounding effects between the two reforms; (ii) it is more likely that these older faculties had an effect, if any, on the lawyer density level (by issuing law degrees) observable around the years 2001, 2008, 2013 of the ITANES surveys. Indeed, after graduated, lawyer candidates need to go through a long preparative process before starting their careers. And this process usually takes several years to be completed. The national law regulates the criteria needed for lawyers to be eligible to enrol into official registers. In fact, access to the legal profession in Italy requires a first degree in law (5 years), followed by a 2-year apprenticeship in a legal practice (*praticantato*). In order to obtain the official qualification as lawyers, successful candidates have then to pass a two-stage selection process, which takes approximately 1 year to conclude. Therefore, considering more recent years with respect to 1997 to map the density of operating lawyers over the timing of the ITANES surveys could be misleading.

<sup>24</sup>According to ISTAT and MIUR, in 2013 – year of the last ITANES survey considered for the research – the number of these public institutions was 41.

Catanzaro, Salerno, Trento.

Fig.5  
*Active public law schools*



*Notes:* initial letters of the name of Italian cities with at least a public law school active at the time of the Codignola reform of 1969 (left panel) and in 1997 (right panel). In black: Turin, Milan, Pavia, Teramo, Padua, Trieste, Genoa, Parma, Modena, Bologna, Ferrara, Florence, Pisa, Siena, Perugia, Urbino, Camerino, Macerata, Rome, Naples, Bari, Palermo, Messina, Catania, Sassari, Cagliari. In red: Brescia, Caserta, Alessandria, Campobasso, Foggia, Catanzaro, Salerno, Trento.

However, only 2 more public law schools were founded during the 1970s, and 4 more during the 1980s. At the same time, it is important to notice that by the end of the 1980s, law graduate density doubled (see Figure 4) whilst the number of law schools increased by less than a quarter. Thus, in light of this described “bottleneck effect” it sounds reasonable to believe most of the new demand for studies in law induced by the national level reform being satisfied (in particular over the 1970s and the 1980s) by the set of all institutions active in 1969. In Section 4.3 we present evidence to show that this has been likely

the case: yearly new graduates in law over the Italian population systematically increased after 1969 for the group of universities in activity at that time. As well, there are no strong a priori reasons to believe that over the years, the more recent law schools - operating under the new “mass university” regime as well - absorbed enough students to counterbalance this effect<sup>25</sup>. The panel structure of our data allows us to test this claim. According to our estimates, at least until 2014 those faculties already in activity at the time of the Codignola reform issued on average more law degrees with respect to schools established after the new legislation (see Section 4.3 and 4.4). Thus, our main identifying hypothesis is the following: as long as we agree on the relation between distance from a faculty and student density documented by the literature, territories closer to the set of law schools in activity already in 1969 are likely to be characterized, on average, by a greater stock of graduates in law - and then of operating attorneys - with respect to areas closer to more recent public universities.

We recall that in the Italian provinces a higher lawyer density has been associated to lower justice efficiency not explainable by socioeconomic fundamentals (Finocchiaro Castro and Guccio, 2014; Buonanno and Galizzi, 2014)<sup>26</sup>. In the light of these results, density of lawyers in municipality  $a$  should proxy for technical efficiency of its closest tribunals. Then, according to our data those municipalities closer to law schools active in 1969 should be, on average, also in proximity of less performing courts. We can use this argument to define an instrument for law enforcement efficiency over the Italian territory and studying its effects, if any, on our two proxies for trust.

To make our point, the main results presented in Section 5 come from

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<sup>25</sup>In other words, we want that *ceteris paribus* the number of law degrees issued by more recent schools did not offset that one of the group operating already in 1969, at least by the time we measure our two proxies for trust.

<sup>26</sup>As mentioned, reasons behind these results seem to be a higher litigation rate due to an asymmetric information advantage of lawyers on clients as well as strong rigidities - at least until the so-called Bersani decree in 2006 - to compete on prices as minimum fees for legal services were in place (Finocchiaro Castro and Guccio, 2014; Buonanno and Galizzi, 2014). About this, Section 6 presents and discusses some empirical results we obtain using novel data. Our evidence is in line with previous findings: lawyer density is positively and significantly correlated to the length of first degree civil trials, our measure of enforcement efficiency.

reduced form equations based on the following econometric setting:

$$TRUST_{i,a,j} = \gamma_0 + \gamma_1 Z_{a,j} + \gamma_j + u_{3,i,a,j} \quad (1)$$

where  $TRUST_{i,a,j}$  is equal to 1 if for individual  $i$ , resident in city  $a$ , province  $j$ , “most people can be trusted”<sup>27 28</sup>.  $Z_{a,j}$  equals 1 if the closest law school to town  $a$  in 1997, province  $j$ , was already active before the Codignola reform;  $\gamma_0$  is a constant term;  $\gamma_j$  are province fixed effects;  $u_{3,i,a,j}$  are the residuals. Moreover,  $\gamma_0 = \alpha_0 + \alpha_1 \pi_0$ ,  $\gamma_1 = \alpha_1 \pi_1$ ,  $\gamma_j = \alpha_j + \alpha_1 \pi_j$  and  $u_{3,i,a,j} = \alpha_1 u_{1,a,j} + u_{2,i,a,j}$ . These elements of Equation 1 come from plugging Equations 3 in 2:

$$TRUST_{i,a,j} = \alpha_0 + \alpha_1 d_{a,j} + \alpha_j + u_{2,i,a,j} \quad (2)$$

$$d_{a,j} = \pi_0 + \pi_1 Z_{a,j} + \pi_j + u_{1,a,j} \quad (3)$$

where  $d_{a,j}$  is lawyer density in municipality  $a$ , province  $j$ .  $\pi_0$  and  $\alpha_0$  are constant terms;  $\pi_j$  and  $\alpha_j$  are province fixed effects;  $u_{1,a,j}$  and  $u_{2,i,a,j}$  are the residuals. Equation 3 describes how the Codignola reform impacted lawyer density. Equation 2 refers to hypothetical effects of this density on individual level trust - through law enforcement efficiency. However, we do not apply a 2SLS procedure here. We are forced to estimate Equation 1 since we do not have data about  $d_{a,j}$ <sup>29 30</sup>.

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<sup>27</sup>We do not explicitly set the setting also for our second measure of trust (namely the number of nonprofit organizations per person at a municipal level). However, intuitions would remain unchanged.

<sup>28</sup>For the sake of clarity, here we do not consider other regressors such as personal characteristics of respondents. However, we do consider more comprehensive models when we go to the data. See Section 5 for details.

<sup>29</sup>Assuming that  $\alpha_1$  in Equation 2 is the true effect of justice inefficiency on generalized trust (through lawyer density), we want to stress that  $\gamma_1$  in Equation 1 does not capture exactly this effect. More precisely,  $\gamma_1$  is the effect on trust of the higher inefficiency caused by the Codignola reform through its impact on lawyer density.

<sup>30</sup>We have information on the yearly number of lawyers enrolled in each *albo degli avvocati* (city-level lawyer registers). However, the number of these *albi* is very small compared to the totality of Italian municipalities. They were located in 166 municipalities only; in 139 after 2015 (Italian municipalities are 7903 as on July, 1st, 2020; see [www.istat.it/it/archivio/6789](http://www.istat.it/it/archivio/6789)). Therefore, we do not think that using these data to obtain an approximation of  $d_{a,j}$  could be reliable.

Crucial assumptions to estimate  $\gamma_1$  are our instrument  $Z_{a,j}$  being relevant - namely  $\pi_1 \neq 0$ ;  $Z_{a,j}$  affects  $TRUST_{i,a,j}$  only through  $d_{a,j}$  - exclusion restriction;  $Z_{a,j}$  being exogenous to trust and other determinants of lawyer density - from which follows that  $Cov(u_{1,a,j}, Z_{a,j}) = 0$  and  $Cov(u_{2,i,a,j}, Z_{a,j}) = 0$ . Does our setting satisfy these conditions? Our mentioned results on the dynamics of new graduates in law after the Codignola reform leads us to think our instrument  $Z_{a,j}$  being relevant. In Section 4.3 below we present the empirical evidence supporting this claim. Turning to the other conditions, thinking as exogenous to law efficiency and trust level in town  $a$  the switch (in 1969) to a mass university system at a national level does not sound too unrealistic. However, even if the Codignola reform was realistically exogenous, it might be argued that some of those municipalities affected the most by the consequences of the new legislation are also in proximity of long-established law schools. In fact, 22 out of 26 faculties operating in 1969 were already in function well in advance the completion of the Italian unification in 1871<sup>31</sup>. Thus, it might be possible that some spurious relations between our indicator dummy  $Z_{a,j}$  and unobservable variables occur which drive our results. Long lasting cultural and institutional features related to the distance from historical universities could credibly affect both generalized trust and enforcement performance of each area, leading us to obtain inconsistent estimates (e.g. Acemoglu and Robinson, 2008; Alesina and Fuchs-Schündeln, 2007; Guiso et al., 2016). For example, it might be the case of these faculties being localized in geographical areas historically more favourable to economic activity and then, to the settlement of better designed institutions. Nowadays, persistent spillovers on both trust and law enforcement efficiency in proximity of these territories might be expected (Acemoglu et al., 2001; Putnam, 1993; Guiso et al., 2004).

We cannot completely rule out this issue. However, we show empirically that it might be a minor concern in our setting. For this purpose, we ran a battery of balance tests using city level data coming from

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<sup>31</sup>For example, in this set there is the law school of Bologna, one of the most ancient in the world.

the national census conducted by ISTAT in 1961<sup>32</sup>. Explanations and descriptive statistics for all implemented variables can be found in Table A.2 in Appendix. Scope of this exercise is to show that before the Codignola reform was promulgated, the two sets of municipalities identified by the dummy  $Z_{a,j}$  were on average identical. This fact would make us more confident about the plausibility of using  $Z_{a,j}$  to identify only the effects of the reform on generalized trust. Tables 2 and 3 report the results of these tests. As mentioned earlier, to construct  $Z_{a,j}$  (the variable “Closest law school active before Codignola reform” presented in both tables), we consider the distance from the closest public law school active in 1997. Using R software and its packages for geolocalization, we first calculated the distance in kilometres of town  $a$  from every public law school active in 1997 and then we took the minimum. After that, we defined our dummy variable  $Z_{a,j}$  being equal to 1 if this school was in function already before the Codignola law. In our sample, Italian municipalities are localized at a distance of 35.74 kilometres on average from their closer public law schools. We report further details in Table A.3 in Appendix.

According to Table 3, our dummy positively correlates with the resident population in 1961 at a significance level of 10%. A priori, we cannot exclude that resident population - and its dynamic over time - could be related to enforcement efficiency and generalized trust in some ways. This being true, it would lead us to obtain inconsistent results. For this reason, in what follows we show results from our reduced forms where we also control for this variable. We try in this way to detect potential biases - if any - due to omitted controls.

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<sup>32</sup>We use this national census since it is the closer in terms of years to the Codignola reform of 1969.

Tab.2  
*Balance tests*

	<i>Dependent variable:</i>				
	Property house	Services per property	Gender schooling disparities	Labour market participation (males)	Employment rate
Closest law school active before Codignola reform	2.461 (6.766)	-9.138 (6.817)	-18.689 (44.264)	-1.532 (3.122)	-1.005 (3.204)
Province FE	YES	YES	YES	YES	YES
Observations	7,663	7,663	7,561	7,663	7,663
R <sup>2</sup>	0.424	0.367	0.111	0.371	0.374
Adjusted R <sup>2</sup>	0.416	0.357	0.098	0.362	0.365

*Notes:* OLS estimates using Italian municipal data. Dependent variables are the social dimensions explored by ISTAT (Italian National Institute of Statistics) in its national level census of 1961. Main control is a dummy variable which indicates if the closest public law school (in 1997) to town *a* was active before Codignola reform. Robust standard errors in parentheses. \* p<0.1; \*\* p<0.05; \*\*\* p<0.01.

Tab.3  
*Balance tests*

	<i>Dependent variable:</i>			
	Resident pop.	Pop. scatter	Male pop. share	Elderly index
Closest law school active before Codignola reform	642.397* (371.924)	-0.851 (9.381)	1.685 (3.233)	-8.446 (13.641)
Province FE	YES	YES	YES	YES
Observations	7,659	7,663	7,663	7,663
R <sup>2</sup>	0.162	0.458	0.218	0.678
Adjusted R <sup>2</sup>	0.150	0.450	0.207	0.674

*Notes:* OLS estimates using Italian municipal data. Dependent variables are the social dimensions explored by ISTAT (Italian National Institute of Statistics) in its national level census of 1961. Main control is a dummy variable which indicates if the closest public law school (in 1997) to town *a* was active before Codignola reform. Robust standard errors in parentheses. \* p<0.1; \*\* p<0.05; \*\*\* p<0.01.



### 4.3. Preliminary evidence

As pointed out in the previous paragraphs, core of our empirical approach are the following mechanisms. Firstly, the promulgation of Codignola law in 1969 led to a greater number of students accessing public law schools due to the creation of a mass university system. However, we also observed that most of the new demand for legal studies has been satisfied over time by those institutions already in activity in 1969. Thus, in the years of our proxies for trust the density of law graduates in areas closer to this set of faculties is likely to be, on average, higher. Secondly, to a higher density of law degree holders should correspond a higher density of operating lawyers. In this paragraph we show evidence supporting these claims.

Historical data about the density distribution of law graduates over the Italian peninsula are not available. Thus, as already mentioned we track the effects of the Codignola reform by collecting yearly data about the number of law degrees issued by each public university, from 1958 to 2014. According to these data, in 1960 there were 25 public law schools in activity with an average number of graduated students per year equal to 209. In 1997, last year considered for defining our dummy  $Z_{a,j}$ , this average was 556 with 34 institutions in activity. Table A.5 in Appendix reports descriptive statistics for 6 reference years from our panel set. The implicit assumption here is that these data might help us to understand where the density of law degree holders should be higher in the early 2000s, timing of our dependent variables for trust.

Below we present results from a panel analysis intended to study the behaviour of the collected data after the promulgation of the Codignola reform. Models 1 and 2 in Table 4 give us an indication about the macro effects of mass university creation. In fact, the positive and significant coefficient of the post Codignola year dummy seems to confirm that, in line with what expected, the ratio between yearly new law graduates and the Italian population systematically increased afterwards. However, more importantly for us is to notice that this incremental effect was on average more pronounced for the entire group of universities already operating at the time of the reform (as signalled

by the coefficient of the interaction term in Models 3, 4 and 5). Also very important it is to notice that the average difference in the number of law degrees issued by this group of schools lasted for more than 30 years, until the early 2000s - timing we start to observe our proxies for generalized trust - and beyond<sup>33</sup>. In the light of this persistent and systematic difference, we claim that also the density of law degree holders in proximity of this set of schools should be greater than the other group. In particular, this difference between densities should be very likely around the early 2000s, about 30 years after the Codignola reform. We further test our claim as follows. For each city in our panel we define a proxy for the law graduate density in 1997. To do this, we calculate a cumulative sum of the yearly number of students graduated in law from the corresponding university, starting from 1958, first available year of our data set. Then, we divide these figures by the resident population in 1997 of every considered town. Finally, we regress this variable on a dummy equal to 1 if the law school in that city was active at the time of the reform. Also these results (see Table A.12 in Appendix) are in line with our statement: on average, density of law degree holders in 1997 is higher in those cities with a law school active at the time of the Codignola reform.

