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Testing the Lawyer-Induced Litigation Hypothesis in Europe

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Utilizing a European panel dataset, we contribute to the scant empirical literature on the lawyer-induced litigation hypothesis. To address endogeneity problems that arise when estimating the effect of the number of lawyers on civil litigation rates, we use two strategies. We first estimate our model by means of the 2SLS procedure. Second, we exploit the instrumental variable approach based on the linear GMM estimator of Arellano and Bond. The estimations result in a positive and significant effect of lawyers that is robust across the different model specifications and estimation methods in which we address endogeneity. In criminal litigation, where lawyers cannot induce demand, we find no such positive relation between lawyers and litigation.

JEL codes: K41, J44

Keywords: Lawyers, Supplier-Induced Demand, Lawyer-Induced Litigation, Litigation

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1. Introduction

In his seminal work “Institutions, institutional change and economic performance”, North (1990) puts forth that court efficiency plays a key role in explaining variation in the differential performance of economies. Given that well-functioning judiciaries are a pivotal institutional condition for economic progress (Ramello and Voigt 2012, Posner 1998, North 1990), the functioning of courts has increasingly become the subject of empirical scrutiny among (institutional) economics scholars. This surge is to a great extent motivated by the fact that many judiciaries are faced with an increased litigation rate¹, which aggravates court delays and hence undermines public confidence in the court system (e.g. Haig and Stone 1993, Johnson 1997, Robel 1992, Westover 1958, Beale 1994, Felli et al. 2008, Geyh 1993, Buscaglia and Dakolias 1999).

Prompted by the development of the first economic analyses of settlement and litigation by Landes (1971), Posner (1973) and Gould (1973), law and economics scholars have persistently researched the causes of litigation (Bielen, Marneffe and Vereeck 2015). “In practice, a lawsuit represents a citizen’s choice to invoke the power of the judiciary to settle a dispute rather than to use other means of conflict resolution” (Yates, Davis and Glick 2001, p128). Therefore, changes in the factors that influence the latter decision may cause a proliferation of litigation.

Recently, the discussion on excessive litigiousness has moved to the role of lawyers. More specifically, it is hypothesized that an overabundance of lawyers could incentivize frivolous lawsuits. Strong competition among lawyers can cause them either to lower fees or to persuade clients with low(er) probability cases to file suit. The latter effect, which is called supplier-induced demand, stems from the information asymmetry between legal professionals and their clients, who mainly

¹ Scholars regularly refer to a “litigation explosion”, “hyperlexis” or “adversary society”.

rely on legal advice in their decision to file suit (Cooter and Ulen 2013, Ginsburg and Hoetker 2006, D'Agostino, Sironi and Sobbrío 2012, Zhou 2008, Nelken 2004).

In general, countries have embraced the advantages of fostering competition in the professional service market, particularly in the market for lawyers (Garoupa 2008). Notwithstanding the benefits of competition, excessive competition can induce litigation, which in turn can prompt court backlogs and delays. Given the vital role of lawyers in the strength of the rule of law and the fact that the legal profession is pivotal for institutional quality (Grajzl and Murrell 2005), the hypothesis that lawyers could intentionally induce litigation is worrisome.

Concerns about the growing litigiousness in many European countries and the growing awareness that observed increases in lawyers might further induce litigation rates lead us to the following research question: does a higher lawyer density induce higher civil litigation rates in Europe? To answer this question effectively, we need to address possible endogeneity issues which pose a significant problem for empirical estimation. Furthermore, since the quest for appropriate instruments is challenging, empirical evidence of lawyer induced litigation remains scarce. Although Carmignani and Giacomelli (2010), Buonanno and Galizzi (2014) and Mora-Sanguinetti and Garoupa (2015) find evidence that lawyers induce litigation in Italy and Spain, a cross-country analysis remains non-existent. Our first contribution to the existing literature, therefore, is an analysis of the effect of the number of lawyers on litigation using a panel of countries. For this purpose, we constructed a dataset of 30 countries of the Council of Europe for the years 2006, 2008, 2010, 2012 and 2014. We exploit the panel structure of the dataset to address endogeneity concerns resulting from anticipated unobserved and time-invariant country-level heterogeneity.

