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INTERGENERATIONAL TRANSMISSION IN REGULATED PROFESSIONS AND THE ROLE OF FAMILISM

by Omar Bamieh[*] and Andrea Cintolesi[*]

Abstract

We measure the extent to which familism accounts for the intergenerational transmission of jobs in regulated professions. Before 2004, local committees graded the Italian bar exams for lawyers, but since 2004, exams have been randomly assigned to external committees for grading. We proxy for family ties with the number of successful candidates sharing a family name and law firm address with a lawyer that is already registered. We estimate that the number of new entrants with a family tie drops by at least 10 per cent, while the number of new lawyers does not change, showing that familism accounts for an important part of the intergenerational transmission in our setting. While we do not find any significant differences by gender, familism is stronger in areas with low social capital, which also feature lower rents from licences.

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1 Introduction

Many professions are characterized by a licensing exam graded by a local committee, which is intended to be an important form of regulation for an industry to ensure quality standards. At the same time, despite the presence of licensing exams, intergenerational transmission in these professions remains sizable. For example, in the United States, approximately 10% of the members of regulated professions such as law and medicine have a parent who worked in the same profession (Laband and Lentz (1992); Lentz and Laband (1989)), and in Italy, the probability for a graduate's child to become a liberal professional doubles when the father is a graduate liberal professional (Aina and Nicoletti (2018)).

There are different environmental and economic factors that may explain the intergenerational transmission in regulated professions, such as family upbringing, genetic ability or regulations.[1] Beyond these factors, intergenerational transmission may also be related to familism, i.e. behaviours aimed at creating unfair advantages for relatives to enter the profession. Recent empirical evidence suggest that familism triggers negative selection (e.g. Basso et al. (2021); Durante, Labartino and Perotti (2011)),[2] raising the need to understand the extent to which familism accounts for intergenerational transmission. Besides, to design effective policy limiting familism, we need to understand which are the channels through which familism operates. At present, the empirical evidence on this subject is surprisingly scant.[3] This paper fills this gap and identifies the role played by family connections, strength of social norms and economic incentives in creating dynasties in regulated professions.

Disentangling the share of intergenerational transmission generated by familism from other factors is empirically challenging and requires a suitable testing ground. For this reason, we focus on the legal profession in Italy, in which the licensing exam was reformed in 2004. Before 2004, the Italian bar exam was graded by a local committee, potentially leaving room for relatives to interfere with the grading process. In this respect, it was very similar to many current licensing exams across the world.[4] Then, a reform severely limited

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[1]e.g., Björklund, Lindahl and Plug (2006); Aina and Nicoletti (2018); Mocetti (2016); Mocetti, Roma and Rubolino (2020).

[2]A positive selection could arise if the lower ability of new entrants is compensated by parental spillovers.

[3]An exception is Aina and Nicoletti (2018), who used a set of controls and concluded that familism seems to help with completing compulsory practice but not with passing the licensing exam.

[4]For example, in France, the bar exam is graded at the university level, and in Austria and Germany, it takes place at the state level. The populations of these jurisdictions are comparable to those of the Italian districts.

the extent to which parents can resort to unfair practices to increase the probability of their children passing the bar exam. Starting in 2004, the exams of each district are graded by an external committee that is randomly selected every year.[5]

We proxy for family ties by using the number of successful candidates with the same family name and law firm address of an already registered lawyer. Compared to the existing literature, (Güell, Mora and Telmer (2015)), using law firm addresses does not force us to rely on rare family names to identify plausible family linkages. Since 2004, the number of new entrants per inhabitant with family ties has declined by 10–30%. The number of lawyers did not change following the 2004 reform, suggesting that good candidates used to be crowed out by candidates with family ties. There are different reasons to interpret the estimates as lower bounds of the real effect of complete decentralization of grading committees: first, the 2004 reform decentralized only the written part, while the oral part still takes place at local level; second, we do not track family ties between relative with different surnames, like mother and child; third, family networks might well go beyond districts. To get a sense of the size of the estimated effects, note that since 2004, 4118 new lawyers entered the register with a family connection, representing approximately 4 percent of the new entrants. We estimate that, in the absence of the reform, approximately 1297 more lawyers with family ties would have been in the register. This effect is similar for males and females aspiring lawyers.

Lawyers entering the profession merely because they received family help lowers the standard of the profession (Basso et al. (2021)). This calls for policies intended to combat familism, and in the second part of the paper, we provide insights into where familism prevails. Evidence from the literature suggests that familism should be more common where the rents from a license are high (e.g., Brollo et al. (2013)) and social capital is low (e.g., Durante, Labartino and Perotti (2011)). We investigate whether familism correlates with rents from the license and/or social capital. This investigation is particularly interesting in our setting because the income of Italian lawyers is heterogeneous across geographical areas: districts with higher returns from the profession also have higher degrees of social capital, whereas districts with lower returns have lower social capital, making rents and social capital positively correlated. We repeat our empirical exercise for each Italian region, and we find that places with low social capital are those where familism practices are pervasive, even if the returns from the license are relatively lower

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[5]Note that the reform may reduce familism because external committees are less likely to favour candidates with relatives in the profession, but also because relatives may not even try the exam if they know that they will not receive any help. The objective of this paper is to measure the overall effect and not the two channels separately.

than in other places.

The contribution of this paper is twofold. First, it disentangles the importance of familism in intergenerational transmission in regulated professions from other possible drivers. When committees can be reached through family ties, we estimate that approximately 10–30 percent of new entrants with a family tie pass the exam only because of family help. Given that most licensing committees in Western countries are as close to candidates' families as the ones we analyze, this result may be important for the correct design of current institutions controlling access to regulated professions. Second, this paper provides evidence that familism is pervasive in areas with low social capital, despite the lower rents from licenses, suggesting that beyond economic incentives other components may matter in spreading familism.

The rest of this paper is organized as follows: Section 2 reviews the literature and describes our measure of family ties. Section 3 explains the institutional settings of the Italian bar exam and the 2004 reform. Section 4 describes our empirical strategy. Section 5 shows how the 2004 reform affected familism and Section 6 concludes.

2 Related literature

This paper is related to four streams in the literature. First, it relates to the literature on familism and nepotism. There are two papers on this subject in the context of the legal profession. Basso et al. (2011) found a negative relationship between the age when people start a law practice in Italy and the frequency of their family name in the local register. This association decreased after 2004, pointing in the same direction as this study. Laband and Lentz (1992) studied the intergenerational transmission of the legal profession in the United States, and they concluded that the presence of nepotism at the entry stage cannot be rejected. Basso et al. (2011) and Laband and Lentz (1992) are interested in showing the presence of familism, instead our empirical exercise is primarily intended to quantify the relevance of it with respect to other drivers in accounting for intergenerational transmission. In our setting familism takes the form of cheating in a licensing exam, which can be seen as a form of corruption. Using data for Italy, Gagliarducci and Manacorda (2020), show that politicians extract significant rents, in terms of private sector jobs, for their family members.

Second, this paper relates to the literature on the intergenerational transmission of jobs. This literature stream has mostly focused on the role of innate ability versus

that of family upbringing and environmental factors[6] (Bowles, Gintis and Groves (2009); Björklund, Lindahl and Plug (2006); Sacerdote (2011); Lo Bello and Morchio (2020)). More recently, Mocetti (2016) showed that children are more likely to follow their parents' careers if positional rents favor their parents' profession. Aina and Nicoletti (2018) showed that there is a significant effect of familism on the probability of completing compulsory practice but not on passing the licensing exam. Unlike Aina and Nicoletti (2018), we do not rely on a set of controls to isolate the effect of familism but rather exploit exogenous variation. Furthermore, our sample is larger, as it contains all lawyers and not just those who graduated from a subset of universities. Beyond the previously studied channels, this paper contributes to the literature by showing that dynasties in professions are in part generated by familism.

Third, this paper is in line with the literature on the role of relatives in family firms (e.g., Bertrand and Schoar (2006)), which supports the idea that a culture based on strong family ties may sometimes impede economic development. Suboptimal economic organizations can emerge when relatives place excessive weight on keeping the business in the family, perhaps due to a strong sense of duty towards other family members or a more selfish desire to turn the business into a family legacy.[7] In this paper, we show that senior lawyers not only help their relatives pass the bar exam but also hire relatives to work at their law firms. This familism may represent a cost because the relatives who passed the bar exam thanks to their family name are presumably less qualified than the average candidate. Therefore, senior lawyers gladly bear the cost of hiring less qualified junior lawyers to help family members.

Finally, this paper relates to the literature on social capital and social norms. Durante, Labartino and Perotti (2011) found that decentralization led to a significant increase in familism among Italian academics in areas with low social capital, but to a reduction in familism in areas with high social capital. Their empirical exercise regarded a public sector job, for which one can hire an unqualified relative without bearing any consequence. Compared to Durante, Labartino and Perotti (2011), this study focuses on a private sector job, a context where family members are more likely to internalize the costs of having an unqualified person working for them. Another difference relative to Durante, Labartino and Perotti (2011) is that the returns from accessing a public sector profession are higher in less developed areas of the country, a consequence of the equalized salaries in the Italian public sector. Given that less developed areas are also those with low social capital, one

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[6]These factors might favor the transmission of occupation-specific human capital within the family and, therefore, intergenerational persistence in professions.

[7]Other contributions on the role of relatives in family firms include Bassanini et al. (2013).

cannot say whether the economic motive to help relatives in the academic profession prevails over the civic motive.