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<sup>33</sup>Cross sectional results presented in Appendix (see Tables A.8, A.9, A.10, A.11) indicate that until 2014 the absolute number of law students completing their degree was systematically higher in those schools already active before 1969.

Tab.4

*Panel estimation of the Codignola reform effect on the number of yearly law graduates (public universities only)*

	<i>Dependent variable:</i>				
	Law grad./Italian pop. (with NAs)		Law grad./Italian pop. (with 0 instead of NAs)		
	(1)	(2)	(3)	(4)	(5)
Post Codignola year	0.0025338*** (0.0003412)	0.0018992*** (0.0002188)	0.0008431*** (0.0001382)		
Post Codignola year *			0.0017060*** (0.0003622)	0.0017060*** (0.0003493)	0.0009550** (0.0004058)
Pre Codignola Uni.					
Observations	1.428	2.016	2.016	2.016	2.016
R <sup>2</sup>	0.0446210	0.0401562	0.0477975	0.1732189	0.3293893
Adjusted R <sup>2</sup>	0.0156492	0.0197236	0.0270344	0.1350136	0.2831402
University FE	YES	YES	YES	YES	YES
Year FE	NO	NO	NO	YES	YES
Linear trend	NO	NO	NO	NO	YES

*Notes:* results from fixed effects panel models. Time span: 1958 – 2014. The dependent variable in all models is the ratio between the number of law students graduated from faculty  $i$  in year  $t$  and Italian population the same year. Data come from ISTAT (Italian National Institute of Statistics) bulletins reporting annual statistics of the Italian schooling system (*Annuario statistico dell'istruzione*), and MIUR (Italian Ministry of University Education and Research) online database. In Model 1, we treated years before a law faculty started its activity (and thus, it started issuing law degrees) as missing values. In all the other models, these years are treated as if the issuing of law degrees equals 0. Heteroskedasticity - autocorrelation robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

As long as this mapping of law graduate density across Italian cities is considered exogenous to other determinants of municipal level enforcement efficiency and generalized trust, we can use it for identification. And this, in force of its connection with lawyer density. Thus, between 2001 and 2013, years for which we measure trust, we want lawyer density being on average higher in those territories closer to a law school already in place before 1969. In Table 5 and Table 6 below we try to test these assumptions. In Table 5 we present pooled regressions where a proxy for the density of lawyers is regressed on law graduate density (proxy). To allow for estimation, we aggregate data about lawyers

enrolled in each *albo degli avvocati* (city-level lawyer registers), at a provincial level. In 2001 the number of lawyers (by province) signed in these registers was on average 1265. In 2012 it was equal to 2220. Data were provided for this research by *Cassa Forense* (Italian Institute for Provident Fund and National Forensic Services) and are available under request (Table A.4 in Appendix provides a description of the dataset; data have been aggregated at a province level). In particular, the proxy for lawyer density is calculated as the sum of lawyers enrolled to all lawyer registers of province  $j$  at year  $t$ , divided by the resident population of province  $j$  at time  $t$ <sup>34</sup>. Similar to before, law graduate density proxy (in 1997) is obtained by summing by province all law graduates from each public university, from 1958 to 1997; this figure is then divided by the population of the corresponding province in 1997<sup>35</sup>.

Tab.5

*Relation between provincial level law graduate and lawyer density*

	<i>Dependent variable:</i>		
	Lawyer density		
	(1)	(2)	(3)
Law graduate density proxy (1997)	0.146*** (0.017)	0.160*** (0.015)	0.153*** (0.014)
Macro region FE (North-east/west, Center, South, Islands)	NO	YES	NO
Region FE	NO	NO	YES
Year FE	YES	YES	YES
Observations	600	600	600
R <sup>2</sup>	0.224	0.476	0.598
Adjusted R <sup>2</sup>	0.216	0.467	0.580

<sup>34</sup>Indeed, in Italy it is mandatory to enrol in these registers to work as lawyer. However, it is also possible for a person entitled to work as lawyer to enrol in such registers and not operate. Therefore, with these data in hand, it is only possible for us to calculate a proxy for the operating lawyer density in a given province.

<sup>35</sup>We recall that after graduation the access to the legal profession in Italy requires 2-year apprenticeship in a legal practice (*praticantato*) and a two-stage selection process, which takes approximately 1 year to conclude. Therefore, considering law students graduated later than 1997 to explain the density of operating lawyers from 2001 could be prone to errors.

*Notes:* OLS estimation with pooled data. Data are at a provincial level (2001 administrative division). Law graduate density proxy (1997) is obtained by summing (by province) all law degrees issued by each public university from 1958 to 1997; this figure is then divided by the population of the corresponding province in 1997. Lawyer density is calculated for the years 2001, 2004, 2006, 2008, 2010, 2012 as the ratio between the sum (by province) of lawyers enrolled in the *albi* over the provincial population of each year. Data sources: *Cassa Forense* (Italian Institute for Provident Fund and National Forensic Services) and ISTAT (Italian National Institute of Statistics). The 5 macro regions correspond to NUTS 1 Italian areas. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Tables 5 and 6 confirm the relation between public law schools, law degree holders and the presence of lawyers also documented by the revised literature on the performance of the legal system in Italy. Together with the evidence we found about the effects of the Codignola reform on law graduate density, these results point to the plausibility of our strategy to identify systematic differences in the density of lawyers as well.

Tab.6

*Relation between the number of law schools active before the Codignola reform and lawyer density*

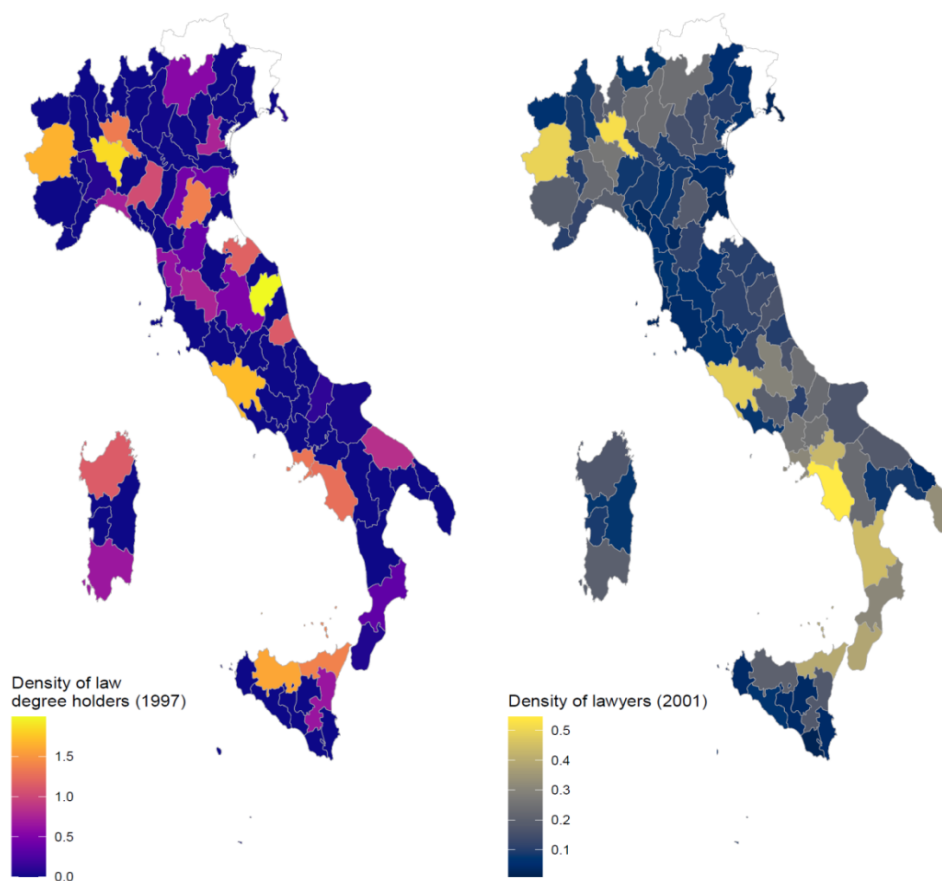
	<i>Dependent variable:</i>		
	Lawyer density		
	(1)	(2)	(3)
Number of law schools active before Codignola reform	0.069*** (0.016)	0.110*** (0.015)	0.127*** (0.015)
Macro region FE (North-east/west, Center, South, Islands)	NO	YES	NO
Region FE	NO	NO	YES
Year FE	YES	YES	YES
Observations	601	601	601
R <sup>2</sup>	0.073	0.339	0.514
Adjusted R <sup>2</sup>	0.064	0.327	0.493

*Notes:* OLS estimation with pooled data. Data are at a provincial level (2001 administrative division). The number of law schools active before the Codignola reform is calculated by province. Lawyer density is calculated for the years 2001,

2004, 2006, 2008, 2010, 2012 as the ratio between the sum (by province) of lawyers enrolled in the *albi* over the provincial population of each year. Data sources: *Cassa Forense* (Italian Institute for Provident Fund and National Forensic Services) and ISTAT (Italian National Institute of Statistics). The 5 macro regions correspond to NUTS 1 Italian areas. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Fig.6

*Density of law degree holders and lawyers in the Italian provinces*



*Notes:* comparison between density of law degree holders and lawyers. Law graduate density in 1997 is a proxy obtained by summing by province the number of law degrees issued by each public university, from 1958 to 1997; this figure is then divided by the population of the corresponding province in 1997. Layer density is the sum of lawyers enrolled in each *albo degli avvocati* (city-level lawyer registers),

at a provincial level in 2001, divided by the resident population in the same year. Sources of the data are ISTAT (Italian National Institute of Statistics), *Cassa Forense* (Italian Institute for Provident Fund and National Forensic Services) and own calculations.

#### 4.4. Placebo tests

We now want to test if the effects of the Codignola reform on the dynamics of yearly graduates in law described in Section 4.3 are imputable to this new legislation or they could be due to other unobserved factors. To do this, we run two different placebo tests. The first one consists in checking if the behaviour of the effects caused by the reform on law degree holders is somehow realistic. In fact, a priori we would expect two things. The reform reduced barriers to enrol in law schools almost immediately after its promulgation at the end of 1969. However, matriculations - and then graduations - were likely to react slowly to the new incentives. Therefore, the average push to both enrolments and graduations should have reached a maximum after sometimes; then, it should have faded away as time went by. In fact, as the demand for legal studies approached its new equilibrium, and the number of yearly graduates also stabilized, no more significant effects should be observed in our data when comparing subsequent years. This reasonable behaviour for the effects of the new legislation over time is precisely what we detect in our data. Results from a set of panel models are presented in Tables 7 and 8.

Observing the magnitude of the coefficients from Model 1 to Model 6 in both tables we can detect evidence supporting our a priori beliefs. For those universities active at the time of the reform, the increase in law degree holders became more pronounced over time, reaching its peak during the 1980s and fading away over the 1990s. We interpret these results as a further sign of the reliability of our identification strategy based on the long lasting effects of the Codignola reform. In other words, it seems plausible to think the average push to new degrees in law we documented before in Section 4.3 for universities active since 1969 being caused by the legislation.

Tab.7

*Effects of the Codignola reform on the number of law graduates considering 5 consecutive time spans (no linear trend)*

	<i>Dependent variable:</i>					
	Law graduates / Italian population					
	(1)	(2)	(3)	(4)	(5)	(6)
Post Codignola reform year *	0.0017060***					
Law school active before reform	(0.0003493)					
Post 1975 year *		0.0019583***				
Law school active before reform		(0.0003453)				
Post 1980 year *			0.0021849***			
Law school active before reform			(0.0003740)			
Post 1985 year *				0.0023059***		
Law school active before reform				(0.0004347)		
Post 1990 year *					0.0018382***	
Law school active before reform					(0.0005337)	
Post 1995 year *						0.0003312
Law school active before reform						(0.0006563)
Observations	2,016	2,016	2,016	2,016	2,016	2,016
R <sup>2</sup>	0.1732189	0.1778605	0.1822609	0.1836749	0.1757432	0.1658310
Adjusted R <sup>2</sup>	0.1350136	0.1398696	0.1444733	0.1459527	0.1376544	0.1272843
Law school FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

*Notes:* fixed effects panel data regressions. Time span: 1958 – 2014. Data sources of the number of law graduates for every public law school are ISTAT (Italian National Institute of Statistics) and MIUR (Italian Ministry of University Education and Research). For the case of those law schools established after 1958, we put a value equal to 0 until the first figure is observed. Heteroskedasticity - autocorrelation robust standard errors in parentheses. \* p<0.1; \*\* p<0.05; \*\*\* p<0.01.



Tab.8

*Effects of the Codignola reform on the number of law graduates considering 5 consecutive time spans (linear trend)*

	<i>Dependent variable:</i>					
	Law graduates / Italian population					
	(1)	(2)	(3)	(4)	(5)	(6)
Post Codignola reform year *	0.0009550**					
Law school active before reform	(0.0004058)					
Post 1975 year *		0.0018410***				
Law school active before reform		(0.0004701)				
Post 1980 year *			0.0029638***			
Law school active before reform			(0.0005423)			
Post 1985 year *				0.0034422***		
Law school active before reform				(0.0005765)		
Post 1990 year *					0.0015389**	
Law school active before reform					(0.0006410)	
Post 1995 year *						-0.0028771***
Law school active before reform						(0.0007316)
Observations	2,016	2,016	2,016	2,016	2,016	2,016
R <sup>2</sup>	0.3293893	0.3324663	0.3378116	0.3401035	0.3304484	0.3356027
Adjusted R <sup>2</sup>	0.2831402	0.2864295	0.2921434	0.2945934	0.2842724	0.2897822
Law school FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Linear trend	YES	YES	YES	YES	YES	YES

*Notes:* fixed effects panel data regressions. Time span: 1958 – 2014. Data sources of the number of law graduates for every public law school are ISTAT (Italian National Institute of Statistics) and MIUR (Italian Ministry of University Education and Research). For the case of those law schools established after 1958, we put a value equal to 0 until the first figure is observed. Heteroskedasticity - autocorrelation robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Our second placebo test is designed to understand if the positive effects we have found on law graduate holders are spurious. As previously discussed, the Codignola reform and its effects are reasonably exogenous to law enforcement efficiency and generalized trust. Results from a battery of balance tests fairly support our claim. In force of this argument, we use the documented repercussions of the reform on law graduate density - and then, on lawyer density - for identification. Here in this section we further test our mechanism by exploiting the

temporal dimension of our dataset. In fact, our approach would be less defensible if we can show that other factors, and not the reform, could explain the movements we observed over time in the number of law graduates. To test for this possibility, we run a set of panel models in which we compare the effects of the Codignola reform with 5 different fictitious legislations. We arbitrarily choose the following years: 1975, 1980, 1985, 1990 and 1995. In line with what we have done for the Codignola reform, to capture the hypothetical effects of each fictitious legislation we construct an interaction term as follows: we multiply a dummy equal to 1 if the law school was active the year of the fictitious reform by another indicator variable equal to 1 for all subsequent years. Considering for example our fictitious reform in 1980, its corresponding variable comes by interacting a dummy equal to 1 if law school  $i$  was active in 1980, with another variable equal to 1 for all years after 1980, 0 otherwise. Calculations are shown in Tables 9 and 10 and they seem supportive of our theory. In particular, results from both Models 1 are something to be expected due to the following reasons. The Codignola reform was promulgated in December, 1969. At that time, and until the early 2000s, in Italy a degree in law lasted 4 years. Thus, it is very likely the case that the first significant effects of the law on the yearly number of new issued degrees should be observed 4/5 years after – namely around 1975, year of our first fictitious reform tested in both Models 1. As well, we notice that the two sets of law schools indicated by the two dummies “Law school active before the reform” and “Law school active in 1975” are almost identical. They differ only for one element: the Salerno law school (in the latter group), which started to issue its degrees in 1972. Given these motivations, it is reassuring that the coefficient of “Post 1975 year \* Law school active in 1975” and not of “Post Codignola reform year \* Law school active before reform” is significant. We believe that this evidence might be interpreted as a further sign supporting the rationale behind our strategy. Moreover, also the results coming from the other panel specifications of Tables 9 and 10 do not contradict it. It is true that coefficients of the fictitious reforms are positive and significant in most cases. However, it should not be surprising. As we already observed, effects of the Codignola

reform spread out mostly during the late 1970s and the 1980s.