Our second contribution to the literature is a methodological one, since we explicitly tackle the problem of reverse causality between lawyers and litigation using several strategies. Following Mora-Sanguinetti and Garoupa (2015), we use

the number of law schools at a point in time that precedes the timespan of our dataset as an exogenous source of variation for the number of lawyers. Additionally, we propose a new instrument: the number of law graduates (again lagged, to ensure exogeneity), which should be highly correlated with the number of lawyers and uncorrelated with unobserved factors affecting litigation rates. Finally, we tackle the anticipated reverse causality between lawyers and litigation by exploiting the instrumental variable approach based on the linear GMM (General Method of Moments) estimator of Arellano and Bond (1991). GMM estimation has become increasingly popular among researchers because it can improve upon the traditional 2SLS approach. GMM allows for efficient estimation in the presence of heteroscedasticity (Baum, Schaffer and Stillman 2003), avoids the loss of observations when using lags of (internal) instruments (Dimitrova-Grajzl et al. 2016, Roodman 2009) and circumvents the necessity of finding appropriate exogenous instruments. To date, the Arellano-Bond estimator has not yet been used to address the endogeneity concerns between lawyers and litigation rates despite its advantages and popularity in other research areas.

We find that the number of lawyers per 10,000 inhabitants has a positive and statistically significant effect on litigation rates. *Ceteris paribus*, an increase of the lawyer density with 1 percent, increases litigation rates on average with 1.16 to 1.47 percent. The positive and significant effect of lawyers is robust across the different model specifications and estimation methods in which we address endogeneity.

The paper is structured as follows. Section 2 provides a conceptual framework of the lawyer-induced litigation hypothesis and our empirical model. Section 3 presents the data set and the variables included in the regression models. Section 4 discusses the empirical strategy and presents the results. Section 5 presents some robustness checks and Section 6 concludes with a discussion of the empirical results.

2. Conceptual framework and empirical model

This paper examines the lawyer-induced litigation hypothesis in a European cross-country setting. To this end, we posit the following basic model:

$$\text{Log}(\text{Litigation})_{it} = \beta \log(\text{Lawyers})_{it} + \sum_{c=1}^c \log(\text{Controls})_{it}^c + \varepsilon_{it} \quad (1)$$

Where Litigation_{it} is the number of incoming civil (and commercial) litigious cases at first instance courts as measured in country i in year t . Controls_{it}^c ($c = 1, \dots, 10$) is a set of controls that is expected to affect *Litigation* (i.e. population, population density, number of judges, legal aid, GDP per capita, change in GDP per capita, number of courts, congestion, use of contingency fees, availability of legal insurance) and ε_{it} is the error term.

With the model depicted by equation (1), we aim to empirically examine the relationship between the number of lawyers and litigation rates. The existing literature has postulated that a higher lawyer density increases the demand for court services. This stems from the fact that lawyers achieve a target income to which they adjust their work, i.e. the target income hypothesis (Carmignani and Giacomelli 2010). Accordingly, the possible effect of more lawyers and thus more substantial competition among lawyers is twofold. First, the pricing effect predicts that lawyers might feel forced to lower their fees to assure that they have a certain level of work. Second, lawyers might be increasingly pressured to take on cases that previously would not have been brought to court (Cooter and Ulen 2013). In other words, lawyers enduring intense competition could exploit their informational advantage and persuade parties to go to court even when their chance of winning is low. Given that legal advice is a credence good, most parties depend on advice from their lawyer (see e.g. Posner 2014) and the exploitation of informational advantage seems the most plausible explanation for an alleged lawyer-induced litigation

effect. The pricing effect (that is, litigation is induced because lawyers decrease their fees, and not because they persuade clients to litigate by exploiting their informational advantage) is rather unlikely, at least for those European countries imposing minimum fees for lawyers.²

Supplier induced demand is not unique to the lawyer profession. For example in the health care sector, patients do not have the required information and expertise to assess the optimal number of medical services. Following the supplier induced demand hypothesis, physicians take into account private monetary incentives and hence induce medical services.³ Studies have found evidence of a supplier induced demand, especially when competition (e.g. amongst doctors or dentists) is tough, for services such as surgeries (Fuchs 1978), dentist services (Birch 1988), laboratory tests (Grytten, Carlsen and Sørensen 1995) and drug prescriptions (Hellerstein 1998). Other applications of the supplier induced demand can be found, for example, in the auto repair market (Peters et al. 2012) and in supplier-buyer contracting (Romano 1994).

Endogeneity concerns complicate the estimation of equation (1). The first source of endogeneity is country-level unobserved heterogeneity: unobservable and omitted country-specific characteristics might have a direct effect on *Lawyers* and *Controls*, causing the right-hand side variables to be correlated with the error term. Legal costs, availability of alternative dispute mechanisms, procedural rules and use of technologies (e.g. video conferencing) are likely to affect litigation rates, but cannot be accounted for because of a lack of (consistent) data. The second source of endogeneity is the reverse causality between *Lawyers* and *Litigation*. Lawyers are likely to induce the demand for court services but, at the same time, additional

² For instance, Germany, Slovenia and Italy list minimum fees (European Commission 2004).