Family names have been used before to study intergenerational mobility. Güell, Mora and Telmer (2015) study intergenerational mobility using the joint distribution of surnames and economic outcomes. Their methodology relies on rare family names because only rare family names are indicative of familial linkages. Applying the same methodology to Italy, Güell et al. (2018) find that higher intergenerational mobility is positively associated with economic activity, education and social capital and negatively correlated with inequality. Our approach circumvents this problem because the Italian national register of lawyers contains information not only on family names but also law firm address. Italian law firms are usually very small, (the median number of lawyers working at the same law firm address is 3, and the average 6.95), therefore, two lawyers sharing their family name and working at the same law firm are very likely to be related. Using the law firm address as additional information to establish family links is particularly important in the Italian setting because the distribution of family names is more concentrated in the south than in the north of Italy.

Differently from Güell, Mora and Telmer (2015), we are not interested in identifying relatives versus nonrelatives in the population of lawyers. Instead, we only want to study if the share of relatives changes following the 2004 reform. In this respect, our paper is closer to Mocetti (2016); Mocetti, Roma and Rubolino (2020), who study how rents and the strictness of professional services regulation determine the propensity to follow the parents' career.

Similarly to our study, Gagliarducci and Manacorda (2020) define families as groups of individuals sharing the same first three consonants and born in the same municipality. Durante, Labartino and Perotti (2011), instead, use the presence of multiple professors with the same last name in the same department as a proxy for the presence of relatives. Fafchamps and Labonne (2017) study political connections in the Philippines, exploiting the fact that in each municipality, a particular family name was given to only one family. Santavirta and Stuhler (2020) compare different name-based estimators used in intergenerational research and show that their interpretation crucially depends on the sampling properties of the data.

3 Institutional background, data and family ties

3.1 Institutional background

The Consiglio Nazionale Forense (CNF) is the institution representing Italian lawyers. Among its functions, the CNF maintains the national register of lawyers, appoints the examiners for the bar exam that grants admittance to the profession, administers the elections of district committees, and resolves disputes and complaints regarding register membership and disciplinary measures decided by district committees.

A law degree is a prerequisite to becoming a lawyer in Italy.[8] After graduating, the prospective lawyer begins a legal traineeship under the supervision of a senior lawyer. Upon completing the traineeship, the candidate can take the bar exam to become a lawyer. After successfully passing the bar exam, the new lawyer is registered in the lawyers' national register and can practice as a lawyer.

The Italian bar exam is composed of a written exam and an oral exam. Both exams take place in all 26 appellate courts. Candidates are assigned to the courts in the area where they had their traineeship. The written exam is anonymous and takes place in different locations simultaneously. The questions are identical across locations and are prepared in advance by the Ministry of Justice. Before 2004, the exams were graded by a local committee composed of lawyers, judges and law professors from the same geographical area in which the candidate took the exam. Grading standards varied significantly across districts, and pass rates were as low as 16% and as high as 96% (Pagliero and Buonanno (2018)). In the early 2000s, the enormous differences in pass rates across districts and anecdotal cases of the more lenient behavior of examiners towards relatives and friends raised concerns about the fairness of the bar exam. The Italian government responded by changing the system in 2004.[9] Starting with the 2004 exam, all the written exams in a district must be graded by a committee from another district that is randomly selected every year. The reform only affected the procedure for grading of the written exam. The oral exam is still graded by a committee from the same district of the candidates.

The number of graders in each district depends on the number of candidates in that district. To avoid excessive workloads on graders, the random assignment of grading

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[8]Law schools offer two degree levels. Students obtain the first degree by attending a three-year program and fulfilling all its requirements. Then, they can enroll in a more specialized two-year program. To become a lawyer, graduation from the five-year (full) program is required to sit for the bar exam.

[9]The so called Castelli reform, Law 180/2003, http://www.camera.it/parlam/leggi/03180l.htm

committees occurs within groups of districts that change marginally over time and consist of districts with similar numbers of candidates. For example, Milan, Rome, and Naples being the three most populous cities, have always been in the same group. Each year the ministry randomly draws Milan to correct the written exams from Rome or Naples, and the same happens to Rome and Naples.

3.2   Data and family ties

The CNF provided us with a full snapshot of the Italian register of lawyers as of 1 July 2017. It contains the following information for 205,311 lawyers: encrypted name, family name,[10] location, gender, date of birth and date of registration in the CNF archives. Lawyers cannot practice unless they are registered in the CNF register. Municipal tax revenues per capita and fiscal capacity data come from the Ministry of the Interior.

Our data allow us to identify lawyers with the same family name, the same law firm address as of July 2017 and the year in which they entered the register. We measure the family ties of new entrants in each Italian municipality with two measures. First, the number of new entrants sharing the family name and law firm address as an already registered lawyer; for year t and municipality c we label this FA_{ct}. Second, we compute the number of new entrants sharing a family name with a lawyer already registered in the same municipality, and we label this FM_{ct}.

Both measures are defined per 10000 inhabitants in year t and municipality c. The first measure is more precise, but also narrower, than the second measure. On the one hand, lawyers with the same family name working in the same place are very likely to be relatives. On the other hand, lawyers with the same family name operating in the same municipality include relatives working elsewhere. Relatives working at different law firms are not rare, and the results show that familism towards them is as large as that for relatives who decide to work together after passing the exam.

Table 1 reports some summary statistics for FA_{ct} and FM_{ct}. Comparison of FA_{ct} and FM_{ct} between different territories may well capture differences in the underlying distribution of family names. The high number of lawyers sharing family names in a given region might be because there are few family names in that region. Our empirical strategy, described in the next section, carefully circumvents this issue because we look at the within-municipality variations of FA_{ct} and FM_{ct} at the moment of the reform, and it is unlikely that the distribution of surnames within-municipalities drastically changed

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[10]Encryption does not allow us to see the real name and surname, but it does allow us to identify which lawyers have the same family name.

between 2004 and 2005. We discuss this in details in the following section.

Table 1: Descriptive statistics

| | FA_{ct} | | | | FM_{ct} | | | |
| | 2000-2004 | | 2005-2010 | | 2000-2004 | | 2005-2010 | |
Region	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Abruzzo e Molise	0.014	0.114	0.014	0.114	0.052	0.260	0.017	0.108
Basilicata	0.017	0.170	0.040	0.432	0.101	0.585	0.092	0.487
Calabria	0.034	0.254	0.028	0.264	0.226	0.762	0.100	0.420
Campania	0.035	0.299	0.035	0.301	0.222	0.802	0.218	0.758
Emilia Romagna	0.010	0.064	0.012	0.081	0.032	0.143	0.049	0.199
Lazio	0.018	0.123	0.034	0.222	0.117	0.438	0.096	0.426
Liguria	0.016	0.105	0.013	0.084	0.026	0.111	0.043	0.175
Lombardia	0.010	0.080	0.010	0.085	0.032	0.210	0.032	0.227
Marche and Umbria	0.018	0.108	0.015	0.089	0.048	0.183	0.046	0.188
Piemonte and VdA	0.009	0.068	0.008	0.071	0.030	0.169	0.025	0.132
Puglia	0.029	0.168	0.038	0.213	0.192	0.778	0.144	0.683
Sardegna	0.016	0.242	0.017	0.127	0.072	0.378	0.064	0.355
Sicilia	0.025	0.186	0.025	0.157	0.117	0.416	0.080	0.301
Toscana	0.022	0.122	0.027	0.155	0.082	0.294	0.072	0.253
Triveneto	0.012	0.097	0.014	0.138	0.019	0.133	0.019	0.125
Italy	0.020	0.172	0.021	0.19	0.10	0.48	0.079	0.40

Note.—Summary statistics for FA_{ct} and FM_{ct} between 2000 and 2004 and between 2005 and 2010. VdA means Valle d'Aosta. Trivento is formed by Friuli-Venezia Giulia, Veneto and the province of Trento. The province of Bolzano is excluded because it is the only province not affected by the reform.

4 Empirical strategy

We estimate a parametric event study model to learn the impact of the 2004 reform on familism. For municipality c and time t,[11]

$$y_{ct} = \alpha + \beta \cdot Reform_t + f(t) + \gamma X_{ct} + \phi_c + \varepsilon_{ct} \qquad (1)$$

where y_{ct} is FA_{ct} or FM_{ct}, $Reform_t$ is a dummy variable taking a value 1 for all years from 2005 (inclusive) onwards, $f(t)$ is a time trend that is allowed to differ before and after 2005, X_{ct} includes time-varying municipality controls and ϕ_c represents the municipality fixed effects. The municipality fixed effect makes our comparison a within-municipality comparison, addressing concerns of heterogeneous family name distributions across municipalities.[12] Our coefficient of interest is β, which identifies the effect of the reform on

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[11]Appendix B presents results for a nonparametric event study.

[12]Note that to identify this effect, the municipality fixed effect controls for the heterogeneity in the distribution of family names in equation (1) under the plausible assumption that the heterogeneity is

the number of successful candidates sharing a family name and law firm address with a lawyer already registered in town c and year t. In other words, because the reform represents a permanent shock affecting the scope to which lawyers can influence exam committees, our estimates disentangle the discontinuous change at municipal level, from possible time trends, in namesake. Note that we exploit FA_{ct} and FM_{ct} to gauge the effect of the 2004 reform on familism and not to estimate the evolution of familism *over time*. Indeed, it is crucial to include a time trend in the empirical analysis: FA_{ct} and FM_{ct} have a time trend, which is generated by a the structure of the data and is fully explained in Appendix F, and a direct interpretation of them as temporal measures of familism may be misleading.