Tab.9

*Effects of the Codignola reform compared with 5 similar fictitious reforms (no linear trend)*

	<i>Dependent variable:</i>				
	Law graduates/Italian population				
	(1)	(2)	(3)	(4)	(5)
Post 1975 year *	0.0027562***				
Law schools active in 1975	(0.0003756)				
Post 1980 year *		0.0023434***			
Law school active in 1980		(0.0003544)			
Post 1985 year *			0.0023490***		
Law school active in 1985			(0.0003911)		
Post 1990 year *				0.0020003***	
Law school active in 1990				(0.0004828)	
Post 1995 year *					0.0001137
Law school active in 1995					(0.0005623)
Post Codignola reform year *	-0.0004473	0.0002820	0.0006457**	0.0010288***	0.0016882***
Law school active before reform	(0.0003669)	(0.0003252)	(0.0003063)	(0.0003038)	(0.0003325)
Observations	2,016	2,016	2,016	2,016	2,016
R <sup>2</sup>	0.1853179	0.1859796	0.1872860	0.1829239	0.1732394
Adjusted R <sup>2</sup>	0.1472288	0.1479215	0.1492890	0.1447229	0.1345857
Law school FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

*Notes:* fixed effects panel data regressions. Time span: 1958 – 2014. Data sources of the number of law graduates for every public law school are ISTAT (Italian National Institute of Statistics) and MIUR (Italian Ministry of University Education and Research). For the case of those law schools established after 1958, we put a value equal to 0 until the first figure is observed. Heteroskedasticity - autocorrelation robust standard errors in parentheses. \* p<0.1; \*\* p<0.05; \*\*\* p<0.01.

Moreover, it was a national level law which did not target only those schools active when it was introduced in 1969. Public faculties created afterwards all started their activity under the new enrolment regime and they absorbed part of the demand for legal studies. These considerations together reasonably explain why our interactions for the fictitious reforms capture some positive and significant effects of the dynamics of law degree holders. They also explain why their mag-

nitudes are larger over the 1970s and the 1980s<sup>36</sup>. However, what is important for our identification strategy is to find that the coefficients of the interaction for the Codignola reform remain positive, significant in most cases. Thus, in the light of these results we can still conclude that, *ceteris paribus*, the sample of law schools in activity in 1969 issued more degrees in law over the 4 decades following the Codignola reform. Therefore, again we claim that municipalities closer to these institutions should have on average a higher density of law graduates and then, of operating lawyers. This is what we need for identification and our results from this second placebo test do not contradict it.

Tab.10

*Effects of the Codignola reform compared with 5 similar fictitious reforms (linear trend)*

	<i>Dependent variable:</i>				
	Law graduates/Italian population				
	(1)	(2)	(3)	(4)	(5)
Post 1975 year *	0.0028199***				
Law schools active in 1975	(0.0004191)				
Post 1980 year *		0.0033924***			
Law school active in 1980		(0.0004972)			
Post 1985 year *			0.0040798***		
Law school active in 1985			(0.0005876)		
Post 1990 year *				0.0026238***	
Law school active in 1990				(0.0007002)	
Post 1995 year					-0.0024264***
Law school active in 1995					(0.0007426)
Post Codignola reform year *	-0.0001773	0.0007736**	0.0016406***	0.0017555***	0.0003478
Law school active before reform	(0.0003331)	(0.0003798)	(0.0004517)	(0.0004968)	(0.0004140)
Observations	2,016	2,016	2,016	2,016	2,016
R <sup>2</sup>	0.3372809	0.3412751	0.3439693	0.3345773	0.3326727
Adjusted R <sup>2</sup>	0.2912001	0.2954720	0.2983535	0.2883085	0.2862714
Law school FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Linear trend	YES	YES	YES	YES	YES

<sup>36</sup>Instead, the negative and significant coefficient of the interaction “Post year 1995 \* Law school active in 1995” in Model 5 of Table 10 might capture the negative flexion in the ratio between total law graduates and Italian population we observe in Figure 4.

*Notes:* fixed effects panel data regressions. Time span: 1958 – 2014. Data sources of the number of law graduates for every public law school are ISTAT (Italian National Institute of Statistics) and MIUR (Italian Ministry of University Education and Research). For the case of those law schools established after 1958, we put a value equal to 0 until the first figure is observed. Heteroskedasticity - autocorrelation robust standard errors in parentheses. \* p<0.1; \*\* p<0.05; \*\*\* p<0.01.

## 5. Main results

To study the relation between law enforcement efficiency and generalized trust we rely on the reduced form below:

$$TRUST_{i,a,j,n} = \gamma_0 + \gamma_1 Z_{a,j} + \gamma_n + \beta' X_{i,a,j,n} + \gamma_j + u_{i,a,j,n} \quad (4)$$

where  $i$  indicates each respondent to the  $n$  ITANES survey (either 2001, 2008 or 2013), with her/his residency in city  $a$ , province  $j$ . As described before,  $TRUST_{i,a,j,n}$  takes a value of 1 if the respondent  $i$  says that most people can be trusted, 0 otherwise.  $Z_{a,j}$  is a dummy variable which takes value 1 if the closest public law school to municipality  $a$  in province  $j$  (in 1997) was already active before the Codignola reform of 1969. Similarly to Nunn and Wantchekon (2011),  $X_{i,a,j,n}$  is a vector of individual level controls containing information about gender, age, study level and employment status. These variables are also intended to control for individual income which has been shown to be related with trust. Although we do not have direct information about income of the respondents, occupation and education level can be considered good proxies (Nunn and Wantchekon, 2011).  $\gamma_j$  are province level fixed effects;  $\gamma_0$  is a constant term;  $\gamma_n$  are survey level fixed effects;  $u_{i,a,j,n}$  is the error term. As already mentioned, answers to the ITANES survey question about trust could suffer of some measurement errors. Thus, in line with Putnam (2000) we also implement the variable  $ASS.DENSITY_{a,j}$ , namely the number of nonprofit associations per inhabitant in each municipality  $a$ , province  $j$ , in 2001, as a further measure of trust. We modify Equation 4 accordingly:

$$ASS. DENSITY_{a,j} = \pi_0 + \pi_1 Z_{a,j} + \pi_j + u_{a,j} \quad (5)$$

in which  $\pi_0$  is a constant term;  $\pi_j$  are province level fixed effects;  $u_{a,j}$  are error terms<sup>37</sup>. We hope that by considering same cities and similar time period, both  $ASS. DENSITY_{a,j}$  and  $TRUST_{i,a,j,n}$  could be two different proxies of the same underlining phenomenon: generalized trust in municipality  $a$ <sup>38</sup>. Results are in Table 11 below:

Tab.11

*The Codignola reform effect on two different measures of generalized trust*

	<i>Dependent variable:</i>					
	Individual level trust				2001 nonprofit ass. density	
	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	<i>OLS</i>
	(1)	(2)	(3)	(4)	(5)	(6)
Closest law school active before Codignola reform	-0.028 (0.031)	-0.163 (0.180)	-0.026 (0.031)	-0.169 (0.183)	-0.001** (0.0002)	-0.001** (0.0002)
2001, 2008, 2013 survey FE	YES	YES	YES	YES		
Province FE	YES	YES	YES	YES	YES	YES
Individual level controls	NO	NO	YES	YES		
Resident population in 2001	NO	NO	YES	YES	NO	YES
Observations	6,621	6,621	6,580	6,580	1,626	1,626
R <sup>2</sup>	0.063		0.084		0.388	0.390
Adjusted R <sup>2</sup>	0.047		0.068		0.345	0.347
Akaike Inf. Crit.		6.976.665		6.806.532		

*Notes:* Models 1, 2, 3, 4 are OLS and logit estimations with individual level pooled data from ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys. Models 5, 6 are estimated at a municipal level only for the 2001 reference year. Municipalities in these two models correspond with those covered by the three ITANES surveys. The dependent variable in 5, 6 is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). The “resident population in 2001” regressor is intended to control for omitted variable bias problems, as suggested by the results of the balance tests

<sup>37</sup>We also include in some specifications of Equations 4 and 5 the variable “Resident population in 2001” of each municipality  $a$ , province  $j$  to control for possible problems of omitted variables. Indeed, resident population before the Codignola reform has been shown to be potentially correlated with our dummy  $Z_{a,j}$  by our balance tests. See Section 4.2 for details.

<sup>38</sup>Results from Equation 5 calculated for all Italian municipalities can be found in Tables A.14 and A.15 in Appendix.

in Section 4.2. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

The sign of all six coefficients is in line with our predictions: municipalities closer to law schools already active before the Codignola reform seem to witness lower levels of interpersonal trust. In our view, this pattern might be due to a systematic higher density of lawyers. According to the revised theory, this density in turn interferes with the average efficiency of the closest tribunals, leading people to develop heuristics of more mistrust. Unfortunately, only the coefficients in Models 5 and 6 are statistically significant: when  $Z_{a,j}$  equals 1, non-profit associations drop by 1 unit per 1000 inhabitants. However, our results seem not conclusive.

At this point, we try to make a step further. We recall that  $Z_{a,j}$  attempts to map systematic differences of the lawyer density distribution over the Italian territory in the early 2000s - brought about by the Codignola reform. Indeed, we show the push to this density after the reform being credibly stronger in those cities closer to law schools in activity at that time. However, we expect this difference among cities closer to faculties active either before or after 1969 fading away as the distance from these schools increases: in fact, as a faculty gets farer and farer, costs to study in there become prohibiting. Therefore, it sounds realistic to think that the effects of the reform should be decreasing with distance, up to a point in which there is no difference between cities being at the same distance  $x$  from a university active either before or after the Codignola reform. And this fact due to prohibiting distance costs. Therefore, we modify Equations 4 and 5 as follows:

$$TRUST_{i,a,j,n} = \gamma_0 + \gamma_1 Z_{a,j} + \gamma_2 x_{a,j} + \gamma_3 x_{a,j} Z_{a,j} + \beta' X_{i,a,j,n} + \gamma_n + \gamma_j + u_{i,a,j,n} \quad (6)$$

$$ASS. DENSITY_{a,j} = \pi_0 + \pi_1 Z_{a,j} + \pi_2 x_{a,j} + \pi_3 x_{a,j} Z_{a,j} + \pi_j + u_{a,j} \quad (7)$$

where  $x_{a,j}$  is the distance in kilometres of town  $a$ , province  $j$  from its closest law school.  $x_{a,j} Z_{a,j}$  is the interaction between this distance and

our dummy. However, there is a caveat. We already stressed that it is very likely the case of variable  $x_{a,j}$  being correlated with other factors, which are very difficult to control for in an econometric specification. Indeed, universities are not randomly localized over a territory. On the other hand, in Section 4.2 we also notice that thinking our dummy  $Z_{a,j}$  as exogenous to law efficiency and trust determinants is fairly plausible. According to asymptotic results in Nizalova and Murtazashvili (2014) and Bun and Harrison (2019), exogeneity of  $Z_{a,j}$  is a sufficient condition for the coefficient of  $x_{a,j}Z_{a,j}$  to be consistently estimated by OLS in a linear model. Also inference is reliable. In Table 12 we present the results coming from Equations 6 and 7. The sign of the coefficients are in line with our predictions: the intercept  $Z_{a,j}$  is always negative, indicating less trust in towns closer to universities active at the time of the reform in 1969. It becomes significant once we control for the resident population in 2001 in Models 3, 4 - we insert this variable in accordance with the results from our balance tests.  $Z_{a,j}$  is always significant when considering Models 5, 6 with nonprofit association density as the dependent variable. According to our estimates, when  $Z_{a,j}$  equals 1 the probability of answering that most people can be trusted in one of the three ITANES surveys decreases by around 8%. In other words, in those areas with a higher density of lawyers people seem to develop lower levels of interpersonal trust. Moreover, the positive and significant difference in slope signalled by the coefficient of the interaction  $x_{a,j}Z_{a,j}$  gives us a clear indication that, as we get farther from a law school, the systematic difference of generalized trust among towns - indicated by the coefficient of  $Z_{a,j}$  - tends to disappear. Probability goes up by 0.1% on average for every kilometre away from a law school active at the time of the Codignola reform. Turning to the results of the models with  $ASS. DENSITY_{a,j}$ , when  $Z_{a,j}$  equals 1, nonprofit associations drop by 1 unit per 1000 inhabitants. Then, they increase at a rate equal to 0.02 units per 1000 inhabitants every kilometre. In our context, we believe the channel in action being the negative relation between the density of operating lawyers and efficiency of the courts: effects of the Codignola reform in terms of lawyer density changes were likely milder for cities distant from law schools. Then, *ceteris paribus*



distant tribunals should work at a similar efficiency level. As  $x_{a,j}$  increases, also the difference in trust between the two groups of cities signalled by  $Z_{a,j}$  should vanish. And this is what we observe in the data.

Overall, we read our results as a possible piece of evidence supporting theories about complementarities between a pro-social attitude of agents and an institutional environment in which more cooperation is sustained (e.g. Tabellini, 2008; Nunn and Watchenkon, 2011).