³ Note, however, that the information asymmetry in the healthcare sector is somewhat different compared to the lawyer profession. That is, a patient has little information on the quality of medical care (even after treatment), whereas a litigant at least observes whether he loses the case. Therefore, we expect that if we find evidence of supplier induced demand, the magnitude of the effect will be smaller compared to the effect in the health care sector

litigation will attract more lawyers. Consequently, causation not only runs from the number of lawyers to litigation, but also in the opposite direction and hence *Lawyers* will be endogenous to *Litigation* (Carmignani and Giacomelli 2010, Buonanno and Galizzi 2014, Mora-Sanguinetti and Garoupa 2015). Analogously, scholars have had endogeneity issues when analysing the supplier-inducement in the health care sector because health care providers might locate in areas of high demand for their services (Birch 1988).

To avoid erroneous inferences and inaccurate policy recommendations, we address both endogeneity concerns in Section 4. We first proceed with a discussion of our data and variables in Section 3.

3. Data and variables

Data

This paper uses homogeneous data on the daily functioning of the judicial systems of the member states of the European Council. The data was gathered by the Commission for the Efficiency of Justice (hereafter CEPEJ) of the Council of Europe (CEPEJ 2008, 2010, 2012, 2014, 2016). The fact that the data are based on Member States' own responses to an evaluation scheme elaborated by the CEPEJ minimizes difficulties of interpretation of the questions, allowing uniformity despite differences between the organization of judiciaries (CEPEJ 2014). Furthermore, the CEPEJ datasets have the advantage of being collected biennially, allowing international comparisons as well as comparisons over time (Roussey and Deffains 2012).

For 30 countries⁴ in the period 2006-2014, we observe biennial data on number of new civil (and commercial) cases per 10,000 inhabitants, number of lawyers per 10,000 inhabitants, number of inhabitants, number of inhabitants per square kilometer, number of judges per 10,000 inhabitants, legal aid budget per 10,000 inhabitants, GDP per capita, change in GDP per capita, number of courts per million inhabitants, number of civil (and commercial) pending cases per judge, use of contingency fees and availability of legal insurance. Table 1 provides full description of all variables included in our dataset. Table 2 presents descriptive statistics. Not surprisingly, Table 2 reveals that for each variable (with the exception of *Growth*), there is more variation across countries than over time.

[Insert Table 1 here]

[Insert Table 2 here]

Dependent variable: measuring litigation

Following existing literature (see, e.g. D'Agostino, Sironi and Sobbrío 2012, Clemenz and Gugler 2000, Yates, Davis and Glick 2001), we use the litigation rate to measure the demand for court services. More specifically, we use the number of new civil⁵ and commercial legal disputes filed per 10,000 inhabitants in first instance courts in a particular year.

Figure 1 shows the litigation rate for each country in 2014, demonstrating the discrepancies in the propensity to litigate among the countries in our sample (see also the between-variation in Table 2). Scandinavian countries, and more generally

⁴ Albania, Armenia, Austria, Azerbaijan, Bosnia and Herzegovina, Croatia, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Italy, Latvia, Lithuania, Macedonia, Moldova, Montenegro, Norway, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden, Switzerland and Turkey.

⁵ In the CEPEJ questionnaire, "civil law cases" refer to other than criminal law cases and include for example family law cases, commercial law cases, employment dismissal cases and administrative law cases.

countries in Northern Europe, have relatively low litigation rates. Surprisingly, several post-transition countries (such as Croatia and Romania) are among the most litigious countries. Generally, a high propensity to use the court system to resolve civil disputes seems to exist especially in Southern and Eastern Europe countries.

[Insert Figure 1 here]

The litigation rate varies also considerably from one period to another. Over the past decade, scholars have increasingly reported a rise in civil litigation rates in countries around the world, leading to increasing concerns on the excessive use of courts to resolve disputes (Buscaglia and Dakolias 1999, Clemenz and Gugler 2000, Yates, Davis and Glick 2001, D'Agostino, Sironi and Sobbrío 2012). In our European dataset, litigation rates increased in most countries between 2006 and 2014. The time evolution of litigation rates between 2006 and 2014 is presented in Figure 2.

[Insert Figure 2 here]

Key explanatory variable: measuring the number of lawyers

Our key variable of interest is the number of lawyers in a country. In the CEPEJ reports, a lawyer is defined as “... *a person qualified and authorized according to the national law to plead and act on behalf of his or her clients, to engage in the practice of law, to appear before the courts or advise and represent his or her clients in legal matters*” (CEPEJ 2014). We use the number of lawyers per 10,000 inhabitants.