The exercise assumes that the year of registration is the year in which a lawyer passes the bar exam. Because lawyers cannot practice until they are registered, they have no reason to delay their registration once they have passed the bar exam.[13] Therefore, the registration year is a reasonable proxy for the year in which a lawyer passed the bar exam.

Finally, because we seek to identify geographical heterogeneities, for every Italian region r composed of a set of municipalities M_r, we run the same but region-specific model:

$$y_{ct} = \alpha + \beta_r \cdot Reform_t + f(t) + \gamma X_{ct} + \phi_c + \varepsilon_{ct} \quad \text{if} \quad c \in M_r \qquad (2)$$

where the β_r is now a region-specific coefficient and the different βs allow us to rank regions according to their pre-reform familism levels.
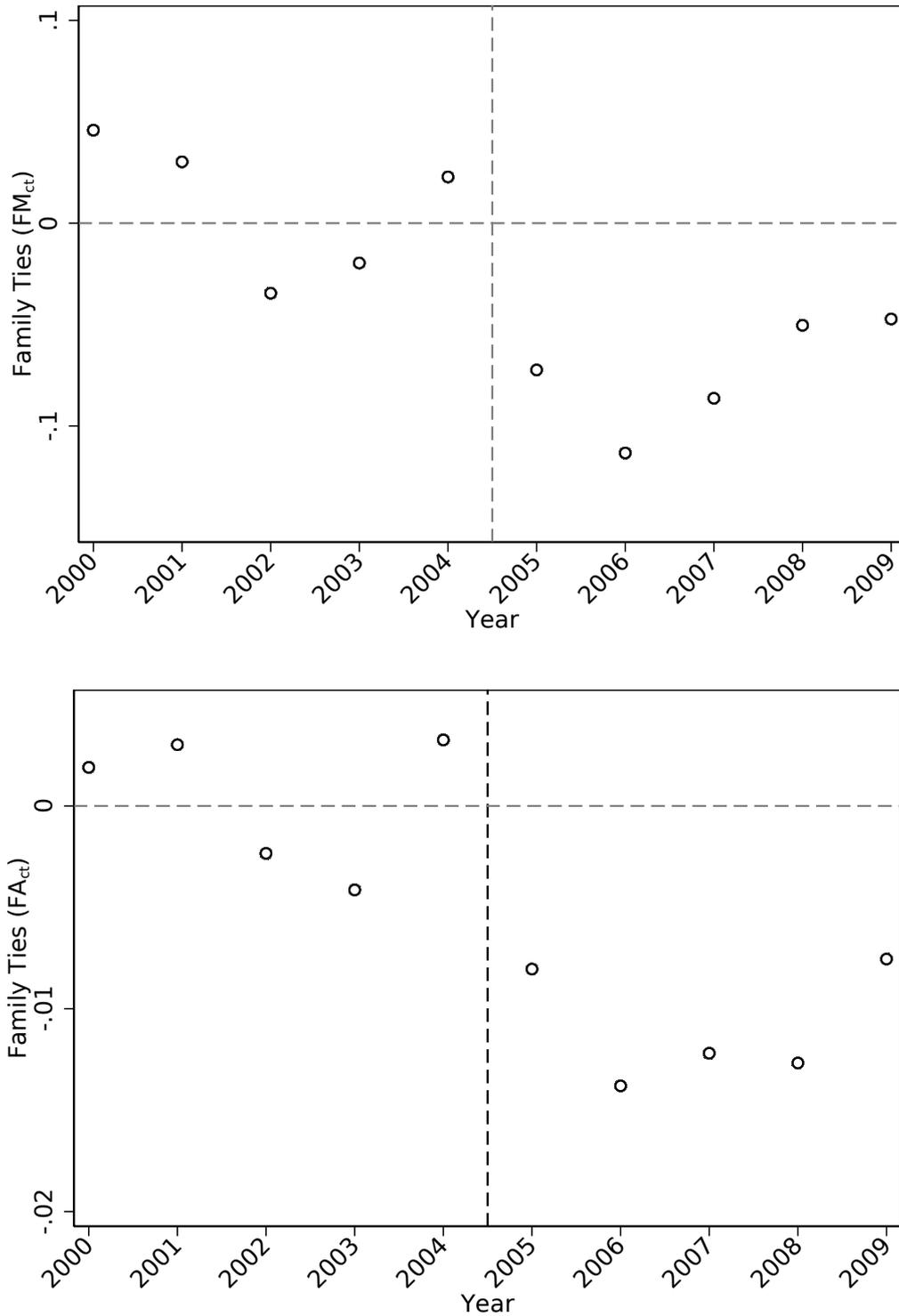
5 Main results

Figure 1 plots the evolution of FA_{ct} (top panel), number of entrants with the same family name and law firm address of another lawyer, and FM_{ct} (bottom panel), number of entrants with the same family name of an already registered lawyer in the same municipality. Both variables are detrended over time. The number of successful candidates we proxy to be related to an already registered lawyer drops starting in 2005 as the first cohort of successful candidates under the new bar exam rules became lawyers.[14] We interpret this result as evidence of familism because before the reform, written exams were graded locally, and senior lawyers could use their professional connections to provide easier access

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constant over time.

[13]The results of the exam are published in June. So, it is unlikely that a lawyer forgoes half of his/her income to avoid yearly fix costs like registration or insurance costs. Further, this should cause stronger results to be present in areas with higher rents, but results in Figure 5 show the opposite.

[14]The first written exam under the new rules took place in 2004, but the oral exam is always scheduled during the next calendar year. Therefore, the first new entrants who took the exam under the new rules entered the profession in 2005.

Figure 1: Drop in family ties of new entrants at the time of reform

Note.—Time series for FA_{ct} and FM_{ct}. We estimate a linear trend in the pre-reform period, and we predict FA_{ct} for all the periods. The figure reports the difference between the observed data and the predicted time trend.

for their relatives taking the bar exam. Instead, this estimate does not capture the help
a senior relative can provide to a junior candidate studying for the bar exam because
the impact of candidate preparation on the exam does not depend on where the exam
is graded. We interpret the drop in Figure 1 as a measure of how many lawyers passed
the exam before the reform because they received help through their family connections.
Those who received help before the reform but would have passed the exam anyway are
not part of the drop.

To confirm the results of Figure 1, Table 2 reports the results for specification (1).
In the upper panel, we use FA_{ct} as the dependent variable. All the specifications show
a large decrease in the number of successful candidates sharing a family name and law
firm address with an already registered lawyer. The estimates range from 0.006 to 0.008
percentage points when a linear trend is assumed, corresponding to a decline of approxi-
mately 20–30% at the mean value of the dependent variable in the year before the reform.
The results are robust to the specification of a quadratic trend. The lower panel of Table
2 reports the results from specification (1), where the outcome variable is FM_{ct}, number
of entrants with the same family name as another lawyer in the same municipality. The
results are similar as those obtained with FA_{ct}, correspond to a decline of approximately
10% at the mean value of the dependent variable, and indicate that a significant share
of the candidates who receive help from a family member do not then work with one, at
least in the long run.[15] Tables A1 and A2 in the appendix show further robustness to
the specification of different time trends respectively for FA_{ct} and FM_{ct}

To understand how many lawyers would have entered the register with family help
after 2004 in the absence of the reform, we perform a counterfactual exercise. We estimate
a linear trend in FA_{ct} and FM_{ct} before the reform, and we extrapolate it for the years
after the reform. In this way, we obtain a counterfactual for the fraction of new entrants
sharing family names and law firm addresses had the reform not been implemented.
Between 2004 and 2016, 98,576 new lawyers entered the profession. Among them, 4118
lawyers shared family names and law firm addresses with another lawyer already in the
register. We estimate that in the absence of the reform, there would have been 1297 more
of these lawyers. The actual number would have been 31% higher in the absence of the
reduction in familism attributed to the reform.

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[15]Since our data are a snapshot of the national register as of July 2017, we can observe only
whether they share a law firm address in 2017, but we cannot observe whether they shared one pre-
viously.

Table 2: Reform effects on familism

	(1)	(2)	(3)

A. Same family name and law firm address (FA_{ct})
Mean of the dependent variable in 2004: 0.026. Standard deviation: 0.24.

Linear trend (N=29360)

	(1)	(2)	(3)
Reform	-0.006*	-0.006*	-0.008*
	(0.003)	(0.003)	(0.004)

Quadratic trend (N= 95025)

	(1)	(2)	(3)
Reform	-0.005*	-0.005*	-0.006
	(0.003)	(0.003)	(0.006)

B. Same family name and municipality (FM_{ct})
Mean of the dependent variable in 2004: 0.10. Standard deviation: 0.48.

Linear trend (N=29360)

	(1)	(2)	(3)
Reform	-0.021**	-0.021**	-0.022**
	(0.009)	(0.009)	(0.009)

Quadratic trend (N= 95025)

	(1)	(2)	(3)
Reform	-0.013*	-0.013*	-0.024**
	(0.007)	(0.007)	(0.011)
Municipality fixed effect	NO	YES	YES
Economic controls	NO	NO	YES

Note.—Standard errors clustered at the district level are in parentheses. * $p < 0.1$, ** $p < 0.05$. These are results for specification 1. ($y_{ct} = \alpha + \beta \cdot Reform_t + f(t) + \gamma X_{ct} + \phi_c + \varepsilon_{ct}$). Control variables: the time trend that is allowed to differ before and after the reform, the population in columns 2, 3 and 4 and the income per capita and fiscal capacity of the municipality in column 4. The time period considered is 2001-2008 when controlling for a linear trend, 1993-2016 when controlling for a quadratic trend and 1998-2016 when including economic controls and a quadratic trend.