Tab.12

*The Codignola reform effect on two different measures of generalized trust*

	<i>Dependent variable:</i>					
	Individual level				2001 nonprofit ass.	
	trust				density	
	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Distance from closest law school (km)	-0.001 (0.001)	-0.007 (0.004)	-0.001 (0.001)	-0.004 (0.005)	0.00001 (0.00001)	0.00001 (0.00001)
Closest law school active before Codignola reform	-0.070 (0.062)	-0.427 (0.314)	-0.086* (0.050)	-0.572* (0.330)	-0.001*** (0.0004)	-0.001*** (0.0004)
Interaction	0.001 (0.001)	0.005 (0.005)	0.001 (0.001)	0.008 (0.005)	0.00002*** (0.00001)	0.00002*** (0.00001)
2001, 2008, 2013 survey FE	YES	YES	YES	YES		
Province FE	YES	YES	YES	YES	YES	YES
Individual level controls	NO	NO	YES	YES		
Resident population in 2001	NO	NO	YES	YES	NO	YES
Observations	6,620	6,620	6,579	6,579	1,625	1,625
R <sup>2</sup>	0.063		0.085		0.407	0.412
Adjusted R <sup>2</sup>	0.048		0.068		0.365	0.370
Akaike Inf. Crit.		6,973.533		6,803.357		

*Notes:* Models 1, 2, 3, 4 are OLS and logit estimations with individual level pooled data from ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys. Models 5, 6 are estimated at a municipal level only for the 2001 reference year. Municipalities in these two models correspond with those covered by the three ITANES surveys. The dependent variable in 5, 6 is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). The “resident population in 2001” regressor is intended to control for omitted variable bias problems, as suggested by the results of the balance tests in Section 4.2. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

## 6. Channel

The relation between lawyer density and law enforcement efficiency is crucial for our identification strategy. Indeed, we think this channel being the most likely explanation behind our main results presented in Section 5. Although we already revised past empirical research in its favour, we also run some linear models to test if this relation holds right before the year 2001, timing we start to observe our proxies for generalized trust<sup>39</sup>. Confirming the connection between lawyer density and law enforcement efficiency could be another piece of evidence in favour of our theory: if the Codignola reform had an effect on lawyer density, it would probably transmit also to justice efficiency.

To the best of our knowledge there are no readily available data about the density of Italian operating lawyers and their locations, especially for years back in the past. To overcome this problem, we use again a proxy that we define as follows: for every *albo degli avvocati* we take the number of lawyers enrolled at time  $t$ , divided by the resident population of corresponding town  $i$  of the *albo* for the same year  $t$ . Our assumption is that this ratio could represent a good approximation of the operating lawyer density of that city. As before, for the efficiency level of close tribunals we use the average length (in days) of first degree civil trials of the court district a town belongs to, calculated with the CEPEJ formula. We consider three reference years: 1995, 1997, 1999. In Table 13 below we present our estimations. Results from this set of regressions seem to confirm what inferred by past research: in Italy there has been a positive relation between lawyer density and the length of civil trials. And in accordance with the literature, one of the reasons behind this phenomenon could be an opportunistic behaviour of lawyers, whose the incentive is to inflate their emoluments by more judicial activity. *Ceteris paribus*, this inflated demand for justice translates into an increase in the time needed for the resolution of

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<sup>39</sup>In appendix, Table A.13, we also report results from the same linear models, but considering law enforcement efficiency in 2001, 2008, 2012 (namely the ITANES time span, but not until 2013 due to data constraints). Also for the case of these years we find confirmation for the negative relation between lawyer density and law enforcement efficiency underlined by the literature.

disputes due to a greater workload for tribunals (Finocchiaro Castro and Guccio, 2014).

Tab.13  
*Effects of lawyer density on first degree civil trial length*

	<i>Dependent variable:</i>					
	Civil trial length					
	(n. of days - CEPEJ formula)					
	(1995)		(1997)		(1999)	
	(1)	(2)	(3)	(4)	(5)	(6)
Lawyer density (1995)	12,501.290** (5,601.662)	11,973.650** (5,014.336)				
Lawyer density (1997)			13,232.460*** (2,741.910)	12,633.810*** (2,699.153)		
Lawyer density (1999)					5,014.445** (2,474.144)	5,957.986** (2,353.688)
5 macro regions FE (North-east/west, Center, South, Islands)	YES	NO	YES	NO	YES	NO
3 macro regions FE (North, Center, South)	NO	YES	NO	YES	NO	YES
Observations	143	143	143	143	143	143
Adjusted R <sup>2</sup>	0.455	0.475	0.523	0.540	0.461	0.459

*Notes:* OLS estimations for the following reference years: 1995, 1997, 1999. The dependent variable is the average length (in days) of first degree civil trials in each of the 26 Italian court districts, calculated with CEPEJ (European Commission for the Efficiency of Justice) disposition time formula. Data come from bulletins reporting annual statistics of the Italian justice system (*Annuario di statistiche giudiziarie*), available in ISTAT (Italian National Institute of Statistics) library. Lawyer density corresponds to the number of lawyers enrolled in the *albi degli avvocati* (city-level lawyer registers), divided by the resident population of the corresponding town  $i$  for the considered year. The 5 macro regions correspond to NUTS 1 Italian areas. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Finally, we try to shed further light on the connection between the effects of the Codignola reform on law degree holders, lawyer density and the performance of tribunals. To do this, we check if our proxy for the provincial density of graduates in law in 1997 - that we have already shown in Section 4.3 being correlated with our measure for provincial lawyer density - could also explain law enforcement efficiency of close

future years. This in force of the fact that some of these graduates are (or are going to be) operating lawyers likely to “stress” tribunals in these provinces with their activity. According to our results in Table 14 the density of law degree holders strongly predict the efficiency of tribunals in their provinces<sup>40</sup>. We interpret them as a further reassurance about the plausibility of the mechanisms we put forward to explain the negative relation between law enforcement efficiency and trust.

Tab.14

*Relation between law graduate density in 1997 and law enforcement efficiency from 2001 to 2013*

<i>Dependent variable:</i>			
First degree civil trial length (num. of days)			
	(1)	(2)	(3)
	(2001 - 2004)	(2008 - 2013)	(All)
Law graduate density proxy (1997)	2,858.039*** (941.763)	1,990.007*** (454.350)	2,418.653*** (496.119)
Regional FE	YES	YES	YES
Year FE	YES	YES	YES
Data source FE	NO	NO	YES
Observations	84	120	204
R <sup>2</sup>	0.703	0.857	0.832
Adjusted R <sup>2</sup>	0.620	0.828	0.809

*Notes:* OLS estimations with pooled data. The dependent variable is the average length (in days) of first degree civil trials in each of the 26 Italian court districts, calculated with CEPEJ (European Commission for the Efficiency of Justice) disposition time formula. For year 2001, data come from bulletins reporting annual statistics of the Italian justice system (*Annuario di statistiche giudiziarie*), available in ISTAT (Italian National Institute of Statistics) library. For years 2008 to 2013 data for calculations are downloadable from the Italian Ministry of Justice website ([www.giustizia.it/giustizia](http://www.giustizia.it/giustizia)). Law graduate density proxy (1997) is obtained by summing (by province) all law degrees issued by each public university

<sup>40</sup>In Table 14 we show estimations coming from three pooled regressions. In Tables A.18 and A.19 in Appendix we report instead results year by year.

from 1958 to 1997; this figure is then divided by the population of the corresponding province in 1997 (data source: ISTAT). Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

## 7. Robustness

We now perform a battery of robustness checks to see if our main results in Section 5 are driven by unobservable factors. For those models using the nonprofit association density in 2001 as dependent variable we only report the results obtained for the sample of cities in the 2001, 2008 and 2013 ITANES surveys as well. Estimations using all Italian municipalities can be found in Tables A.14 to A.17 in Appendix<sup>41</sup>.

### 7.1. Dropping relevant municipalities: cities with a law school in 1997 and provincial capitals

Cities with a law school might differ from the rest of the sample. For example, faculties are usually established in more developed urban areas. We could also argue that law schools might be founded in towns with higher levels of criminal activity and then, less trust on average. Thus, a priori we cannot exclude that factors which are proper of these territories could influence our results. To check if this is the case, we estimate our models without the group of municipalities with a law school in activity in 1997. Calculations from our econometric specifications are presented in Tables 15 and 16.

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<sup>41</sup>In this case we find weaker evidence when considering the simpler regressions without interaction between  $Z_{a,j}$  and distance of municipality  $a$  from its closest law school. Instead, our theory seems to find again support when running those models with the interaction term as well.

Tab.15

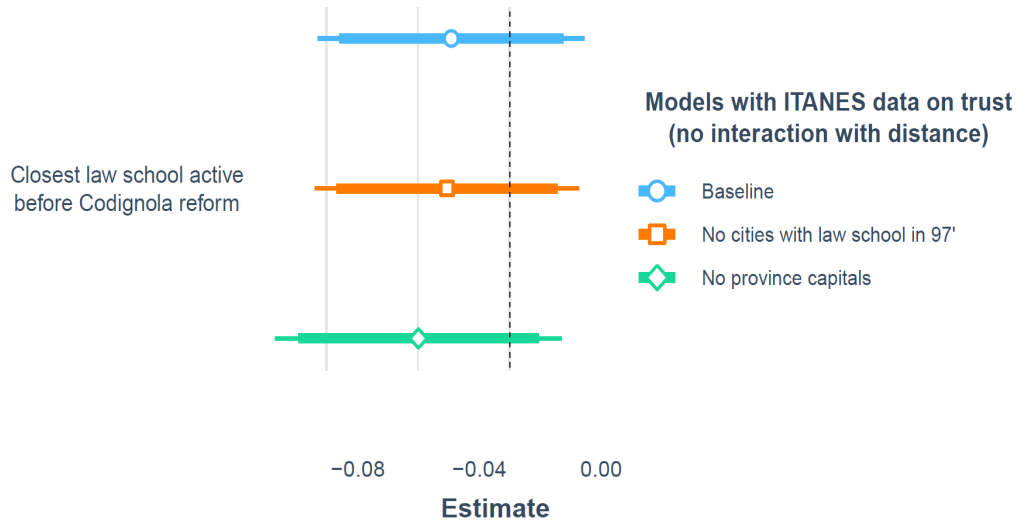
*The Codignola reform effect on two different measures of generalized trust – no cities with a law school active in 1997*

	<i>Dependent variable:</i>					
	Individual level				2001 nonprofit ass.	
	trust				density	
	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Closest law school active before Codignola reform	-0.030 (0.031)	-0.168 (0.181)	-0.027 (0.031)	-0.169 (0.185)	-0.001** (0.0002)	-0.001** (0.0002)
2001, 2008, 2013 survey FE	YES	YES	YES	YES		
Province FE	YES	YES	YES	YES	YES	YES
Individual level controls	NO	NO	YES	YES		
Resident population in 2001	NO	NO	YES	YES	NO	YES
Observations	5,294	5,294	5,262	5,262	1,593	1,593
R <sup>2</sup>	0.076		0.096		0.392	0.395
Adjusted R <sup>2</sup>	0.057		0.076		0.349	0.352
Akaike Inf. Crit.		5,443.720		5,319.804		

*Notes:* Models 1, 2, 3, 4 are OLS and logit estimations with individual level pooled data from ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys. Models 5, 6 are estimated at a municipal level, only for the 2001 reference year. Municipalities in these two models correspond with those covered by the three ITANES surveys. The dependent variable in 5, 6 is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). For each sample we do not consider cities with a law school in activity in 1997. The “resident population in 2001” regressor is intended to control for omitted variable bias problems, as suggested by the results of the balance tests in Section 4.2. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Outcomes in Table 15 are similar to our main results obtained with the entire sample, both in terms of magnitude and significance. This analogy is also appreciable in both Figures 7 and 8; here we compare the coefficients from Model 3 and Model 6 of our robustness test (indicated in colour orange) with those obtained from the corresponding specifications in the Main Results section (indicated in colour blue).

Fig.7  
*Comparison of coefficients*

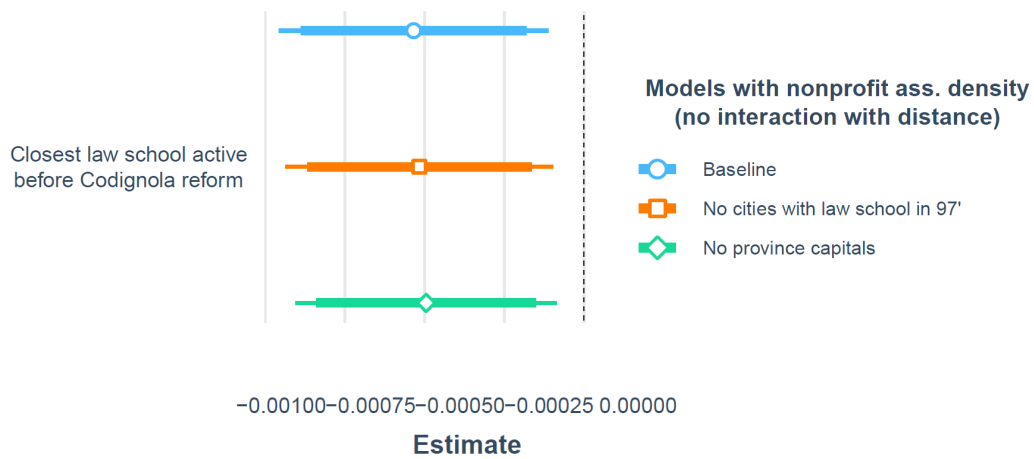


*Notes:* OLS estimations for the coefficient of the dummy variable “Closest law school active before Codignola reform”. Dependent variable is the individual level trust in ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys. 90% (ticker) and 95% confidence intervals using robust standard errors are shown. Results in blue come from the main specification in Section 5, Model 3, Table 11. Results in orange come from the same specification but dropping cities with a law school active in 1997 from the sample (i.e. Model 3, Table 15 of this section). In green, results dropping this time provincial capitals from the sample (i.e. Model 3, Table 17 of this section).

Sharper differences can be noticed when running the models augmented by the interaction terms between  $Z_{a,j}$  and distance of municipalities from their closest law schools. Results are shown in Table 16. In accordance with our theory, both the dummy and its interaction with the distance from the closest law school for each municipality have the expected signs: negative for the former, positive for the latter. However, differently from before the coefficients are significant also for all Models 1 to 4 with individual level trust as dependent variable. As well, their magnitudes increase by about 50% in absolute terms. On the contrary, no big differences can be noticed for the case of Model

5 and 6 with nonprofit association density as dependent variable. As before, we make our results easy to interpret by comparing them in Figures 9 and 10 (in orange) with our main results from Section 5 (in blue).

Fig.8  
*Comparison of coefficients*



*Notes:* OLS estimations for the coefficient of the dummy variable “Closest law school active before Codignola reform”. Dependent variable is municipal level trust calculated as the number of nonprofit associations per inhabitant for each municipality in the sample. Data source is ISTAT (Italian National Institute of Statistics). 90% (ticker) and 95% confidence intervals using robust standard errors are shown. Results in blue come from the main specification in Section 5, Model 6, Table 11. Results in orange come from the same specification but dropping cities with a law school active in 1997 from the sample (i.e. Model 6, Table 15 of this section). In green, results dropping this time provincial capitals from the sample (i.e. Model 6, Table 17 of this section).