Figure 3 shows the number of lawyers per 10,000 inhabitants in 2014 in each of the countries included in our sample. Again, significant discrepancies exist between the different countries. Remarkably, the number of lawyers increases for each country between 2006 and 2014 (see Figure 4). Even in countries where the lawyer

density was already considerable, the increase is significant. For example, Italy had 29 lawyers per 10,000 inhabitants in 2006 and 37 in 2014 (i.e. an increase of 27%). The finding that many European countries have a high number of lawyers and that this number is increasing in almost each of them, raises the question whether this evolution is affecting litigation rates. We empirically investigate this hypothesis in Section 4.

[Insert Figure 3 here]

[Insert Figure 4 here]

Control variables

Our model includes the (natural logarithms of) following controls: population, population density, number of judges, legal aid, GDP per capita, change in GDP per capita, number of courts, congestion, use of contingency fees and availability of legal insurance.⁶

Following Buonanno and Galizzi (2014) and D'Agostino, Sironi, and Sobbrino (2012), we include population density as a control to proxy the average concentration of population. It is hypothesized that in more densely populated countries the number of social interactions, and hence, the potential for conflicts is higher.

The number of judges per 10,000 inhabitants is included as a proxy for productivity of the courts (Buonanno and Galizzi 2014, Ginsburg and Hoetker 2006), and therefore we expect that more judges increase litigation rates.

Legal aid lowers the costs of filing a suit and presumably induces litigation. In the CEPEJ reports, legal aid is defined as “*aid given by the State to persons who*

⁶ Because they are time-invariant, use of contingency fees and availability of legal insurance are only used in models without country fixed effects.

do not have sufficient financial means to defend themselves before a court (or to initiate a court proceeding). [...] However, legal aid consists also in legal advice” (CEPEJ 2008). Thus, legal aid is defined in a broad sense and not only includes financial help to initiate court proceedings and hire legal representation, but also includes financial aid for legal advice. The latter comprises information policies and mechanisms to stimulate parties in preventing trials. The two components of legal aid (i.e. lowering litigation costs and stimulating alternative dispute resolution) make it difficult to predict its effect on the demand for civil trials. On the one hand, legal aid reduces the financial burdens of filing a suit and thus induces litigation. On the other hand, legal advice enhances parties’ realistic assessment of winning probabilities, hence encouraging alternative dispute resolution. Consequently, the net effect of legal aid on litigation rates cannot be predicted unambiguously ex ante.

The impact of GDP per capita on litigation has been examined in previous studies (Clemenzen and Gugler 2000, Mora-Sanguinetti and Garoupa 2015, Buonanno and Galizzi 2014), but the direction of the effect remains ambiguous. On the one hand, in wealthier countries, parties find it easier to fulfil obligations, fewer contracts are broken and creditors are presumably more patient. On the other hand, a higher GDP per capita not only implies a higher number of transactions but also more complex transactions, which in turn augments the potential for conflicts (Hanssen 1999, Posner 1997, Clemenzen and Gugler 2000, Ginsburg and Hoetker 2006). Moreover, parties living in more prosperous regions can be presumed to have a higher ability to pay potential litigation costs (Sobbrio, D’Agostino and Sironi 2010). We also include the growth rate of GDP per capita to take into account economic cycles. We expect that negative growth rates are associated with higher litigation rates since contracts are more likely to be broken (Carmignani and Giacomelli 2010, Ginsburg and Hoetker 2006, Clemenzen and Gugler 2000).

Since the availability of courts is very likely to affect litigation, we also include the number of courts per million inhabitants. Because the proximity of a court reduces the costs of going to trial, relatively more courts will presumably increase litigation. This implies a positive relation between courts and litigation. However, relatively less courts could be an indication of larger courts where specialization and economies of scale increase efficiency. Then, the more efficient courts presumably attract more cases (since cases are likely to be resolved more expeditiously) and hence a negative relation exists between the number of courts and the demand for justice.

When courts are backlogged, alternative dispute resolution (such as settlement) becomes a more appealing option than filing suit (Murrell 2001). Therefore, we expect a negative relationship between court congestion and litigation rates. To measure backlogs, we use the number of pending cases per judge.

We also include a dummy that equals one if contingency fees are allowed in the observed country. Many authors have argued that contingency fee contracts enable plaintiffs with limited resources to secure otherwise unaffordable costs, and therefore the availability of contingency fees increases access to courts (Rubinfeld and Scotchmer 1993, Brickman 2003, Zamir and Ritov 2010). Furthermore, scholars have argued that lawyers' exploitation of their informational advantage is more common when lawyers are paid per legal act or on an hourly basis (Emons 2000, Polinsky and Rubinfeld 2003). Following the existing literature, we hypothesize that the litigation rates are higher, *ceteris paribus*, in countries where lawyers are allowed to use contingency fees.