Table 3: Heterogeneous effects of the reform with respect to gender

	Male			Female		
	(1)	(2)	(3)	(4)	(5)	(6)

A. Same family name and law firm address (FA_{ct})
Mean of the dependent variable in 2004: male=0.011, female=0.008.
Standard deviation: male=0.19, female=0.10.

Linear trend (N=29224)

	Male			Female		
Reform	-0.003	-0.003	-0.004*	-0.002	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)

Quadratic trend (N= 94583)

	Male			Female		
Reform	-0.002	-0.002	-0.002	-0.002	-0.002	-0.003
	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)

B. Same family name and municipality (FM_{ct})
Mean of the dependent variable in 2004: male=0.038, female=0.031.
Standard deviation: male=0.30, female=0.22   .

Linear trend (N=29224)

	Male			Female		
Reform	-0.009*	-0.009*	-0.010*	-0.004	-0.004	-0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)

Quadratic trend (N= 94583)

	Male			Female		
Reform	-0.008*	-0.008*	-0.010	-0.002	-0.002	-0.006
	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)	(0.006)
Municipality fixed effect	NO	YES	YES	NO	YES	YES
Economic controls	NO	NO	YES	NO	NO	YES

Note.—Standard errors clustered at the district level are in parentheses. * $p < 0.1$. These are results for specification 1. ($y_{ct} = \alpha + \beta \cdot Reform_t + f(t) + \gamma X_{ct} + \phi_c + \varepsilon_{ct}$). Control variables: the time trend that is allowed to differ before and after the reform, the population in columns 2, 3 and 4 and the income per capita and fiscal capacity of the municipality in column 4. The time period considered is 2001-2008 when controlling for a linear trend, 1993-2016 when controlling for a quadratic trend and 1998-2016 when including economic controls and a quadratic trend.

Table 4: Effect of the reform on the number of entrants

	(1)	(2)	(3)
A. New Entrants per 10000 Inhabitants			
Mean of the dependent variable in 2004: 0.81. Standard deviation: 1.84.			
Linear trend (N=29360)			
Reform	-0.004	-0.004	0.012
	(0.041)	(0.041)	(0.039)
Quadratic trend (N= 95025)			
Reform	-0.013	-0.013	0.003
	(0.047)	(0.047)	(0.065)
B. Males: New Entrants per 10000 Inhabitants			
Mean of the dependent variable in 2004: 0.37. Standard deviation: 1.25.			
Linear trend (N=29360)			
Reform	-0.039	-0.039	-0.027
	(0.027)	(0.027)	(0.026)
Quadratic trend (N= 95025)			
Reform	-0.034	-0.034	-0.037
	(0.026)	(0.026)	(0.035)
C. Females: New Entrants per 10000 Inhabitants			
Mean of the dependent variable in 2004: 0.43. Standard deviation: 1.21.			
Linear trend (N=29360)			
Reform	0.038*	0.038*	0.042*
	(0.022)	(0.022)	(0.021)
Quadratic trend (N= 95025)			
Reform	0.023	0.023	0.040
	(0.026)	(0.026)	(0.042)
Municipality fixed effect	NO	YES	YES
Economic controls	NO	NO	YES

Note.—Standard errors clustered at the district level are in parentheses. * $p < 0.1$. These are results for specification 1. ($y_{ct} = \alpha + \beta \cdot Reform_t + f(t) + \gamma X_{ct} + \phi_c + \varepsilon_{ct}$). Control variables: the time trend that is allowed to differ before and after the reform, the population in columns 2, 3 and 4 and the income per capita and fiscal capacity of the municipality in column 4. The time period considered is 2001-2008 when controlling for a linear trend, 1993-2016 when controlling for a quadratic trend and 1998-2016 when including economic controls and a quadratic trend.

Table 5: Reform effects on age at entry into the profession

	Linear trend		Quadratic trend	
	(1)	(2)	(3)	(4)
New entrants average age				
Mean of the dependent variable in 2004: 32.52. Standard deviation: 3.65.				
Reform	0.04	0.08	0.08	0.33
	(0.126)	(0.159)	(0.138)	(0.214)
Observations	26234	21316	26234	21316
Municipality fixed effect	YES	YES	YES	YES
Economic controls	NO	YES	NO	YES

Note.—Standard errors clustered at the district level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. These are results for specification 1. ($y_{ct} = \alpha + \beta \cdot Reform_t + f(t) + \gamma X_{ct} + \phi_c + \varepsilon_{ct}$). Control variables: the time trend that is allowed to differ before and after the reform, the population and, in columns 2 and 4, the income per capita and fiscal capacity of the municipality. The time period considered is 2001-2008 when controlling for a linear trend, 1993-2016 when controlling for a quadratic trend and 1998-2016 when including economic controls and a quadratic trend.

5.1 Heterogeneities

Having established that the number of relatives drops with the decentralization of the grading system, we study if there are heterogeneous effects with respect to gender and if the reform affected other lawyers' characteristics, such as the absolute number of new lawyers, their age, and gender composition.

Table 3 reports result from the same analysis as in table 2, for men and women separately. In columns (1), (2), and (3), the outcomes are FA_{ct} and FM_{ct} computed just for males. Columns (4), (5), and (6) instead look at female lawyers. Although the estimates are imprecise, the effect of the reform decreased the number of male familistic lawyers, more than the number of female familistic lawyers. However, the mean of the dependent variable for males is larger than for females, and the percentage drops are similar, suggesting that familism accounts for the same share of intergenerational trasmission in both groups.

We test if the reform changed the number of new lawyers estimating the same specification as before, equation (1), using the number of new lawyers as an outcome. Panel A of table 4 reports the results and shows that the reform did not change the number of new lawyers. Although there is no quota on the number of new lawyers, our evidence shows that exam committees might have a focal point of how many people can pass the bar exam every year. In fact, the oral part of the exam is still locally graded, hence this is where examiners can regulated the number of new entrants. When it was possible

for senior lawyers to help their younger relatives, those relatives took the spot of non-relatives in the bar exam, as documented in the previous section. Next, we test if the reform changed the gender composition of new lawyers. It could be that females, more than males, used to be discouraged by the unfair familist practices that plagued the bar exam and, because of this, did not even try to take the exam. Panel B shows that the reform did not affect the number of males new lawyers. Panel C shows instead that the number of female new lawyers increased by about 5-10% following the implementation of the reform. This evidence is in line with the results reported in Table 3. However, these estimates are imprecise and not robust to different specifications of the time trend.

Finally, we assess if the reform has changed the age at which people become lawyers. Law school graduates aspiring to become lawyers might fail in their first attempt at the bar exam. Presumably, the age at which one becomes a lawyer measures the number of attempts needed to pass the bar exam, which proxies candidates' quality. Table 5 shows that the reform had no effect on the profession's age at entry for lawyers.
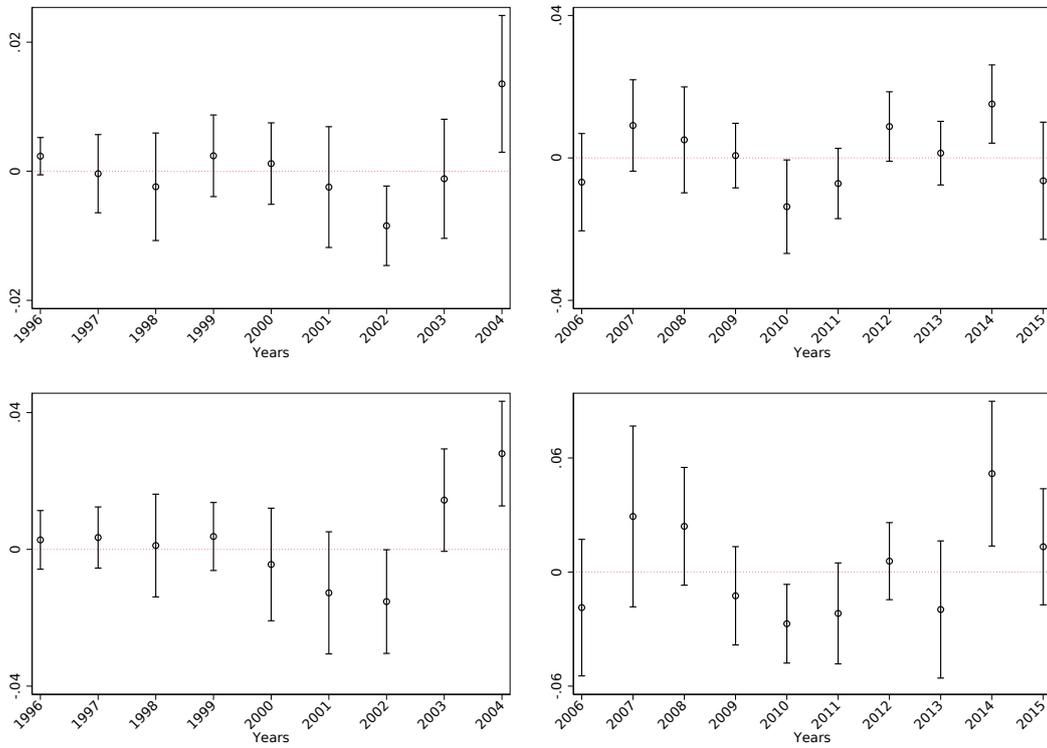
5.2 Robustness

Figure 2 plots the estimations of a placebo test for the results in Table 2 that falsifies the year of the reform. The x-axis shows the false year of reform, and the y-axis shows the relative placebo coefficient and its 95% confidence interval. The figure on the left reports the results using all the pre-reform periods, while that on the right reports those for all the post-reform periods. Overall, the results are not significant and much smaller in size than the estimates in Table 2. Tables A1 and A2 confirm the robustness of our main findings with respect to different specification of the time trend.