Tab.16

*The Codignola reform effect on two different measures of generalized trust – no cities with a law school active in 1997*

	Dependent variable:					
	Individual level				2001 nonprofit ass.	
	trust				density	
	OLS	logistic	OLS	logistic	OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Distance from closest law school (km)	-0.002*** (0.001)	-0.014** (0.006)	-0.002*** (0.001)	-0.014** (0.005)	0.00001** (0.00001)	0.00001** (0.00001)
Closest law school active before Codignola reform	-0.187*** (0.062)	-1.180*** (0.411)	-0.183*** (0.055)	-1.231*** (0.374)	-0.001*** (0.0004)	-0.001*** (0.0004)
Interaction	0.003*** (0.001)	0.020*** (0.006)	0.003*** (0.001)	0.021*** (0.006)	0.00002*** (0.00001)	0.00002*** (0.00001)
2001, 2008, 2013 survey FE	YES	YES	YES	YES		
Province FE	YES	YES	YES	YES	YES	YES
Individual level controls	NO	NO	YES	YES		
Resident population in 2001	NO	NO	YES	YES	NO	YES
Observations	5,294	5,294	5,262	5,262	1,593	1,593
R <sup>2</sup>	0.078		0.098		0.420	0.425
Adjusted R <sup>2</sup>	0.059		0.077		0.379	0.383
Akaike Inf. Crit.		5,438.043		5,313.205		

*Notes:* Models 1, 2, 3, 4 are OLS and logit estimations with individual level pooled data from ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys. Models 5, 6 are estimated at a municipal level only for the 2001 reference year. Municipalities in these two models correspond with those covered by the three ITANES surveys. The dependent variable in 5, 6 is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). For each sample we do not consider cities with a law school in activity in 1997. The “resident population in 2001” regressor is intended to control for omitted variable bias problems, as suggested by the results of the balance tests in Section 4.2. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

In our next robustness test we drop instead the *capoluoghi di provincia* (provincial capitals) from the entire sample. Again, these cities might systematically differ in many socio-economic aspects from the rest of the sample as they are, in general, the most relevant urban areas of each province. In turn, these factors could also affect our estimates since some law schools are localized in these towns. Nevertheless, conclusions from the models in Table 17 remain very similar to our main

estimations. See also Figure 7 and 8 for further details (results are highlighted in green). Moreover, results from the augmented models in Table 18 lead to similar conclusions of the previous robustness check: coefficients are significant for all specifications and their magnitudes increase in absolute terms for the case of Models 1 to 4. On the contrary, no evident changes with respect to our main estimations are detected when turning to the last two models. A visual comparison of the results can be found in Figures 9 and 10.

Tab.17

*The Codignola reform effect on two different measures of generalized trust – no provincial capitals*

	<i>Dependent variable:</i>					
	Individual level trust				2001 nonprofit ass. density	
	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	<i>OLS</i>
	(1)	(2)	(3)	(4)	(5)	(6)
Closest law school active before Codignola reform	-0.040 (0.033)	-0.234 (0.204)	-0.040 (0.034)	-0.254 (0.210)	-0.001*** (0.0002)	-0.0005** (0.0002)
2001, 2008, 2013 survey FE	YES	YES	YES	YES		
Province FE	YES	YES	YES	YES	YES	YES
Individual level controls	NO	NO	YES	YES		
Resident population in 2001	NO	NO	YES	YES	NO	YES
Observations	4,390	4,390	4,365	4,365	1,519	1,519
R <sup>2</sup>	0.079		0.095		0.401	0.415
Adjusted R <sup>2</sup>	0.056		0.070		0.356	0.371
Akaike Inf. Crit.		4,431.299		4,355.330		

*Notes:* Models 1, 2, 3, 4 are OLS and logit estimations with individual level pooled data from ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys. Models 5, 6 are estimated at a municipal level only for the 2001 reference year. Municipalities in these two models correspond with those covered by the three ITANES surveys. The dependent variable in 5, 6 is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). For each sample we do not consider provincial capitals. The “resident population in 2001” regressor is intended to control for omitted variable bias problems, as suggested by the results of the balance tests in Section 4.2. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

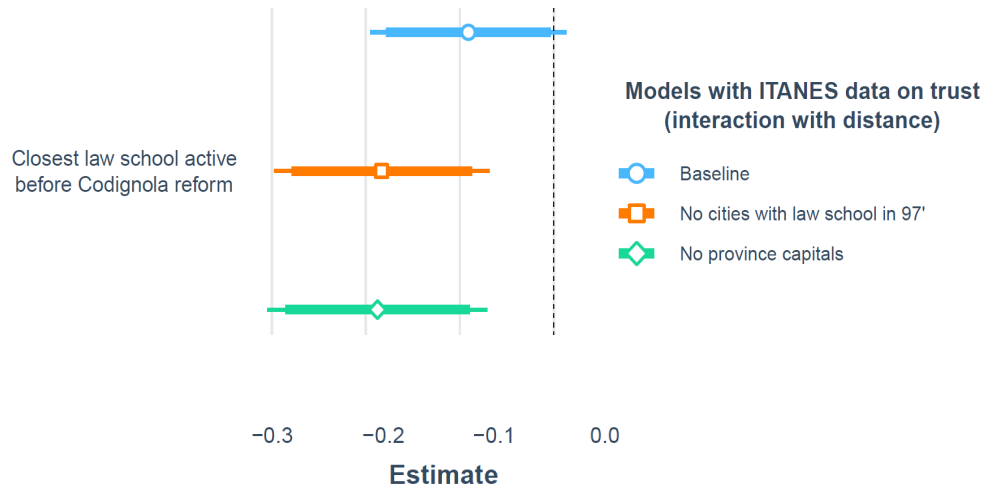
Tab.18

*The Codignola reform effect on two different measures of generalized trust – no provincial capitals*

	<i>Dependent variable:</i>					
	Individual level trust				2001 nonprofit ass. density	
	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Distance from closest law school (km)	-0.002*** (0.001)	-0.016*** (0.006)	-0.002*** (0.001)	-0.016*** (0.006)	0.00001** (0.00000)	0.00001* (0.00000)
Closest law school active before Codignola reform	-0.192*** (0.056)	-1.266*** (0.392)	-0.188*** (0.056)	-1.302*** (0.403)	-0.001*** (0.0003)	-0.001*** (0.0003)
Interaction	0.003*** (0.001)	0.021*** (0.007)	0.003*** (0.001)	0.021*** (0.007)	0.00002*** (0.00001)	0.00002*** (0.00001)
2001, 2008, 2013 survey FE	YES	YES	YES	YES		
Province FE	YES	YES	YES	YES	YES	YES
Individual level controls	NO	NO	YES	YES		
Resident population in 2001	NO	NO	YES	YES	NO	YES
Observations	4,389	4,389	4,364	4,364	1,518	1,518
R <sup>2</sup>	0.081		0.097		0.433	0.443
Adjusted R <sup>2</sup>	0.058		0.071		0.390	0.400
Akaike Inf. Crit.		4,421.783		4,346.183		

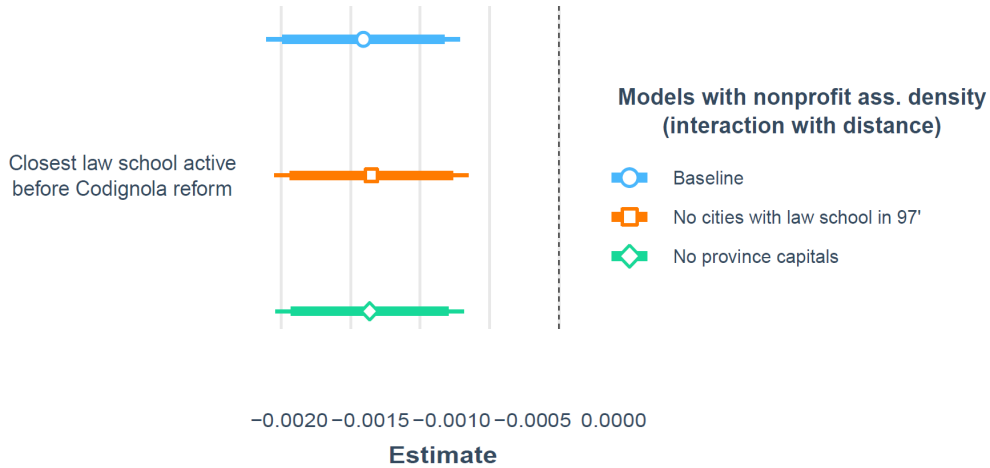
*Notes:* Models 1, 2, 3, 4 are OLS and logit estimations with individual level pooled data from ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys. Models 5, 6 are estimated at a municipal level only for the 2001 reference year. Municipalities in these two models correspond with those covered by the three ITANES surveys. The dependent variable in 5, 6 is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). For each sample we do not consider provincial capitals. The “resident population in 2001” regressor is intended to control for omitted variable bias problems, as suggested by the results of the balance tests in Section 4.2. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Fig.9  
*Comparison of coefficients*



*Notes:* OLS estimations for the coefficient of the dummy variable “Closest law school active before Codignola reform”. Dependent variable is individual level trust in ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys. 90% (ticker) and 95% confidence intervals using robust standard errors are shown. Results in blue come from the main specification in Section 5, Model 3, Table 12 with the distance from closest law school for each town in the sample. Results in orange come from the same specification but dropping cities with a law school active in 1997 from the sample (i.e. Model 3, Table 16 of this section). In green, results dropping this time provincial capitals from the sample (i.e. Model 3, Table 18 of this section).

Fig.10  
*Comparison of coefficients*



*Notes:* OLS estimations for the coefficient of the dummy variable “Closest law school active before Codignola reform”. Dependent variable is municipal level trust calculated as the number of nonprofit associations per inhabitant for each municipality in the sample. Data source is ISTAT (Italian National Institute of Statistics). 90% (ticker) and 95% confidence intervals using robust standard errors are shown. Results in blue come from the main specification in Section 5, Model 6, Table 12 with distance from closest law school for each town in the sample. Results in orange come from the same specification but dropping cities with a law school active in 1997 from the sample (i.e. Model 6, Table 16 of this section). In green, results dropping this time provincial capitals from the sample (i.e. Model 6, Table 18 of this section).

## 7.2. Distance from provincial capitals and other types of faculties

We now perform two other types of robustness tests. Since universities are usually established in relevant municipalities, it might be also possible that the dummy  $Z_{a,j}$  - that we recall being equal to 1 if the closest law school to municipality  $a$  in province  $j$  was already in function in 1969 - could correlate with omitted variables which depend instead on the distance from the main cities of our sample. To check for this issue,

we firstly run our main models from Section 5, augmented this time by the distance of each municipality  $a$  from its closest provincial capital as well. Estimations are presented in Tables 19 and 20.

Tab.19

*The Codignola reform effect on two different measures of generalized trust – with distance from closest provincial capitals*

	<i>Dependent variable:</i>					
	Individual level				2001 nonprofit ass.	
	trust				density	
	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	
(1)	(2)	(3)	(4)	(5)	(6)	
Closest law school active before Codignola reform	-0.036 (0.031)	-0.206 (0.182)	-0.028 (0.031)	-0.185 (0.184)	-0.0005** (0.0002)	-0.0005** (0.0002)
2001, 2008, 2013 survey FE	YES	YES	YES	YES		
Province FE	YES	YES	YES	YES	YES	YES
Distance from closest <i>capoluogo di provincia</i>	YES	YES	YES	YES	YES	YES
Individual level controls	NO	NO	YES	YES		
Resident population in 2001	NO	NO	YES	YES	NO	YES
Observations	6,606	6,606	6,565	6,565	1,624	1,624
R <sup>2</sup>	0.064		0.084		0.390	0.393
Adjusted R <sup>2</sup>	0.049		0.067		0.347	0.350
Akaike Inf. Crit.		6,940.765		6,784.212		

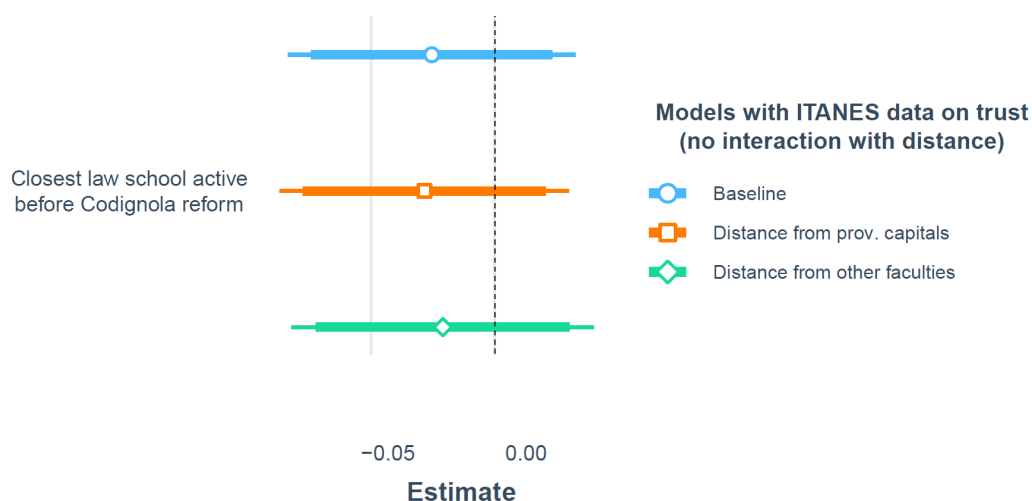
*Notes:* Models 1, 2, 3, 4 are OLS and logit estimations with individual level pooled data from ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys. Models 5, 6 are estimated at a municipal level only for the 2001 reference year. Municipalities in these two models correspond with those covered by the three ITANES surveys. The dependent variable in 5, 6 is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). Models are augmented by the distance of each municipality from the closest provincial capital. The “resident population in 2001” regressor is intended to control for omitted variable bias problems, as suggested by the results of the balance tests in Section 4.2. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Conclusions from Table 19 are in line with our main results in Section 5. As before, we make these patterns observable also in Figures 11 and 12 where we compare the coefficients obtained for our dummy variable

$Z_{a,j}$  in the robustness test (in orange) and from our main specifications (in blue). Very similar estimations are also observable in Table 20: the magnitude of coefficients is almost identical to those presented in the Main Results section. However, the effects for  $Z_{a,j}$  in Models 3 and 4 are, in this case, not significant.