Finally, we control for the availability of legal insurance, since the latter increases access to justice. That is, we expect (all else equal) that countries in which legal insurance is available, litigation rates will be on average higher.

Since the availability of legal insurance and contingency fees are time-invariant over our timespan, we can only include them in our models that do not control for country fixed effects.

4. Empirical strategy and results

We use different empirical strategies to estimate variations of equation (1). In all models, both our dependent and independent variables are logged⁷ to facilitate the interpretation of coefficients as elasticities, to achieve more normal distributions on the left- and right-hand side of the equation and to mitigate the effect of outliers. Standard errors are always heteroscedasticity-robust (White 1980) and clustered at the country level to correct for a possible correlation of error terms over time for all countries (Newey and West 1987).

Benchmark: Pooled OLS

First, we estimate the following models using pooled OLS:

$$\text{Log}(\text{Litigation})_{it} = \alpha + \beta \log(\text{Lawyers})_{it} + \sum_{c=1}^{10} \log(\text{Controls})_{it}^c + \mu_i t + \sum_{t=2}^5 \gamma_t D_t + \varepsilon_{it} \quad (2)$$

Where α is the intercept and ε_{it} the error term for country i in year t . The term $\mu_i t$ is a country-specific linear time trend and controls for unobserved country-specific trends in litigation rates. D_t represent year dummies that control for year fixed effect and capture factors that vary over time but affect all countries (e.g. financial crisis effects).

We first estimate equation (2) without the year dummies, and subsequently without them. The results of the pooled OLS models are shown in columns (1) and

⁷ With exception of *Growth*, which has negative values, and *Congestion*, which is measured as cases per judge.

(2) of Table 3. Both estimations result in a statistically insignificant impact of *Lawyers* on *Litigation*. As discussed in section 2, however, unobserved heterogeneity is plausible since countries are assumed to differ in terms of (legal) culture, legal origin, availability of alternative dispute resolution, etc. Some of these differences remain unobserved, but are correlated with the observed independent variables, and consequently the pooled OLS-regressions yield biased estimations of β , the effect of *Lawyers* on *Litigation*. Therefore, we now address this omitted variable bias concern by applying a panel fixed effects approach.

Fixed effects estimation

To tackle the possible endogeneity problem arising from omitted variables in our panel dataset, we estimate a two-way fixed effects model in the following form:

$$\text{Log}(\textit{Litigation})_{it} = \alpha_i + \gamma_t + \beta \log(\textit{Lawyers})_{it} + \sum_{c=1}^{10} \log(\textit{Controls})_{it}^c + \alpha_i t + u_{it} \quad (3)$$

In equation (3) α_i is the country fixed effect, which measures the time-invariant unobserved country-specific heterogeneity such as legal culture or legal origin. γ_t represents the year fixed effect (the equivalent of the set of time dummies in equation (2)), i.e. the time-varying effects that are uniform across countries. $\alpha_i t$ is a linear country-specific time trend and u_{it} is the error term.

One important note is that in our dataset, the number of lawyers changes significantly between 2006 and 2014 (see Table 2 and Figure 4) and is therefore not subsumed in the fixed effects.

The estimation results are presented in column (3) of Table 3. With the inclusion of the country fixed effects, the effect of *Lawyers* on *Litigation* remains statistically insignificantly different from zero.

Although our two-way fixed effects model takes into account the unobserved and time-invariant heterogeneity, it does not tackle the plausible reverse causality

between *Lawyers* and *Litigation* and possible time-varying country-heterogeneity. Therefore, we apply several instrumental variable approaches in the next Section.

Addressing endogeneity: 2SLS instrumental variable estimation

As discussed, the relationship between *Lawyers* and *Litigation* may be affected by reverse causality because lawyers not only induce litigation, but additional litigation will also attract more lawyers. To address this simultaneity bias, we first apply a 2SLS approach.

In this paper, we separately use two instruments for the endogenous covariate *Lawyers*. First, we follow Mora-Sanguinetti and Garoupa (2015) and Buonanno and Galizzi (2014) and use the number of law schools. More precisely, we use the number of law schools that exists in each country, and relate this number to the country's surface.⁸ This instrument is expected to be correlated with the number of lawyers in a country, since a higher law school density captures the presence of a tradition for legal studies (which promotes the development of the lawyer profession) (Carmignani and Giacomelli 2010). Furthermore, educational costs increase when the distance to a law school is higher. Therefore, the number of law schools per square kilometer presumably (positively) affects the number of lawyers in a country. Yet, the requirement that our instrument must be exogenous to changes in litigation rates may not be satisfied since the number of law schools in a country is likely to be affected by the level of litigation. We address this issue by constructing the instrument with reference to a period that precedes the timespan of our dataset. Therefore, to ensure exogeneity of our instrument, we use the number of law schools in 1970 (i.e. 36 year prior to the beginning of our dataset).