Appendix E replicates the main results of the study using an alternative definition of the outcome variable. We divide the outcome variables used so far, the number of new entrants with the same family name and law firm address as an already registered lawyer (FA_{ct}) and the number of new entrants with the same family name as a lawyer already registered in the same municipality (FM_{ct}), by the number of newly registered lawyers in year t and municipality c. This alternative definition of the outcome produce results qualitatively equivalent to those of the main section of this study.

5.3 Where to target monitoring efforts: rents or social capital?

The literature generally holds that whether intergenerational transmission is desirable is debatable (e.g., Bowles, Gintis and Groves (2009); Björklund, Lindahl and Plug (2006); Sacerdote (2011)); however, concerning a regulated profession, it may be inefficient if it

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Figure 2: Placebo estimates

Note.—Placebo estimates from specification (1) for FA_{ct} (above) and FM_{ct} (below). In the left (right) panels, we run specification (1) ($y_{ct} = \alpha + \beta \cdot Reform_t + f(t) + \gamma X_{ct} + \phi_c + \varepsilon_{ct}$) using each year as the false year of the reform with all the pre-reform (post-reform) periods. The estimates are not significant and small in size, confirming the robustness of the results in Table 2.

relates to familism designed to favor the entrance of relatives in the profession. Clearly, our results follow this case and reveal inefficient behavior that lowers the standard in the profession. This consideration naturally raises another question, especially for a monitoring authority interested in reducing familism: where should we look for familism?

The existing literature has shown that misbehavior prevails where rents are higher (e.g., Brollo et al. (2013)) and social capital is lower (e.g., Durante, Labartino and Perotti (2011)). In our setting, this phenomenon is particularly interesting: we estimate rents from being a lawyer in every Italian region as the net salary markup over the average salary of a graduate person in the same region including those without an employment and correcting the salary of a lawyer with a measure of possible tax evasion.[16][17] Figure 3 plots the correlation between rents from being a lawyer against different measures of social capital. Clearly, rents and social capital are positively correlated, generating a tradeoff in monitoring efforts for policymakers interested in reducing familism. Although the fact that rents are lower in Southern region could be surprising, it is consistent with the fact that in the South of Italy, regulated occupations represent a higher share of total employment than in the North (Mocetti, Rizzica and Roma (2021)): the higher competition in regulated occupation is one of the possible explanation for the lower level of rents in the South.

Table 6 reports the estimated familism for each region separately under specification (2). We prefer to use FA_{ct} as the dependent variable because it results in more precise estimates (it also considers familism towards relatives of lawyers who do not join the law firm of the senior lawyer, so it has greater variation), but the results are qualitatively similar using FM_{ct}, as reported in appendix D. In Figures 4 and 5, we plot the estimated degree of familism against measures of social capital and rents from being a lawyer. Figure 4 shows that lower levels of social capital are associated with higher familism, while Figure 5 suggests that rents are negatively associated with it. This finding may be attributed to the different levels of variation in social capital and rents. However, the variation in rents is remarkably large, as they range between the lowest level of 1,208 EUR in Calabria and the highest of 5,457 EURO in Lombardia, with a standard deviation of 1,331 EUR.

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[16]An alternative comparison would consider the salary markup over a graduate from law school in 2003. Some versions of this type of data are available for only a few schools; they cover incomes for a few years after graduation, and they do not include the potential rents of a full career, which is particularly important in a profession such as lawyers in Italy for which returns at early stages of the career are very low (or null).
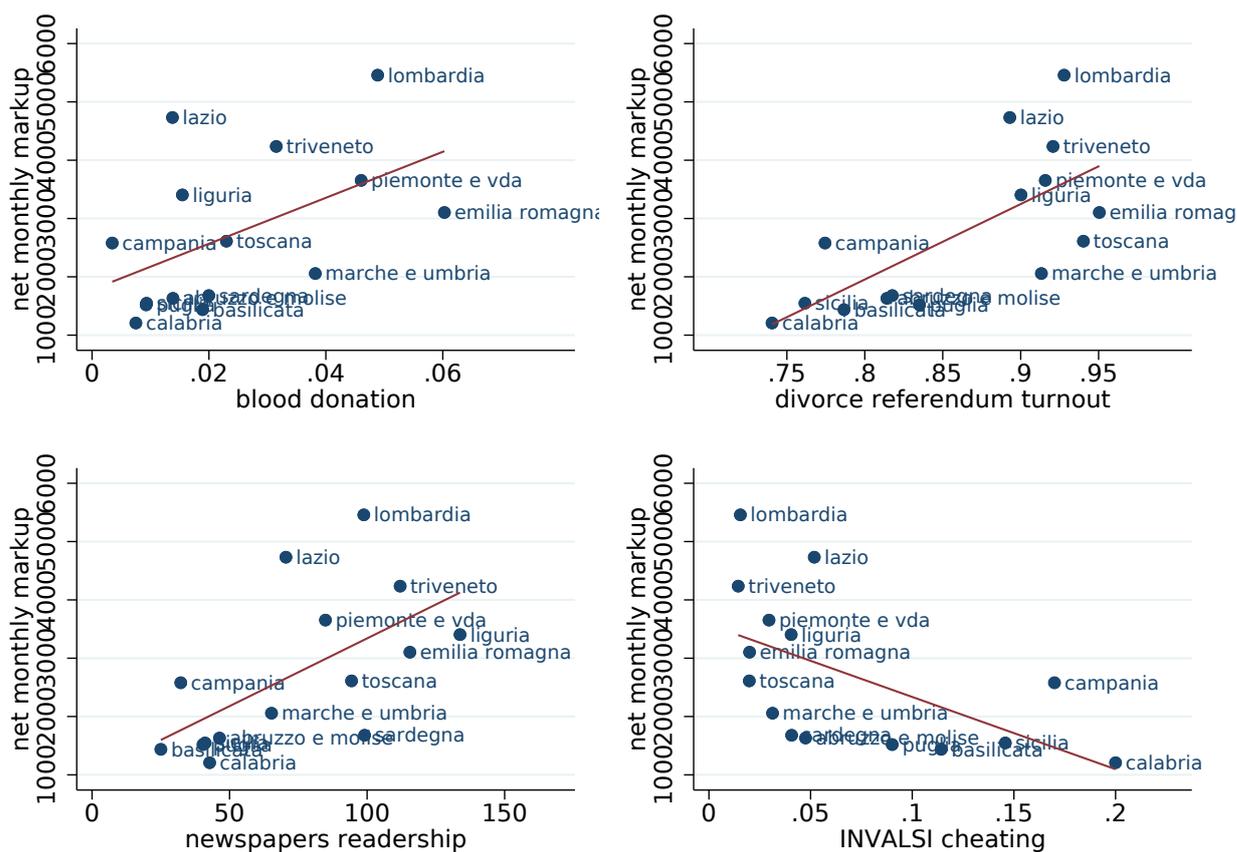
[17]Data for the average salary of a graduate are from the publicly available version of the Labour Force Survey. Given the lack of estimate on labour income tax evasion at regional level, we use a regional estimate of the tax evasion on property tax in 2012 produced by the Italian Revenue Agency as measure of tax evasion.