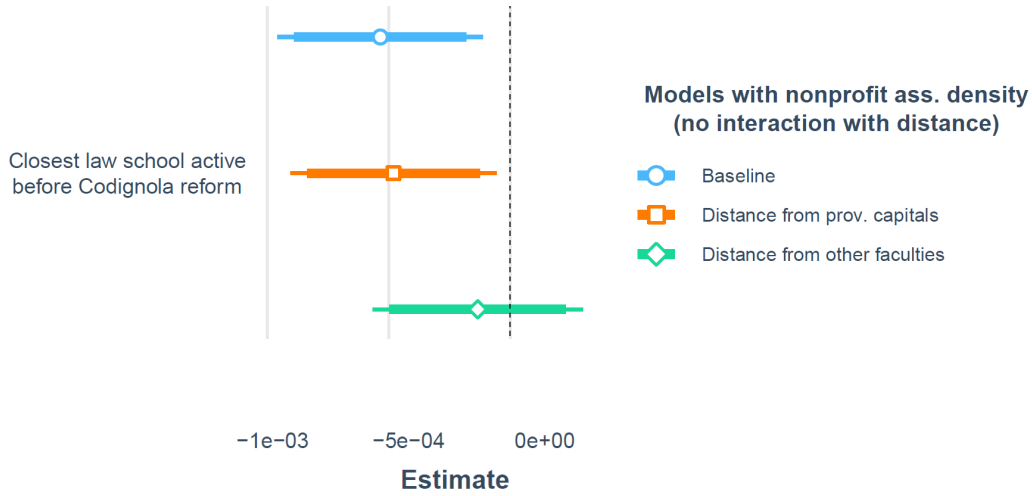
Fig.11

*Comparison of coefficients*



*Notes:* OLS estimations for the coefficient of the dummy variable “Closest law school active before Codignola reform”. Dependent variable is individual level trust in ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys. 90% (ticker) and 95% confidence intervals using robust standard errors are shown. Results in blue come from the main specification in Section 5, Model 3, Table 11. Results in orange come from the same specification but adding the distance of each respondent’s municipality from the closest provincial capital (i.e. Model 3, Table 19 of this section). In green, results adding the distance of each respondent’s municipality from the closest faculty in economics, medicine, engineering and mathematics already in activity at the time of the Codignola reform (i.e. Model 3, Table 21 of this section).

Fig.12  
*Comparison of coefficients*



*Notes:* OLS estimations for the coefficient of the dummy variable “Closest law school active before Codignola reform”. Dependent variable is municipal level trust calculated as the number of nonprofit associations per inhabitant for each municipality in the sample. Data source is ISTAT (Italian National Institute of Statistics). 90% (ticker) and 95% confidence intervals using robust standard errors are shown. Results in blue come from the main specification in Section 5, Model 6, Table 11. Results in orange come from the same specification but adding the distance of each respondent’s municipality from the closest provincial capital (i.e. Model 6, Table 19 of this section). In green, results adding the distance of each respondent’s municipality from the closest faculty in economics, medicine, engineering and mathematics already in activity at the time of the Codignola reform (i.e. Model 6, Table 21 of this section).

We then repeat the calculations by considering instead the distance of each city in our sample from its closest faculty of economics, medicine, mathematics and engineering already active at the time of the Codignola reform<sup>42</sup>. The scope of this exercise is twofold. We try to control for factors correlated with the distance from the urban areas where the

<sup>42</sup>For this purpose, we first calculated the distance in kilometres of town *a* from every public school in economics, medicine, mathematics and engineering active in 1969. Then, for each category we selected the faculty at the minimum distance.



aforementioned universities are located. As well, we also try to control for other long lasting effects of the Codignola reform that could drive our results. Indeed, we recall that in 1969 the new legislation lowered the entry barriers for all types of faculties, not only for law schools. Thus, in theory we could expect a similar push to the number of graduates we documented for law schools also happening for the case of other universities. As long as the distance from schools active at the time of the reform is also a proxy for the density of their graduates in more recent years, we could try to gain further confidence about the reliability of the channel we think is driving our main results: lawyers and not other types of degree holders are behind our estimates for the negative effects of law enforcement efficiency on trust (and this, in force of the fact that more operating lawyers slow down courts).

Tab.20

*The Codignola reform effect on two different measures of generalized trust – with distance from closest provincial capitals*

	<i>Dependent variable:</i>					
	Individual level trust				2001 nonprofit ass. density	
	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Distance from closest law school (km)	0.0001 (0.001)	0.001 (0.005)	0.0001 (0.001)	0.002 (0.005)	0.00001 (0.00001)	0.00001 (0.00001)
Closest law school active before Codignola reform	-0.063 (0.049)	-0.415 (0.324)	-0.077 (0.050)	-0.524 (0.337)	-0.001*** (0.0004)	-0.001*** (0.0004)
Interaction	0.0005 (0.001)	0.004 (0.005)	0.001 (0.001)	0.006 (0.005)	0.00002*** (0.00001)	0.00002*** (0.00001)
2001, 2008, 2013 survey FE	YES	YES	YES	YES		
Province FE	YES	YES	YES	YES	YES	YES
Distance from closest <i>capoluogo di provincia</i>	YES	YES	YES	YES	YES	YES
Individual level controls	NO	NO	YES	YES		
Resident population in 2001	NO	NO	YES	YES	NO	YES
Observations	6,605	6,605	6,564	6,564	1,623	1,623
R <sup>2</sup>	0.065		0.085		0.408	0.412
Adjusted R <sup>2</sup>	0.049		0.068		0.365	0.369
Akaike Inf. Crit.		6,937.650		6,778.769		

*Notes:* Models 1, 2, 3, 4 are OLS and logit estimations with individual level pooled data from ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys.

Models 5, 6 are estimated at a municipal level only for the 2001 reference year. Municipalities in these two models correspond with those covered by the three ITANES surveys. The dependent variable in 5, 6 is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). Models are augmented by the distance of each municipality from the closest provincial capital. The “resident population in 2001” regressor is intended to control for omitted variable bias problems, as suggested by the results of the balance tests in Section 4.2. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Again, results from Models 1 to 4 in Table 21 are comparable with those obtained from our reference models in Section 5. Instead, coefficients in Models 5 and 6 lose their significance and reduce value substantially. Comparison of the estimates can be also found in Figures 11 and 12 as well (shown in green). More reassuring results are obtained when considering the specifications in which  $Z_{a,j}$  interacts with the distance from the closest law school. Estimations can be found in Table 22. The magnitude of the coefficients is similar to our reference results and signs are in line with what we expect from our theory. This analogy is also appreciable in Figures 13 and 14. As well, coefficients for the variables of interest are all statistically significant.

On the one hand, our robustness checks suggest we need to be careful: both size and significance of the effects of law enforcement efficiency on our proxies for trust are unclear. Therefore, we cannot completely exclude that also other factors which we are not able to account for are influencing in part our estimations. On the other hand, these tests do not disconfirm our main conclusions. In fact, we always find corroboration of a negative impact of our dummy for lawyer density on both proxies for generalized trust. Moreover this impact seems to vanish with distance, as expected.

Tab.21

*The Codignola reform effect on two different measures of generalized trust – with distance from closest schools of economics, medicine, engineering and mathematics*

	<i>Dependent variable:</i>					
	Individual level				2001 nonprofit ass.	
	trust				density	
	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Closest law school active before Codignola reform	-0.037 (0.032)	-0.215 (0.187)	-0.021 (0.032)	-0.135 (0.193)	-0.0002 (0.0002)	-0.0001 (0.0002)
2001, 2008, 2013 survey FE	YES	YES	YES	YES		
Province FE	YES	YES	YES	YES	YES	YES
Distance from closest Economics, Medicine, Math, Engineering schools	YES	YES	YES	YES	YES	YES
Individual level controls	NO	NO	YES	YES		
Resident population in 2001	NO	NO	YES	YES	NO	YES
Observations	6,605	6,605	6,564	6,564	1,623	1,623
R <sup>2</sup>	0.064		0.085		0.409	0.413
Adjusted R <sup>2</sup>	0.048		0.068		0.366	0.370
Akaike Inf. Crit.		6,951.469		6,784.061		

*Notes:* Models 1, 2, 3, 4 are OLS and logit estimations with individual level pooled data from ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys. Models 5, 6 are estimated at a municipal level only for the 2001 reference year. Municipalities in these two models correspond with those covered by the three ITANES surveys. The dependent variable in 5, 6 is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). Models are augmented by the distance of each municipality from the closest university of economics, medicine, engineering and mathematics already in activity when the Codignola reform was promulgated. The “resident population in 2001” regressor is intended to control for omitted variable bias problems, as suggested by the results of the balance tests in Section 4.2. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

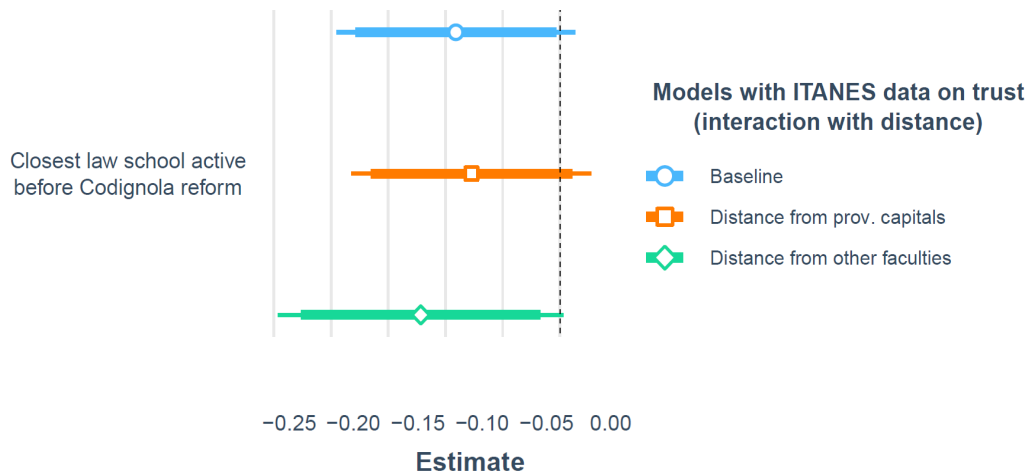
Tab.22

*The Codignola reform effect on two different measures of generalized trust – with distance from closest schools of economics, medicine, engineering and mathematics*

	<i>Dependent variable:</i>					
	Individual level				2001 nonprofit ass.	
	trust				density	
	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	<i>logistic</i>	<i>OLS</i>	
(1)	(2)	(3)	(4)	(5)	(6)	
Distance from closest law school (km)	-0.001 (0.001)	-0.004 (0.005)	-0.0001 (0.001)	-0.001 (0.005)	-0.00000 (0.00001)	-0.00000 (0.00001)
Closest law school active before Codignola reform	-0.123** (0.057)	-0.804** (0.391)	-0.122** (0.058)	-0.840** (0.403)	-0.001** (0.0004)	-0.001*** (0.0004)
Interaction	0.001* (0.001)	0.010* (0.006)	0.002* (0.001)	0.011* (0.006)	0.00001** (0.00001)	0.00002** (0.00001)
2001, 2008, 2013 survey FE	YES	YES	YES	YES		
Province FE	YES	YES	YES	YES	YES	YES
Distance from closest Economics, Medicine, Math, Engineering schools	YES	YES	YES	YES	YES	YES
Individual level controls	NO	NO	YES	YES		
Resident population in 2001	NO	NO	YES	YES	NO	YES
Observations	6,604	6,604	6,563	6,563	1,622	1,622
R <sup>2</sup>	0.064		0.086		0.411	0.416
Adjusted R <sup>2</sup>	0.048		0.068		0.368	0.373
Akaike Inf. Crit.		6,948.498		6,780.067		

*Notes:* Models 1, 2, 3, 4 are OLS and logit estimations with individual level pooled data from ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys. Models 5, 6 are estimated at a municipal level only for the 2001 reference year. Municipalities in these two models correspond with those covered by the three ITANES surveys. The dependent variable in 5, 6 is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). Models are augmented by the distance of each municipality from the closest university of economics, medicine, engineering and mathematics already in activity when the Codignola reform was promulgated. The “resident population in 2001” regressor is intended to control for omitted variable bias problems, as suggested by the results of the balance tests in Section 4.2. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

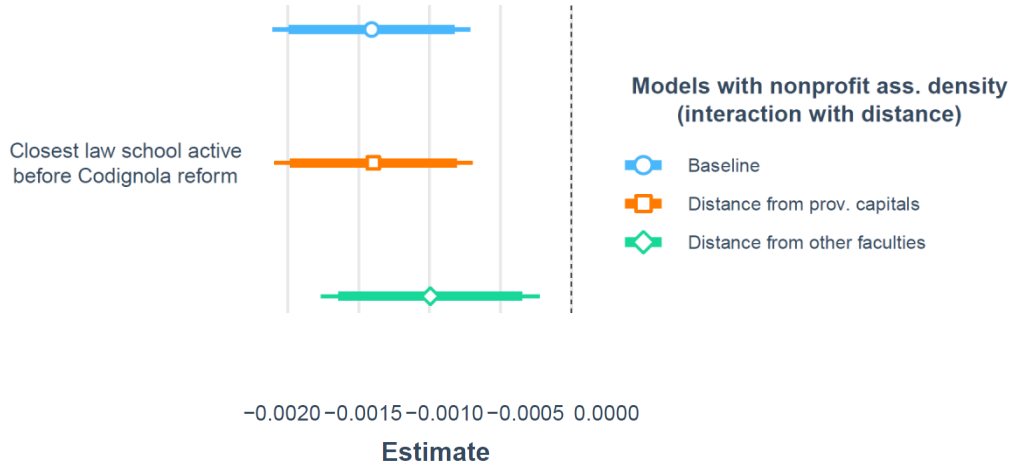
Fig.13  
*Comparison of coefficients*



*Notes:* OLS estimations for the coefficient of the dummy variable “Closest law school active before Codignola reform”. Dependent variable is individual level trust in ITANES (Italian National Election Studies) 2001, 2008, 2013 surveys. 90% (ticker) and 95% confidence intervals using robust standard errors are shown. Results in blue come from the main specification in Section 5, Model 3, Table 12 with distance from closest law school for each town in the sample. Results in orange come from the same specification but adding the distance of each respondent’s municipality from the closest provincial capital (i.e. Model 3, Table 20 of this section). In green, results adding the distance of each respondent’s municipality from the closest faculty in economics, medicine, engineering and mathematics already in activity at the time of the Codignola reform (i.e. Model 3, Table 22 of this section).

Fig.14

*Comparison of coefficients*



*Notes:* OLS estimations for the coefficient of the dummy variable “Closest law school active before Codignola reform”. Dependent variable is municipal level trust calculated as the number of nonprofit associations per inhabitant for each municipality in the sample. Data source is ISTAT (Italian National Institute of Statistics). 90% (ticker) and 95% confidence intervals using robust standard errors are shown. Results in blue come from the main specification in Section 5, Model 6, Table 12 with distance from closest law school for each town in the sample. Results in orange come from the same specification but adding the distance of each respondent’s municipality from the closest provincial capital (i.e. Model 6, Table 20 of this section). In green, results adding the distance of each respondent’s municipality from the closest faculty in economics, medicine, engineering and mathematics already in activity at the time of the Codignola reform (i.e. Model 6, Table 22 of this section).

## 8. Conclusion

In this paper we tried to figure out if law enforcement efficiency can affect generalized trust. More in general, we add to the literature that seeks to understand how factors of the socioeconomic environment lead

beliefs and norms of cooperation to emerge and evolve<sup>43</sup>.