⁸ A list of law schools in Europe was gathered from HG.org, an online law and government information site, founded by Lex Mundi. The year of foundation of the law faculty was gathered from the official website of each university.

The use of a time-invariant instrument (the number of law schools in 1970) prevents us from using country fixed effects. Although dropping the fixed effects would allow us to use the instrument to address the reverse causality issues, it would re-introduce endogeneity in the form of unobserved and time-invariant country-heterogeneity. Therefore, we follow Mora-Sanguinetti and Garoupa (2015) and include regional fixed effects⁹ (which allows us to use the law school instrument) and include as controls the average of each control to account for country-specific unobserved heterogeneity:

$$\text{Log}(\text{Litigation})_{it} = \beta \log(\text{Lawyers})_{it} + \sum_{c=1}^{10} \log(\text{Controls})_{it}^c + \sum_{c=1}^{10} \overline{\log(\text{Controls})_c} + \alpha_r + \delta_t + \alpha_{rt} + u_{it} \quad (4)$$

Where α_r are region fixed effects and α_{rt} region-specific time trends.

The 2SLS results where *Lawyers* is instrumented by the number of law schools per square kilometer in 1970 are presented in columns (1) and (2) of Table 4. Both with and without the inclusion of the time trends, we find a positive and statistically significant effect of *Lawyers* on *Litigation*. All else equal, a 1 percent increase in *Lawyers* increases *Litigation*, on average, with 1.46 to 1.47 percent.

As discussed extensively in literature (see, e.g. Stock and Yogo 2005, Staiger and Stock 1994), instruments can be weak in the sense that they are satisfactorily exogenous, but only weakly correlated with the endogenous covariate. In that case, “the cure can be worse than the disease” (Bound, Jaeger and Baker 1993). Therefore, we first evaluate the goodness-of-fit of the first stage results as a way to examine whether the excluded instrument is sufficiently correlated with the included endogenous regressor.¹⁰ According to Staiger and Stock (1994), an F statistic of less than 10 in the first stage of the two-stage procedure suggests a weak instrument. Nevertheless, smaller F values are acceptable as well in our case since we have a just-identified model with one endogenous variable and one instrument.

⁹ We grouped countries into 4 categories: North, East, South and West-Europe.

¹⁰ The first stage results are omitted because of space considerations. The results are available upon request.

This is true because in case of just-identified models, 2SLS is median-unbiased (Angrist and Pischke 2009, Mocan and Cannonier 2016). However, with F-values of 13.19 and 11.99 respectively, we can convincingly reject the hypothesis that our instrument is weak.

To test the robustness of our results and to overcome the suboptimal strategy of including regional fixed effects instead of country fixed effects in the presence of a time-invariant instrument, we propose a second instrument. More specifically, we construct an instrument that measures the number of law graduates (related to population).¹¹ We expect that an increase in the number of law graduates induces the number of lawyers in a country. Although not every law graduate will enter the lawyer profession, at least a significant fraction will. Nonetheless, as with our law school instrument, the requirement that our instrument must be exogenous to changes in litigation rates may not be satisfied. Higher litigation rates could motivate individuals to pursue an education in law and hence a higher number of law graduates can be expected. Therefore, we lag the number of graduates with two years.¹² However, the instrument turns out to be very weak: the F-statistic of the excluded instrument is only 0.37. The results are presented in column (3) of Table 4.

Addressing endogeneity: GMM instrumental variable estimation

As a second strategy, we use the GMM difference estimator to tackle reverse causality between *Lawyers* and *Litigation*. Checking the robustness of our results with GMM is particularly appealing because in our first 2SLS estimation (results

¹¹ The data were gathered from the OECD (statistics on Education and Training – Graduates by field of education) and from National Statistical Offices for the non-OECD countries.

¹² A lag of two years may not seem a considerable amount of time, but note that we are looking at law *graduates*, and not *newly registered* law students. Hence, the decision to start a law school program from graduates in year t was made in year $t-x$ (x being the number of years of law education). Furthermore, we re-ran our regressions and lagged *graduates* with 4 years. Results were qualitatively the same but the sample reduced in size due to data limitations.

in column (1) and (2) of Table 4) we had to rely on region FE rather than country FE and because of the weakness of the instrument used in the second 2SLS estimation (results in column (3) of Table 4).