Table 6: Regional estimates of familism

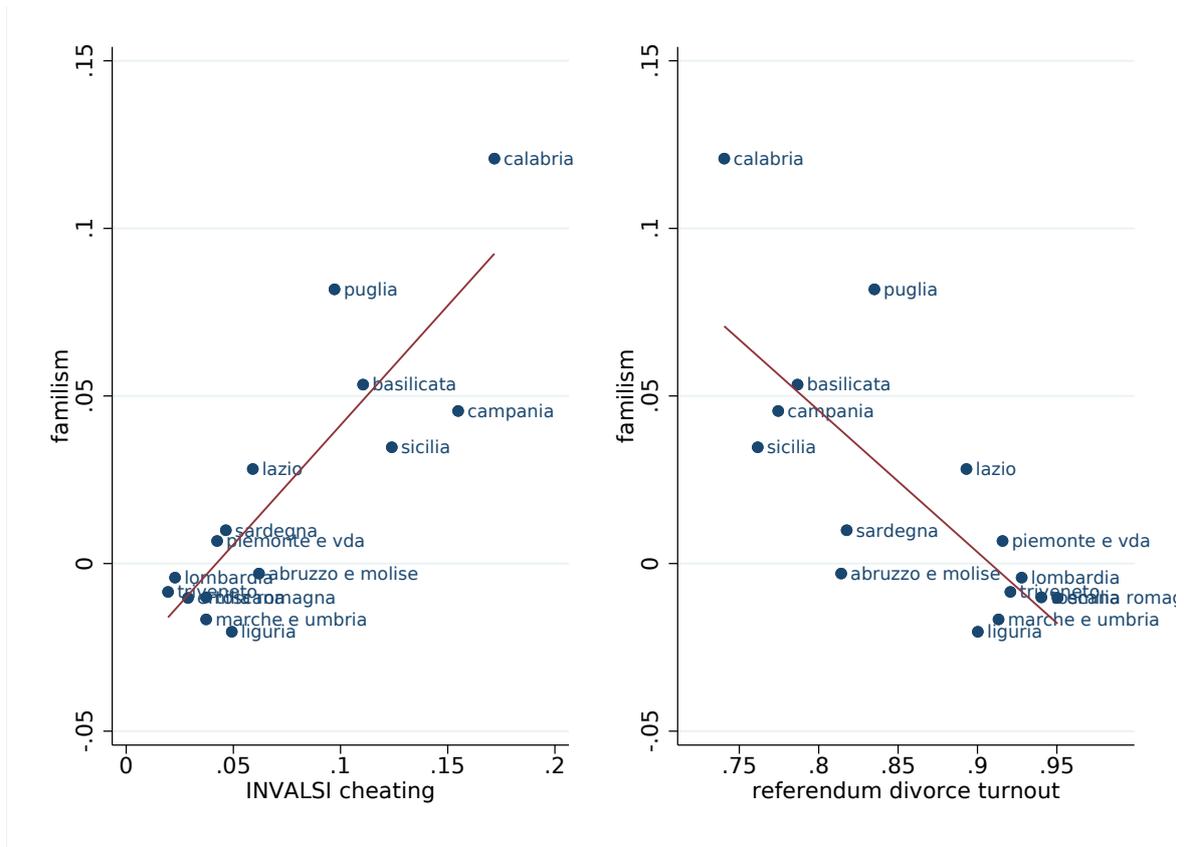| Region	Coefficient	Standard Error	p-value	Towns	Pre-Reform Average
Calabria	-0.121	0.043	0.00	323	0.23
Puglia	-0.082	0.028	0.00	221	0.19
Basilicata	-0.053	0.062	0.39	117	0.10
Campania	-0.046	0.039	0.24	453	0.22
Sicilia	-0.035	0.020	0.09	281	0.12
Lazio	-0.028	0.035	0.41	176	0.12
Sardegna	-0.010	0.024	0.68	157	0.07
Piemonte e VdA	-0.007	0.012	0.59	175	0.03
Abruzzo and Molise	0.003	0.017	0.86	204	0.05
Lombardia	0.004	0.007	0.55	707	0.03
Triveneto	0.008	0.011	0.45	177	0.02
Toscana	0.010	0.021	0.64	190	0.08
Emilia Romagna	0.010	0.008	0.21	235	0.03
Marche and Umbria	0.017	0.014	0.25	169	0.05
Liguria	0.020	0.018	0.29	73	0.03

Note.— We run specification (2) for town c in region d: $FM_{ct} = \alpha + \beta_d \cdot Reform_t + f(t) + \gamma X_{ct} + \phi_c + \varepsilon_{ct}$ if $c \in d$. For each region, column 1 reports the estimated β, column 2 its standard error, column 3 its p-value and column 4 the number of towns used in the estimation (i.e., those that have registered lawyers). We merge some geographically nearby regions because they do not have enough towns to provide an adequate level of precision in the estimates. For the same reason, we prefer FM_{ct} to FA_{ct}, although the results are qualitatively the same. Then, we merge Piemonte and Valle d'Aosta, Abruzzo and Molise, Marche and Umbria, Veneto and Friuli-Venezia Giulia and province of Trento. The reform did not change the rules in the province of Bolzano.

Figure 3: Social capital and rents from the license

Note.—Correlation of social capital and rents from being a lawyer across Italian regions. We estimate rents from being a lawyer in every Italian region as the gross salary markup over the average salary of a graduate person in the same region including unemployment and a correction for the estimate tax evasion in the region. We measure social capital with the most commonly used indicator of social capital in the literature for Italy: levels of newspaper readership, levels of blood donation, political turnout in the divorce referendum and estimated cheating on the Istituto Nazionale per la Valutazione del Sistema Educativo di Istruzione e di Formazione (INVALSI) test in elementary school (sources: Cartocci (2007); Guiso, Sapienza and Zingales (2004) and Invalsi). Overall, lower social capital is associated with lower rents from the license.
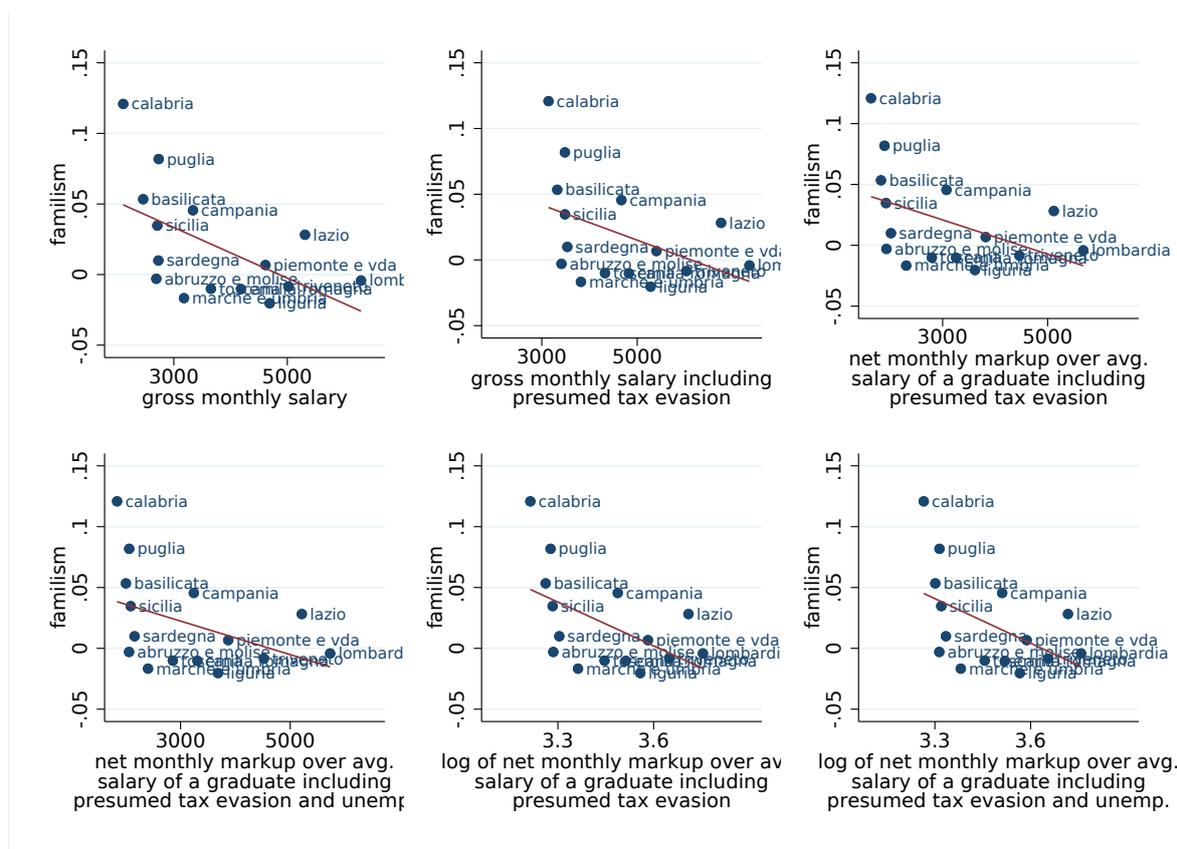
Figure 4: Social capital and familism

Note.— Correlations between measures of social capital and familism across Italian regions. We consider the indicators of social capital most commonly used in the literature: levels of blood donations, political turnout, estimated cheating on the Invalsi test at elementary school and newspaper readership. Familism for region r is measured as $-\beta_r$ from specification 2. Overall, familism is higher in areas with lower levels of social capital.

6 Conclusions

The intergenerational transmission of jobs between relatives is common across countries (Corak and Piraino (2011); Lentz and Laband (1990)), and it is generated by different factors (e.g., Björklund, Lindahl and Plug (2006); Aina and Nicoletti (2018); Mocetti (2016); Mocetti, Roma and Rubolino (2020)). In regulated professions, an additional driver of intergenerational transmission is familism, i.e., the misbehavior of people already in a profession intended to favor relatives on the licensing exam.

In this paper, we measure the extent to which familism accounts for intergenerational transmission in regulated professions. We examine the effects of the 2004 reform of the Italian bar exam. Before 2004, local committees graded the exams, whereas from 2004 onwards, exams were randomly assigned to external committees. We find that in Italy, approximately 10–30 percent new lawyers with a family tie entered the register with

Figure 5: Rents from the license and familism

Note.— Correlations between rents from being a lawyer and familism across Italian regions. We estimate rents from being a lawyer in every Italian region with four different measures. In the top right panel we consider the gross salary of a lawyer, in the top left panel we correct the gross salary of a lawyer for the presumed tax evasion, in the bottom right panel we use the markup of the salary of a lawyer corrected for the presumed tax evasion over the average salary of a graduate person in the same region and in the bottom left we correct the latter measure considering also unemployed graduate. Overall, familism is higher in areas with lower levels of rents, suggesting that weak social norms are more important determinants of familism than economic incentives.

the help of a relative before 2004. Since 2005, approximately 4118 lawyers entered the profession and worked in the same law firm as a relative. We conclude that familism at the licensing stage is an important determinant of intergenerational transmission.