We are aware of problems of endogeneity in this case and we tried to overcome them by proposing a novel identification approach. Scope was to define a plausible empirical strategy able to answer our research question. We started by noticing that, according to previous literature, one of the main determinants of law enforcement efficiency in Italy seems to be lawyer density. However, using this quantity as an explanatory variable is still problematic as it could be connected with other unobservable determinants of trust. Results in Giacomelli and Carmignani (2010) and Mora-Sanguinetti et al. (2015) came to rescue us: the density of operating attorneys appears correlated to the distance from a law school, because it is function of the number of law graduates. Thus, we looked for an exogenous shock to the density of law degree holders distributed over the Italian territory, of which effects could be appreciable in recent years. The hypothesis here is that such a shock could have an effect also on the lawyer density, and then, on enforcement efficiency in that area, allowing a causal study of the consequences of this swing on generalized trust. The Codignola reform of 1969, which created a mass university system in Italy, sounded like a possible candidate for this scope. Therefore, we collected historical data about the number of degrees issued by the Italian public law schools; we were able in this way to document an augmentative impact of the legislation on the figures coming from the group of faculties already active in 1969. As well, we also showed that the density of law degree holders in their proximity - at the time we observe our proxies for trust - should be, on average, higher. Furthermore, our data allow us to present empirical results connecting the effects of the Codignola reform on law graduates with the density of attorneys over the Italian peninsula between 2001 and 2012. Thus, as main instru-

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<sup>43</sup>Stiglitz and Hoff (2016) emphasize how culture and the social context shape the way an individual perceives the world, the lens through which he sees it, and the categories that he uses to understand and interpret it. Indeed, according to the authors agents' preferences, perception, and cognition are subject among other things to two deep social influences: the social contexts to which he has become exposed and, especially, accustomed; and the cultural mental models - including categories, identities, narratives, and worldviews - that he uses to process information (Stiglitz and Hoff, 2016).

ment for law enforcement efficiency in town  $a$  we defined an indicator variable equal to 1 if its closest law school was active at the time of the reform. However, even if the promulgation of the new legislation was likely to be exogenous to determinants of municipal level trust and legal enforcement efficiency, we also pointed out that this approach is not completely safe from possible criticalities. Indeed, there could still be the chance of some spurious relations between our indicator variable and other unobservable factors. There might exist cultural and institutional features, common to the territories with schools active since the time of the reform, and which drive both generalized trust and performance of legal enforcement of these areas. This fact would lead us to obtain inconsistent coefficients. We could not get rid of this further potential issue; however, results from a battery of balance tests seemed to indicate that it might be a minor concern in our case. Moreover, they gave us some indications on a possible source of bias to control for in our econometric models.

According to our main estimations, in those areas with a higher density of attorneys - due to the Codignola reform - people seem characterized by lower levels of interpersonal trust. In our theoretical framework this result should be due to a lower efficiency of monitoring in these territories, which in turn creates incentives to misbehave and develop less pro-social beliefs. Thus, we also presented evidence connecting the presence of attorneys with the performance of courts and law enforcement. For this scope we showed the positive impact of both lawyer density and a proxy for the density of law degree holders on the length of first degree civil trials - an efficiency measure of law enforcement according to past research. Therefore, also with our data we could confirm previous results in the literature about a negative relation between lawyer density and efficiency of legal enforcement in Italy. Finally, we performed multiple robustness checks to test if unobservable characteristics common to important urban areas as well as to (some of) the cities with a law school active since the Codignola reform are affecting our results. For this aim we re-estimated all reduced form equations without considering in our sample, alternatively, provincial capitals and municipalities with a law school in 1997.



We also performed further tests augmenting our regression equations with the distance from provincial capitals, and the distance from cities with faculties of economics, medicine, engineering and mathematics already active since the reform in 1969. Looking at the results of these robustness checks, we notice that magnitude and significance of the coefficients of our instruments for legal enforcement efficiency vary across some of the specifications. Thus, we cannot completely rule out the possibility that also the effects of other omitted variables drive in part our calculations. However, the hypothesized negative effect on both our proxies for generalized trust is always supported, and statistically significant in many cases. Moreover, this effect dies out in relation with the distance from a law school, as expected. We read these regularities as a possible sign in support of our thesis: a poorly performing legal enforcement seems to lead agents to develop less trust.

What does it influence the behaviour of a person? In game theoretic terms, his/her expected payoffs of his/her actions. And these expected payoffs depend in many circumstances on his/her beliefs and preferences (e.g. Bigoni et al., 2015; Bigoni et al., 2018). However, beliefs, preferences and more in general our behaviour, are not only affected by the external environment in which we operate in that given point in time. As a great deal of psychological and economic research has shown, they are also influenced by a more “rigid” mental structure, made of sanctions, traumas, taboos, customs, traditions, and codes of conduct, which develops especially during childhood (for example through parenting and schooling) and tends to be very persistent over a person’s life<sup>44</sup>. Related to our enquiry is the fact that how much

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<sup>44</sup>In Tangney and Dearing (2004) and Tangney et al. (2007), shame and guilt, together with empathy, are regarded as “moral” emotions that help us “keep to the straight and narrow” . Like empathy, shame and guilt - or the anticipation of these emotions - are presumed to inhibit all manner of misdeeds and wrongdoing. According to their empirical research - which builds on the seminal work by Lewis (1971) where shame involves a negative evaluation of the global self and guilt involves a negative evaluation of a specific behaviour - our capacity of such feelings and their intensity is intimately connected not only to actual consequences of the action under scrutiny. For example, they might also depend on what we think it could be the opinion of our parents about this action. As well, these feelings are related to the parenting style we were exposed (e.g. less or more humiliating) during our childhood and how harsh parents used to “punish” us in case of wrongdoings. These elements from the past are “memorized” in the subconscious and influence our (moral) behaviour when adults as well, often in very subtle manners. For instance,

to be trustworthy and trust others today seems also connected to an education of cooperation received during childhood (Stiglitz and Hoff, 2016; Tabellini, 2008; Nunn and Watchenkon, 2011). According to the theoretical model in Tabellini (2008) the educational style of parents and other key educators might also be influenced by some institutional features. In particular, Tabellini (2008) shows that parents living in environments with an effective legal enforcement are more likely to transmit values of cooperation and trust since economically convenient for their pupils. Thus, what matters for the level of generalized trust in a society might be not only the current performance of law enforcement - as we attempted to show. Also its efficiency in the past could be relevant due to possible effects on the present behaviour of agents through education. Empirical evidence about this dynamic would be desirable.

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Stuewig et al. (2015) collected data about 380 subjects, interviewed when aged 10-12 and then re-interviewed when aged 18-21. Authors show that individuals prone to feel guilt (guilt-proneness) during childhood were on average less likely during young adulthood to use drugs and alcohol, get involved with the criminal justice system, have fewer sexual partners. More controversial the results for children more prone to shame (shame-proneness). In fact, shame-proneness was a risk factor for later deviant behaviour: subjects were more likely to have unprotected sex and use illegal drugs in young adulthood. Stuewig et al. (2015) argue that this evidence might connect to the abovementioned distinction of shame and guilt by Lewis (1971) and the fact that being shame-prone involves a persistent negative evaluation of the global self and not only of the “bad” actions that occur. According to Stuewig et al. (2015), a person with a devaluated self might be less incentivized to avoid (self) harming actions.

## Appendix

Tab.A.1

*Descriptive statistics - individual level data*

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Age	7,321	48	17	18	34	63	96
Trust:							
Trust in others	1606 (23.03%)						
No trust in others	5367 (76.96%)						
Gender:							
Women	3730 (50.89%)						
Men	3599 (49.11%)						
Study level:							
Elementary school (not completed)	241 (3.29%)						
Elementary school	1176 (16.06%)						
Lower middle school	2327 (31.79%)						
Professional certificate	498 (6.8%)						
High school	2302 (31.45%)						
University degree	774 (10.57%)						
Employment status:							
Other	39 (0.53%)						
Housework	1030 (14.11%)						
Unemployed	327 (4.48%)						
Employee	3350 (45.90%)						
Retired	1917 (26.27%)						
First employment	120 (1.64%)						
Rent	6 (0.08%)						
Student or army volunteering	508 (6.96%)						

*Notes:* descriptive statistics of the individual level data. Sources are the 2001, 2008, 2013 ITANES (Italian National Election Studies) surveys.

Tab.A.2

*Descriptive statistics – municipal level data from the Italian census of 1961*

Statistic (1961 Italian census data)	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Resident population	8,035	5,538	18,447	6	1,224	4,897	784,194
Population scatter (% pop. leaving in urban settlements)	8,035	28.36	24.49	0.00	6.20	47.35	94.60
Male pop. share (Male and female pop. share ratio)	8,035	98.94	7.43	56.70	94.70	102.70	243.40
Elderly index (65+ and 0-14 year old pop. share ratio)	8,035	56.67	36.46	6.60	32.50	68.70	325.00
Property house (% of own homes)	8,035	62.42	18.63	1.10	48.00	77.40	100.00
Services per property (Available services per property index)	8,035	28.14	18.00	0.10	12.20	42.10	88.60
Gender schooling disparity (Male and female share with high school degree ratio)	7,922	172.19	89.31	9.70	122.70	197.67	1,336.40
Labour market participation (males) (Employed males over unemployed males)	8,035	74.07	5.79	46.30	70.40	77.80	93.80
Employment rate (Employed individuals over 15+ pop.)	8,035	49.14	7.85	28.30	43.70	53.70	86.20

*Notes:* descriptive statistics of the census municipal level data. Source is the Italian census of 1961 conducted by ISTAT (Italian National Institute of Statistics).

Tab.A.3  
*Descriptive statistics - municipal level data*

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Distance from closest law school active in 1997 (km)	7,312	35.74	32.27	0.00	9.49	54.06	191.01
Distance from closest <i>capoluogo di provincia</i> (km)	7,313	13.78	13.65	0.00	0.00	22.72	137.85
Distance from closest math school active before 1969 reform (km)	7,312	41.42	35.67	0.00	11.64	63.56	223.83
Distance from closest medicine school active before 1969 reform (km)	7,312	43.22	37.75	0.00	12.06	65.95	191.01
Distance from closest engineering school active before 1969 reform (km)	7,312	56.85	54.95	0.00	14.98	79.10	277.99
Distance from closest economic school active before 1969 reform (km)	7,312	42.69	38.45	0.00	11.64	62.70	223.83
Nonprofit association density (2001)	7,313	0.005	0.002	0.0002	0.003	0.006	0.02
Resident population (2001)	7,313	238,605	579,289	289	7,164	91,986	2,552,433
Closest law school active before 1969 reform (dummy = 1)	7,313	0.862					

*Notes:* descriptive statistics of the municipal level data. Sources are ISTAT (Italian National Institute of Statistics) and own calculations.

Tab.A.4  
*Descriptive statistics - provincial level data*

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Lawyers enrolled to <i>albo degli avvocati</i> (2001)	101	1,265	2,100	104	396	1,145	14,882
Lawyers enrolled to <i>albo degli avvocati</i> (2004)	101	1,556	2,541	134	495	1,428	17,919
Lawyers enrolled to <i>albo degli avvocati</i> (2006)	101	1,745	2,870	141	562	1,689	20,675
Lawyers enrolled to <i>albo degli avvocati</i> (2008)	101	1,940	3,087	162	624	1,889	21,597
Lawyers enrolled to <i>albo degli avvocati</i> (2010)	101	2,124	3,458	176	670	2,012	24,767
Lawyers enrolled to <i>albo degli avvocati</i> (2012)	101	2,220	3,555	176	702	2,082	24,354
Proxy for the sum of people graduated in law as in 1997	101	3,367	8,393	0	0	3,707	52,907

*Notes:* descriptive statistics of the number of law graduates in 1997 and of lawyers enrolled in the *albi degli avvocati* aggregated at a provincial level (2001 adminis-

trative division). Law graduates in 1997 is obtained by summing (by province) all law graduates by each public university, from 1958 to 1997. Own calculations based on *Cassa Forense* (Italian Institute for Provident Fund and National Forensic Services) and ISTAT (Italian National Institute of Statistics) data.

Tab.A.5  
*Descriptive statistics – university level data*

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Graduates from public law schools (1960)	25	209	256	47	59	194	896
Graduates from public law schools (1970)	26	231	301	58	98	192	1,419
Graduates from public law schools (1980)	28	288	306	34	118	288	1,236
Graduates from public law schools (1990)	29	472	518	60	193	418	2,293
Graduates from public law schools (1997)	34	556	581	52	237	559	2,836
Graduates from public law schools (2014)	41	461	1,117	73	141	330	7,229

*Notes:* descriptive statistics for the university level data. Data sources are ISTAT (Italian National Institute of Statistics) and MIUR (Italian Ministry of University Education and Research). Public law schools only.

Tab.A.6  
*Descriptive statistics - court district data*

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
1995 first degree civil trial length (n. of days)	26	1,180.33	434.42	630.56	895.88	1,360.65	2,751.81
1997 first degree civil trial length (n. of days)	26	955.36	352.43	474.42	754.30	1,063.51	1,866.87
1999 first degree civil trial length (n. of days)	26	955.52	324.79	528.69	738.46	1,146.52	1,845.98
2001 first degree civil trial length (n. of days)	26	744.98	224.26	383.84	631.60	853.63	1,406.06
2005 first degree civil trial length (n. of days)	25	707.42	185.59	334.75	611.43	790.94	1,170.30
2012 first degree civil trial length (n. of days)	26	431.21	148.47	183.97	312.08	561.39	659.55

*Notes:* descriptive statistics of the court district data. Sources are ISTAT (Italian National Institute of Statistics) and the Italian Ministry of Justice.

Tab.A.7  
*Average first degree civil trial duration*

District	Average duration (in days)		
	1995	2001	2012
Ancona	1,361.620	544.710	322.771
Bari	1,348.989	1,023.625	624.427
Bologna	941.056	632.027	310.341
Brescia	893.412	687.024	300.617
Cagliari	1,142.401	800.785	472.245
Caltanissetta	1,390.929	723.980	498.276
Campobasso	1,357.762	647.927	391.469
Catania	1,073.792	718.869	549.758
Catanzaro	1,446.180	1,044.634	594.875
Firenze	903.296	631.462	322.213
Genova	787.571	605.895	314.500
L'Aquila	980.334	670.299	358.131
Lecce	1,227.814	929.385	492.871
Messina	1,777.657	1,406.063	659.550
Milano	630.564	475.305	247.855
Napoli	1,095.790	856.877	565.278
Palermo	1,063.533	728.997	441.307
Perugia	882.982	783.904	407.615
Potenza	2,751.813	1,060.491	642.768
Reggio Calabria	1,753.147	843.902	623.986
Roma	1,128.822	654.380	519.324
Salerno	1,366.287	896.919	608.548
Torino	713.997	383.849	223.162
Trento	831.312	430.247	183.971
Trieste	866.161	528.908	224.378
Venezia	971.465	659.215	311.285

*Notes:* average number of days it takes to complete a first degree civil trial in each Italian district court using the disposition time formula defined by the CEPEJ (European Commission for the Efficiency of Justice). Three reference years: 1995, 2001, 2012. Data for 1995 and 2001 were manually collected from annual statistics bulletins of the Italian justice system (*Annuario di statistiche giudiziarie*), which can be found in ISTAT (Italian National Institute of Statistics) library. For year 2012 data for calculations are downloadable from the Italian Ministry of Justice website ([www.giustizia.it/giustizia](http://www.giustizia.it/giustizia)).