The GMM difference approach, developed by Arellano and Bond (1991), starts by first-differencing the equation, thus removing the unobserved individual effect α_i (Baum 2006). Subsequently, the equation is estimated using the standard GMM procedure (hence the name “difference GMM”) (Hansen and Singleton 1982). The rationale of Arellano-Bond estimation is drawn on instruments from within the dataset, rather than using external instruments that might be weak (such as our law graduates instrument). Although it is possible to use lagged regressors as instruments with standard 2SLS, the use of deeper lags reduces sample size since observations for which lagged observations are unavailable are dropped. Yet GMM eliminates the trade-off between lag depth and sample size, rendering it possible to include all valid lags of the untransformed variables as instruments (Roodman 2009).

One pitfall of the Arellano-Bond approach is the overfitting of endogenous variables when using a large instrument set. Although there is little guidance from literature on the maximum number of instruments, Roodman (2009) suggest to collapse the used instrument set.

The GMM results are presented in column (4) of Table 4. The results again suggest a positive and statistically significant impact of *Lawyers* on *Litigation*, confirming the lawyer-induced litigation hypothesis. A one percent increase in *Lawyers*, increases *Litigation* with on average 1.16 percent, all else equal.

Finally, we evaluate the consistency of the difference GMM estimator. To test the validity of our internal instruments, we use both the Hansen test of over-identifying restrictions and the Arellano–Bond test for serial correlation of the error term. As reported in Table 4, these tests confirm the validity of our instrument set.

5. Robustness check

To further test the validity of our results, we perform an additional analysis. We hypothesize that a positive relationship exists between litigation rates and lawyer density because lawyers deliberately induce litigation when competition is tough. If this is true, then lawyer density should not impact criminal litigation, since lawyers have no say in the decision to file a criminal suit (this is the competence of the public prosecutor). In other words, if we re-estimate our models and replace the dependent variable (number of new civil cases per 10,000 inhabitants) with the number of criminal cases per 10,000 inhabitants,¹³ we should not find a positive and statistically significant impact of *Lawyers*. Otherwise, something else than the supplier induced demand phenomenon might be causing a positive relationship between *Lawyers* and *Litigation*. Table 5 shows the results. In none of the models *Lawyers* is statistically significantly different from zero. The results of this placebo test add to the credibility of our empirical results and the conclusions we have drawn, namely that an increase in the number of lawyers increases civil litigation because lawyers induce litigation. Given that *Lawyers* is not statistically significant in the crime models, we believe that this result is not a consequence of coincidence.

6. Conclusion and discussion

Economic theory predicts that an overabundance of lawyers induces litigation. Hence, both successful regulation of the lawyer profession and effective policy measures that discourage frivolous lawsuits require a thorough understanding of the relationship between the number of lawyers and the litigation rate in a country. However, empirically examining the relationship between lawyers and litigation is

¹³ Of course, we also replace the number of pending civil cases by the number of pending criminal cases.

challenging because of endogeneity problems. Since it is difficult to find appropriate instruments, empirical evidence remains scarce.

To analyze the effect of the number of lawyers on litigation rates, this paper uses biennial homogeneous data on the functioning of the judicial systems of 30 member states of the European Council in the period 2006-2014. We explicitly tackle the problem of reverse causality between lawyers and litigation using several estimation strategies.

First, we use a 2SLS approach. From a theoretical point of view, both the number of law schools per square kilometer and the number of law graduates per 10,000 inhabitants in a country are expected to be valid instruments. Both should be highly correlated with the number of lawyers and uncorrelated with unobserved factors affecting litigation rates. Since we require an exogenous source of variation for the number of lawyers, we lag the instruments. The 2SLS estimations with law schools as an instrument yield results that are in line with the supplier induced demand hypothesis: the number of lawyers has a positive and statistically significant impact on litigation rate. The number of law graduates appears to be a weak instrument and therefore we cannot rely on the estimation results.

As an alternative approach, we tackle the anticipated reverse causality between lawyers and litigation using the linear GMM (General Method of Moments) estimator of Arellano and Bond (1991). Again, we find a positive and significant effect of lawyer density on litigation rate.