The presence of such a high level of familism is undesirable because new entrants who enter with the help of a family member lower the standards of the profession, and this phenomenon naturally calls for analysis of where familism should be sought. The literature suggests that low levels of social capital and high rents from a license may be two drivers of familism. In our setting, rents and social capital are positively correlated, generating a possible tradeoff for a policymaker seeking to reduce familism. We estimate the effect of the reform at regional level, and we find that familism is associated with lower levels of social capital, despite the fact that low social capital areas are those with

lower rents from the license.

The results of this paper may be relevant in shaping institutions controlling access to regulated professions. However, care should be taken in generalizing the results to other countries: different levels of social capital, different strengths of family ties, different quality of institutions and a number of other factors can undermine the validity of the results across countries.

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A Robustness of the parametric event study

Table A1: Robustness to time trend specifications for FA_{ct}

Dep. Var.:**Same family name and law firm address (FA_{ct})**
Mean of the dependent variable in 2004: 0.026. St. deviation: 0.24.

	(1)	(2)	(3)
Trend: quadratic before and cubic after the reform (N=95025)			
Reform	-0.008**	-0.008**	-0.009*
	(0.003)	(0.003)	(0.005)
Trend: cubic before and quadratic after the reform (N=95025)			
Reform	-0.007*	-0.007*	-0.012**
	(0.004)	(0.004)	(0.005)
Different cubic trends before and after the reform (N=95025)			
Reform	-0.006	-0.006	-0.022**
	(0.004)	(0.004)	(0.009)
Different linear trends before and after the reform (N=80353) Time range: 9 years before/after the reform.			
Reform	-0.006**	-0.005**	-0.004
	(0.002)	(0.002)	(0.003)
Municipality Fixed Effect	NO	YES	YES
Economic Controls	NO	NO	YES

Note.—Standard errors clustered at the district level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. These are results for specification 1. ($y_{ct} = \alpha + \beta \cdot Reform_t + f(t) + \gamma X_{ct} + \phi_c + \varepsilon_{ct}$). Control variables: the time trend, the population in columns 2, 3 and 4 and the income per capita and fiscal capacity of the municipality in column 4.

Table A2: Robustness to time trend specifications for FM_{ct}

Dep. Var.:**Same family name and law firm address (FM_{ct})**
Mean of the dependent variable in 2004: 0.010. St. deviation: 0.48.

	(1)	(2)	(3)
Trend: quadratic before and cubic after the reform (N=94583)			
Reform	-0.030***	-0.030***	-0.041***
	(0.010)	(0.010)	(0.014)
Trend: cubic before and quadratic after the reform (N=94583)			
Reform	-0.035**	-0.035**	-0.056***
	(0.014)	(0.014)	(0.017)
Different cubic trends before and after the reform (N=94583)			
Reform	-0.030**	-0.030**	-0.077***
	(0.013)	(0.013)	(0.021)
Different linear trends before and after the reform (N=79979) Time range: 9 years before/after the reform.			
Reform	-0.005	-0.008	-0.007
	(0.005)	(0.005)	(0.007)
Municipality Fixed Effect	NO	YES	YES
Economic Controls	NO	NO	YES

Note.—Standard errors clustered at the district level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. These are results for specification 1. ($y_{ct} = \alpha + \beta \cdot Reform_t + f(t) + \gamma X_{ct} + \phi_c + \varepsilon_{ct}$). Control variables: the time trend, the population in columns 2, 3 and 4 and the income per capita and fiscal capacity of the municipality in column 4.
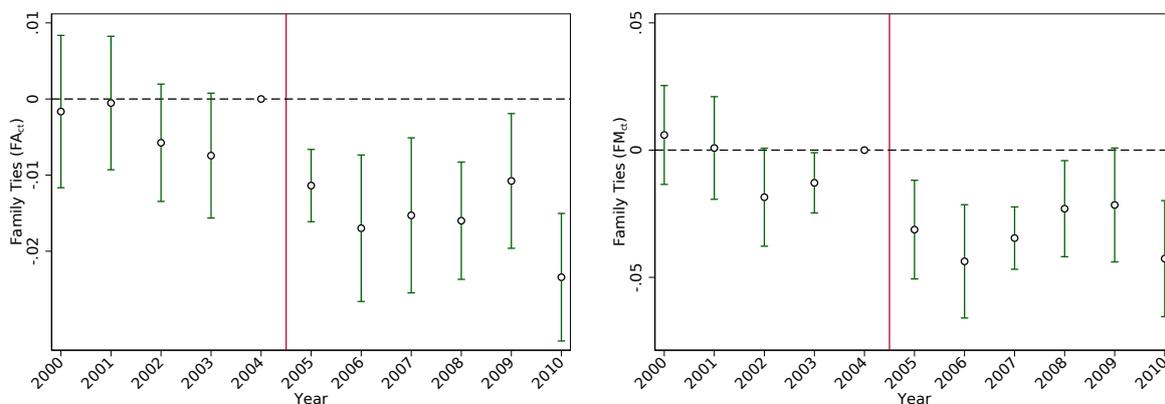
B   Nonparametric event study

This section considers a more flexible nonparametric event study model. We analyze the coefficients on various indicator variables for time relative to the year of implementation of the reform (2004). The advantage of this approach is that it allows us to visually assess the pattern of outcomes relative to the year 2004. The basic specification takes the form,

$$y_{ct} = X_{ct}\gamma + \sum_{t=1999}^{2003} \mu_t + \sum_{t=2005}^{2016} \mu_t + \phi_c + \varepsilon_{ct} \tag{3}$$

where y_{ct} is detrended FA_{ct} or FM_{ct}, X_{ct} is a vector control variables, μ_t are coefficients on dummy for each year t and ϕ_c represents the municipality fixed effects. The set of coefficients μ_t for $t \neq 2004$ describe the evolution of y_{ct} before and after the reform with respect to the last year before the reform implementation.

Figure B1 shows the impact of the 2004 reform on our two measures, FA_{ct} and FM_{ct}, of family ties. For each outcome, we plot the estimated coefficients on event time (μ_ts) from the nonparametric event study regression (equation (3)).

Figure B1: Nonparametric event study

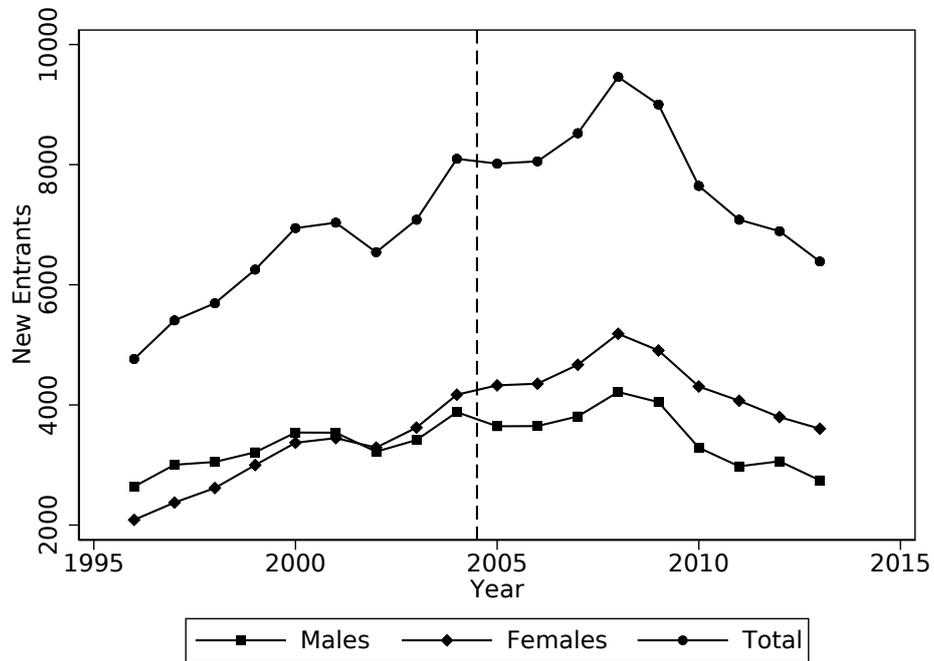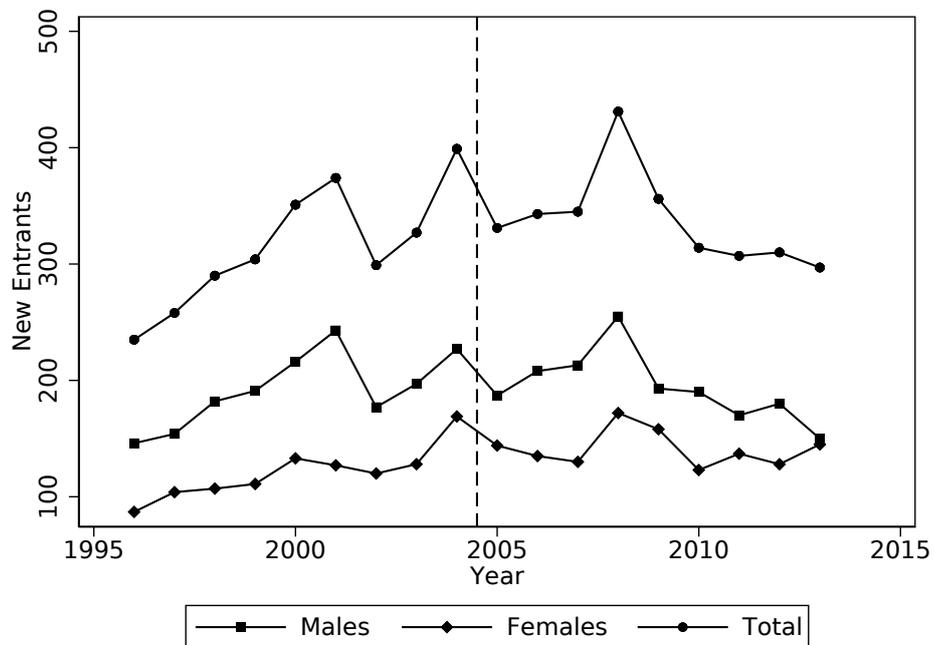# C Evolution of entrants over time