Tab.A.8

*Yearly number of law graduates and the Codignola law (public universities only)*

	<i>Dependent variable:</i>								
	Law graduates								
	(1975)	(1976)	(1977)	(1978)	(1979)	(1980)	(1981)	(1982)	(1983)
Law school active before Codignola law	214.080*** (50.116)	147.115*** (57.076)	164.808** (77.698)	151.885* (87.315)	142.615 (94.938)	131.423 (111.117)	138.154 (98.132)	51.846 (132.850)	143.423 (115.602)
Observations	26	27	28	28	28	28	28	28	28
R <sup>2</sup>	0.027	0.009	0.023	0.021	0.016	0.013	0.018	0.003	0.013

*Notes:* cross sectional analysis of the number of students graduated in law from Italian public law schools. OLS calculations. Reference years: 1975 - 1983. The regressor is a dummy variable that takes value equal to 1 if the law school  $i$  was already in activity before the Codignola reform of 1969. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Tab.A.9

*Yearly number of law graduates and the Codignola law (public universities only)*

	<i>Dependent variable:</i>								
	Law graduates								
	(1984)	(1985)	(1986)	(1987)	(1988)	(1989)	(1990)	(1991)	(1992)
Law school active before Codignola law	242.679*** (65.141)	-38.808 (54.366)	175.346 (110.350)	146.577 (135.586)	192.500 (163.337)	262.256* (139.605)	255.564 (156.294)	272.244* (162.592)	319.596** (146.479)
Observations	29	27	28	28	28	29	29	29	30
R <sup>2</sup>	0.075	0.001	0.022	0.014	0.019	0.030	0.023	0.026	0.045

*Notes:* cross sectional analysis of the number of students graduated in law from Italian public law schools. OLS calculations. Reference years: 1984 - 1992. The regressor is a dummy variable that takes value equal to 1 if the law school  $i$  was already in activity before the Codignola reform of 1969. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Tab.A.10

*Yearly number of law graduates and the Codignola law (public universities only)*

	<i>Dependent variable:</i>								
	Law graduates								
	(1993)	(1994)	(1995)	(1996)	(1997)	(2007)	(2008)	(2009)	(2010)
Law school active before Codignola law	406.135*** (93.765)	369.677*** (137.312)	413.087*** (140.855)	427.875*** (163.732)	430.442*** (141.916)	89.128*** (27.419)	159.728*** (45.732)	194.326*** (53.755)	195.887*** (58.781)
Observations	30	31	31	34	34	41	41	41	41
R <sup>2</sup>	0.091	0.064	0.098	0.083	0.102	0.136	0.151	0.161	0.143

*Notes:* Cross sectional analysis of the number of students graduated in law from Italian public law schools. OLS calculations. Reference years: 1993 - 1997 and 2007 - 2010. The regressor is a dummy variable that takes value equal to 1 if the law school  $i$  was already in activity before the Codignola reform of 1969. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Tab.A.11

*Yearly number of law graduates and the Codignola law (public universities only)*

	<i>Dependent variable:</i>			
	Law graduates			
	(2011)	(2012)	(2013)	(2014)
Law school active before Codignola law	377.233** (189.466)	337.874** (155.426)	235.113*** (68.578)	466.259* (266.479)
Observations	41	41	41	41
R <sup>2</sup>	0.053	0.063	0.150	0.041

*Notes:* Cross sectional analysis of the number of students graduated in law from Italian public law schools. OLS calculations. Reference years: 2011 - 2014. The regressor is a dummy variable that takes value equal to 1 if the law school  $i$  was already in activity before the Codignola reform of 1969. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Tab.A.12

*Proxy for the law graduate density and the Codignola law (public universities only)*

	<i>Dependent variable:</i>		
	Law graduates density (1997)		
	(1)	(2)	(3)
Law school was active before Codignola law	0.076*** (0.025)	0.060*** (0.019)	0.033*** (0.010)
Macro region FE (North-east/west, Center, South, Islands)	NO	YES	NO
Region FE	NO	NO	YES
Observations	42	42	42
R <sup>2</sup>	0.126	0.310	0.802
Adjusted R <sup>2</sup>	0.104	0.214	0.646

*Notes:* OLS calculations. Cross sectional analysis of the cumulative sum of the number of students graduated in law from the Italian public law schools between



1958 to 1997, divided by the population in 1997 of the city where they are located. The cumulative sum equals 0 if the public law school was founded after the 1997. The regressor is a dummy variable that takes value equal to 1 if the law school  $i$  was already in activity before the Codignola reform of 1969. Data sources are ISTAT (Italian National Institute of Statistics) and own calculations. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Tab.A.13  
*Effects of lawyer density on first degree civil trial length*

	<i>Dependent variable:</i>					
	Civil trial length					
	(n. of days - CEPEJ formula)					
	(2001)		(2008)		(2012)	
	(1)	(2)	(3)	(4)	(5)	(6)
Lawyer density (2001)	1,622.403 (1,825.462)	1,311.378 (1,554.857)				
Lawyer density (2008)			2,545.407** (1,005.997)	1,684.517** (857.917)		
Lawyer density (2012)					2,113.452*** (720.533)	1,205.434** (559.370)
5 macro regions FE (North-east/west, Center, South, Islands)	YES	NO	YES	NO	YES	NO
3 macro regions FE (North, Center, South)	NO	YES	NO	YES	NO	YES
Observations	143	143	143	143	143	143
Adjusted R <sup>2</sup>	0.595	0.583	0.697	0.748	0.710	0.785

*Notes:* OLS estimations for the following reference years: 2001, 2008, 2012. The dependent variable is the average length (in days) of first degree civil trials in each of the 26 Italian court districts, calculated with CEPEJ (European Commission for the Efficiency of Justice) disposition time formula. For year 2001, data come from bulletins reporting annual statistics of the Italian justice system (*Annuario di statistiche giudiziarie*), available in ISTAT (Italian National Institute of Statistics) library. For years 2008 and 2012 data for calculations are downloadable from the Italian Ministry of Justice website ([www.giustizia.it/giustizia](http://www.giustizia.it/giustizia)). Lawyer density corresponds to the number of lawyers enrolled in the *albi degli avvocati* (city-level lawyer registers), divided by the resident population of the corresponding town  $i$  for the considered year. The 5 macro regions correspond to NUTS 1 Italian areas. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Tab.A.14

*The Codignola reform effect on the density of nonprofit associations – baseline model and robustness tests (no resident pop. in 2001 among the regressors)*

<i>Dependent variable:</i>					
2001 nonprofit ass.					
density					
	(1)	(2)	(3)	(4)	(5)
Closest law school active before Codignola reform	0.0001 (0.0002)	0.0001 (0.0002)	0.0001 (0.0002)	0.0003 (0.0002)	-0.0004*** (0.0001)
Province FE	YES	YES	YES	YES	YES
Cities with law school active in 97'	YES	NO	YES	YES	YES
<i>Capoluogo di provincia</i> cities	YES	YES	NO	YES	YES
Distance from closest <i>capoluogo di provincia</i>	NO	NO	NO	YES	NO
Distance from closest Economics, Medicine, Math, Engineering schools	NO	NO	NO	NO	YES
Observations	7,570	7,536	7,457	7,550	7,312
R <sup>2</sup>	0.257	0.257	0.258	0.277	0.440
Adjusted R <sup>2</sup>	0.247	0.247	0.247	0.267	0.432

*Notes:* OLS estimates. The dependent variable is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). Sample is formed by all available Italian municipalities. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Tab.A.15

*The Codignola reform effect on the density of nonprofit associations – baseline model and robustness tests (with resident pop. in 2001 among the regressors)*

<i>Dependent variable:</i>					
2001 nonprofit ass.					
density					
	(1)	(2)	(3)	(4)	(5)
Closest law school active before Codignola reform	0.0001 (0.0002)	0.0001 (0.0002)	0.0002 (0.0002)	0.0003 (0.0002)	-0.0003* (0.0001)
Province FE	YES	YES	YES	YES	YES
Resident population in 2001	YES	YES	YES	YES	YES
Cities with law school active in 97'	YES	NO	YES	YES	YES
<i>Capoluogo di provincia</i> cities	YES	YES	NO	YES	YES
Distance from closest <i>capoluogo di provincia</i>	NO	NO	NO	YES	NO
Distance from closest Economics, Medicine, Math, Engineering schools	NO	NO	NO	NO	YES
Observations	7,570	7,536	7,457	7,550	7,312
R <sup>2</sup>	0.258	0.260	0.272	0.277	0.454
Adjusted R <sup>2</sup>	0.247	0.250	0.262	0.267	0.446

*Notes:* OLS estimates. The dependent variable is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). Sample is formed by all available Italian municipalities. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Tab.A.16

*The Codignola reform effect on the density of nonprofit associations – baseline model and robustness tests augmented by the distance from the closest public law school (no resident pop. in 2001 among the regressors)*

	<i>Dependent variable:</i>				
	2001 nonprofit ass.				
	density				
	(1)	(2)	(3)	(4)	(5)
Distance from closest law school (km)	0.00002*** (0.00000)	0.00002*** (0.00000)	0.00002*** (0.00000)	0.00002*** (0.00000)	-0.00001* (0.00000)
Closest law school active before Codignola reform	-0.001*** (0.0003)	-0.001*** (0.0003)	-0.001*** (0.0003)	-0.001*** (0.0003)	-0.001*** (0.0003)
Interaction	0.00003*** (0.00000)	0.00003*** (0.00000)	0.00003*** (0.00000)	0.00003*** (0.00000)	0.00001*** (0.00000)
Province FE	YES	YES	YES	YES	YES
Cities with law school active in 97'	YES	NO	YES	YES	YES
<i>Capoluogo di provincia</i> cities	YES	YES	NO	YES	YES
Distance from closest <i>capoluogo di provincia</i>	NO	NO	NO	YES	NO
Distance from closest Economics, Medicine, Math, Engineering schools	NO	NO	NO	NO	YES
Observations	7,564	7,536	7,451	7,544	7,311
R <sup>2</sup>	0.280	0.280	0.281	0.279	0.441
Adjusted R <sup>2</sup>	0.270	0.270	0.271	0.269	0.432

*Notes:* OLS estimates. The dependent variable is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). Sample is formed by all available Italian municipalities. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Tab.A.17

*The Codignola reform effect on the density of nonprofit associations – baseline model and robustness tests augmented by distance from the closest public law school (with resident pop. in 2001 among the regressors)*

	<i>Dependent variable:</i>				
	2001 nonprofit ass.				
	density				
	(1)	(2)	(3)	(4)	(5)
Distance from closest law school (km)	0.00002*** (0.00000)	0.00002*** (0.00000)	0.00002*** (0.00000)	0.00002*** (0.00000)	-0.00001 (0.00000)
Closest law school active before Codignola reform	-0.001*** (0.0003)	-0.001*** (0.0003)	-0.001*** (0.0003)	-0.001*** (0.0003)	-0.001*** (0.0003)
Interaction	0.00003*** (0.00000)	0.00003*** (0.00000)	0.00002*** (0.00000)	0.00003*** (0.00000)	0.00002*** (0.00000)
Province FE	YES	YES	YES	YES	YES
Resident population in 2001	YES	YES	YES	YES	YES
Cities with law school active in 97'	YES	NO	YES	YES	YES
<i>Capoluogo di provincia</i> cities	YES	YES	NO	YES	YES
Distance from closest <i>capoluogo di provincia</i>	NO	NO	NO	YES	NO
Distance from closest Economics, Medicine, Math, Engineering schools	NO	NO	NO	NO	YES
Observations	7,564	7,536	7,451	7,544	7,311
R <sup>2</sup>	0.280	0.282	0.291	0.279	0.456
Adjusted R <sup>2</sup>	0.269	0.271	0.281	0.269	0.448

*Notes:* OLS estimates. The dependent variable is calculated as the ratio between the number of nonprofit associations in town  $a$ , in 2001, and the municipal resident population of the same year. Data source is ISTAT (Italian National Institute of Statistics). Sample is formed by all available Italian municipalities. Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Tab.A.18

*Relation between law graduate density and law enforcement efficiency*

	<i>Dependent variable:</i>			
	First degree civil trial length (num. of days)			
	(2001)	(2002)	(2003)	(2004)
Law graduate density proxy (1997)	33.557 (1,726.522)	2,573.187*** (912.361)	3,664.993** (1,478.118)	5,160.419*** (1,355.113)
Regional FE	YES	YES	YES	YES
Observations	21	21	21	21
R <sup>2</sup>	0.686	0.732	0.848	0.887
Adjusted R <sup>2</sup>	-0.257	-0.071	0.390	0.549

*Notes:* OLS estimations with cross sectional data. The dependent variable is the average length (in days) of first degree civil trials in each of the 26 Italian court districts, calculated with CEPEJ (European Commission for the Efficiency of Justice) disposition time formula. Data come from bulletins reporting annual statistics of the Italian justice system (*Annuario di statistiche giudiziarie*), available in ISTAT (Italian National Institute of Statistics) library. Law graduate density proxy (1997) is obtained by summing (by province) all law degrees issued by each public university from 1958 to 1997; this figure is then divided by the population of the corresponding province in 1997 (data source: ISTAT). Robust standard errors in parentheses. \* p<0.1; \*\* p<0.05; \*\*\* p<0.01.

Tab.A.19

*Relation between law graduate density and law enforcement efficiency*

	<i>Dependent variable:</i>					
	First degree civil trial length (num. of days)					
	(2008)	(2009)	(2010)	(2011)	(2012)	(2013)
Law graduate density proxy (1997)	1,391.351*** (274.981)	2,157.348*** (458.144)	2,523.405*** (536.631)	3,072.249*** (693.564)	1,452.135*** (480.271)	1,343.554* (691.807)
Regional FE	YES	YES	YES	YES	YES	YES
Observations	20	20	20	20	20	20
R <sup>2</sup>	0.902	0.858	0.843	0.905	0.937	0.896
Adjusted R <sup>2</sup>	0.535	0.326	0.252	0.549	0.699	0.507

*Notes:* OLS estimations with cross sectional data. The dependent variable is the average length (in days) of first degree civil trials in each of the 26 Italian court districts, calculated with CEPEJ (European Commission for the Efficiency

of Justice) disposition time formula. Data for calculations are downloadable from the Italian Ministry of Justice website ([www.giustizia.it/giustizia](http://www.giustizia.it/giustizia)). Law graduate density proxy (1997) is obtained by summing (by province) all law degrees issued by each public university from 1958 to 1997; this figure is then divided by the population of the corresponding province in 1997 (data source: ISTAT, Italian National Institute of Statistics). Robust standard errors in parentheses. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

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