Figure C1: New entrants

Figure C2: New entrants with a family tie

D   Ranking FA_{ct}

Table A3: Regional estimates of familism with FA_{ct}

Region	Coefficient	Standard Error	p-value	Towns	Pre Reform Average
Campania	-0.027	0.026	0.29	453	0.06
Puglia	-0.023	0.013	0.07	221	0.04
Calabria	-0.027	0.011	0.04	323	0.04
Sicilia	-0.017	0.013	0.17	281	0.03
Sardegna	-0.010	0.013	0.51	157	0.02
Abruzzo and Molise	-0.005	0.007	0.53	204	0.02
Triveneto	0.002	0.010	0.83	177	0.02
Piemonte e VdA	0.001	0.010	0.96	175	0.02
Lombardia	0.001	0.003	0.85	707	0.01
Emilia Romagna	0.002	0.005	0.60	235	0.01
Liguria	0.003	0.007	0.62	73	0.01
Marche and Umbria	0.009	0.009	0.33	169	0.01
Lazio	0.009	0.011	0.43	176	0.02
Basilicata	0.010	0.037	0.77	117	0.03
Toscana	0.020	0.014	0.15	190	0.02

Note.— We run specification (2) for town c in region d: $FA_{ct} = \alpha + \beta_d \cdot Reform_t + f(t) + \gamma X_{ct} + \phi_c + \varepsilon_{ct}$ if $c \in d$. For each region, column 1 reports the estimated β, column 2 its standard error, column 3 its p-value and column 4 the number of towns used in the estimation (i.e., those that have registered lawyers). We merge some geographically nearby regions because they do not have enough towns to provide an adequate level of precision in the estimates. For the same reason, we prefer FM_{ct} to FA_{ct}, although the results are qualitatively the same. Then, we merge Piemonte and Valle d'Aosta, Abruzzo and Molise, Marche and Umbria, Veneto and Friuli-Venezia Giulia and province of Trento. The reform did not change the rules in the province of Bolzano.

E Alternative definition of the outcome

This appendix shows the robustness of our main findings with respect to alternative definitions of family ties.

E.1 Familist per new entrants

As for our main results, we measure the family ties of new entrants in each Italian municipality with two measures. First, we compute the fraction of new entrants sharing the family name and law firm address as an already registered lawyer. Second, we compute the fraction of new entrants sharing a family name with a lawyer already registered in the same municipality. Formally, our proxies for the family ties of new entrants in year t and municipality c are,

$$\tilde{FA}_{ct} = \frac{\text{new entrants with the same family name and law firm address as an already registered lawyer}_{ct}}{\text{new entrants}_{ct}}$$

$$\tilde{FM}_{ct} = \frac{\text{new entrants with the same family name as a lawyer already registered in the same municipality}_{ct}}{\text{new entrants}_{ct}}.$$

Compared to our main outcome measures, $\tilde{FA}_{ct}$ and $\tilde{FM}_{ct}$ condition on municipality c having at least one new registered lawyer in year t. Although this might be reasonable to capture the share of lawyers with family ties, it can mismeasure shifts in familism. For example, consider a hypothetical town where all new lawyers must have family ties to pass the bar exam, the reform changes this, and by making it harder to pass the bar exam without help from a relative, nobody in the same town passes the bar exam anymore. $\tilde{FA}_{ct}$ and $\tilde{FM}_{ct}$ would not capture this behavior because they are defined conditionally on new lawyers entering into the profession.

Table A4 present the main results using the alternative definitions of familism defined by $\tilde{FA}_{ct}$ and $\tilde{FM}_{ct}$.

Table A4: Robustness of main results: $\tilde{FA}_{ct}$ and $\tilde{FM}_{ct}$

	(1)	(2)	(3)	(4)
Same family name and law firm address ($\tilde{FA}_{ct}$)				
Mean of the dependent variable in 2003: 0.026. Standard deviation: 0.116.				
Linear trend				
Reform	-0.014***	-0.014***	-0.015***	-0.010**
	(0.003)	(0.003)	(0.003)	(0.004)
Quadratic trend				
Reform	-0.007*	-0.007*	-0.007*	-0.010
	(0.004)	(0.004)	(0.004)	(0.007)
Same family name and municipality ($\tilde{FM}_{ct}$)				
Mean of the dependent variable in 2003: 0.103. Standard deviation: 0.217.				
Linear trend				
Reform	-0.021***	-0.021***	-0.017**	-0.032**
	(0.007)	(0.007)	(0.008)	(0.012)
Quadratic trend				
Reform	-0.021***	-0.021***	-0.017**	-0.032**
	(0.007)	(0.007)	(0.008)	(0.012)
Observations	26355	26355	26355	21412
Municipality Fixed Effect	NO	NO	YES	YES
Economic Controls	NO	NO	NO	YES

Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. These are results for specification 1. ($FA_{ct} = \alpha + \beta \cdot Reform_t + f(t) + \gamma X_{ct} + \phi_c + \varepsilon_{ct}$). Controls: time trend that is allowed to differ before and after the reform, population in columns 2,3 and 4 and income per capita and fiscal capacity of the municipality in column 4. New entrants with family ties decline drastically following the reform.

F Measurement error and attrition

F.1 Measurement error

FA_{ct} and FM_{ct} are proxies for family ties and may be affected by measurement error. FM_{ct} is less precise and includes lawyers that have the same surname and operate in the same municipalities but are not relatives. On the contrary, FA_{ct} is definitely more precise, but fails in measuring family ties between relatives that do not work together. Here we briefly discuss how those measurement errors may affect the results.

FM_{ct} may produce false positives, i.e. it identifies family ties between lawyers with the same surname operating in the same municipalities that are not relatives. In this case, the measurement error does not influence the results if the number of false positives does not jump with the reform. Indeed, FM_{ct} can be decomposed as $FM_{ct} = t_{ct} + f_{ct}$ where t_{ct} are the true positives and f_{ct} the false positives. Our empirical exercise measures the variation in FM_{ct} at the time of the reform. Then, assuming that f_{ct} does not change discontinuously at the time of the reform, the variation in FM_{ct} is an unbiased estimate of the variation in t_{ct}.

FA_{ct} may also produce false negatives, i.e. it does not identify family ties between relatives that do not work together. In this case, the measurement error affects the results if the effect of the reform on lawyers with a correctly specified family tie and lawyers who are false negatives is not the same. Admittedly, we cannot measure the number of false negatives and assess whether the reform affected them differently. However, note that the false negatives of the measure FA_{ct} are considered as lawyers with family ties when using FM_{ct}. Further, the results that we obtain using FM_{ct} are similar to those obtained with FA_{ct}: this suggests that the false negatives are either very few or receive the same effect due to the reform than those with a true family tie.

F.2 Attrition

As mentioned in the main text, our data are a snapshot of the Italian register of lawyers as of July 2017. This generates attrition on the variables FA_{ct} and FM_{ct} related to the structure of the data, and FA_{ct} and FM_{ct} cannot be interpreted as temporal measures of familism for at least three different reasons that may generate mechanical trends. Hereafter we report them, and then we explain under which conditions FA_{ct} and FM_{ct} can still be exploited to measure the variation in entrants with family ties at the time of the reform.

1. The growth in the number of lawyers that occurred in the 2000s may increase the chance of sharing a family name with another lawyer.

2. Because the data are a snapshot of the national register as of July 2017, the variables FA_{ct} and FM_{ct} may underestimate family ties the further we move away from 2017. For example, in year t, some senior lawyers may be active and contribute to the numerator of FA_{ct} and FM_{ct} that were no longer active in 2017. Unfortunately, we do not observe them in the data. The amount of these missing links increase the further t is from 2017, and the underestimation of the numerators of FA_{ct} and FC_{ct} increase the further t is from 2017.

3. Conversely, because the data are a snapshot of the national register as of July 2017, the variable FA_{ct} may overestimate family ties the further we move away from 2017. For example, suppose lawyers tend to join larger law firms over time. This would increase the chance of sharing a family name with a colleague and identify family ties as of July 2017 that did not exist when a lawyer entered the register.

These considerations show that our proxy for family ties may be affected by a mechanical trend that can be positive or negative depending on whether the effects in 1 and 2 prevail over the effect in 3 or not. Under some mild assumptions, FA_{ct} and FM_{ct} measure the variation in entrants with family ties at the time of the reform. In particular, assuming that the mechanical attrition is continuous at the time of the reform and conditional on a correct specification of the time trend, the break in the trend of entrants with family ties is the effect of the reform. Consequently, it is crucial to specify the time trend properly in the empirical model. In Section A we check different degrees of the time trend, allowing it to be different before and after the reform. Overall, results are significant and very similar to the main estimates in Table 2.

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