A major policy implication of our empirical results is that a desired moderation of the growing litigiousness in Europe should be associated with incentives that restrain lawyers from exploiting their informational advantage. For example, Emons (2000) and Polinsky and Rubinfeld (2003) show that lawyers' exploitation of their informational advantage is more common when lawyers are paid per legal act or on an hourly basis (as is most common in continental Europe). Contingency fees, on the contrary, are dependent upon the outcome of the trial and provide an

incentive for lawyers to take on cases with a realistic chance of winning. In that regard, policymakers might want to reconsider strongly regulated lawyers' remunerations and the prohibition of the use of contingency fees that are in effect in most European countries (European Commission 2004).¹⁴ Furthermore, the evidence of the lawyer-induced litigation hypothesis might even call for a *numerus clausus* to limit the number of law students. In some health care systems, for example, a *numerus clausus* limits the number of physicians that may practice under the national health insurance system. The decision to introduce such a restriction is motivated by concerns about health care expenditures (driven by supplier induced demand) and quality of care (see, e.g. Belgian Health Care Knowledge Centre 2008). Similarly, a restriction on the number of lawyers should prevent lawyer-induced litigation, with less lawsuits as a result. Consequently, we expect a decline of expenditures related to both legal aid and court congestion. On the other hand, too few lawyers will lead to increased lawyer fees, which will hamper access to the courts. Therefore, it is crucial to find the optimal number of lawyers, which can differ from one judiciary to the other.

¹⁴ For example, contingency fees are allowed by law in only 37 percent of the OECD countries (Palumbo et al. 2013).

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TABLE 1: VARIABLE DESCRIPTION

Variable	Description
Litigation	Number of incoming civil (and commercial) litigious cases at first instance courts.
Lawyers	Number of lawyers, per 10,000 inhabitants.
Population	Number of inhabitants.
Density	Number of inhabitants, per square kilometer.
Judges	Number of judges, per 10,000 inhabitants
Legal aid	Annual public budget allocated to legal aid (€), per 10,000 inhabitants.
GDP	GDP per capita.
Growth	Change in GDP per capita.
Courts	Number of courts, per million inhabitants.
Congestion	Number of pending civil (and commercial) litigious cases at first instance courts, per judge.
Contingency	Dummy equal to one if contingency fees are allowed.
Insurance	Dummy equal to one if private system of legal expense insurance exists.

TABLE 2: SUMMARY STATISTICS

Variable	No. Obs.	Mean	Std. Dev.	Min.	Max.
Litigation					
Overall	N = 130	227.57	148.53	15.03	685.16
Between	n = 30		137.78	18.90	493.20
Within	T = 3.71		66.33	6.94	455.43
Lawyers					
Overall	N = 130	11.27	8.47	0.64	38.07
Between	n = 30		8.85	0.82	37.51
Within	T = 3.71		1.63	5.28	18.19
Population (in millions)					
Overall	N = 130	18.40	2.70	0.08	143.00
Between	n = 30		3.10	0.08	143.00
Within	T = 3.71		0.40	1.67	19.90
Density					
Overall	N = 130		160.38	8.30	1,316.76
Between	n = 30	115.21	215.97	8.34	1,304.65
Within	T = 3.71		2.59	103.10	128.333
Judges					
Overall	N = 130	1.97	1.03	0.57	5.35
Between	n = 3		1.07	0.62	4.91
Within	T = 3.71		0.10	1.54	2.41
Legal aid					
Overall	N = 130	49,768.87	89,440.48	67.07	535,540.1
Between	n = 30		84,311.90	212.23	392,128.8
Within	T = 3.71		22,953.00	-58,001.61	193,180.10
GDP					
Overall	N = 130	19,821.40	16,842.2	745	79,235.00
Between	n = 30		16,234.34	1,279.80	66,190.80
Within	T = 3.71		2,478.26	9,630.60	32,865.60
Growth					
Overall	N = 130	0.14	0.025	-0.30	1.53
Between	n = 30		0.17	-0.10	0.67
Within	T = 3.71		0.20	-0.33	1.34
Courts					
Overall	N = 130	18.99	12.56	0.71	80.76
Between	n = 30		11.25	0.71	47.41
Within	T = 3.71		7.60	-17.08	69.09
Congestion					
Overall	N = 130	92.99	116.73	2.49	643.68
Between	n = 30		112.56	7.69	578.04
Within	T = 3.71		21.04	20.66	177.45
Contingency					
Overall	N = 130	0.65	0.48	0	1
Between	n = 30		0.49	0	1
Within	T = 3.71		0	0.65	0.65
Insurance					
Overall	N = 130	0.72	0.45	0	1
Between	n = 30		0.40	0	1
Within	T = 3.71		0.22	-0.03	1.47

TABLE 3: BASELINE REGRESSION RESULTS: POOLED OLS AND FIXED EFFECTS

VARIABLES	(1) Pooled OLS	(2) Pooled OLS	(3) Fixed effects
Lawyers	0.1674 (0.1431)	0.1542 (0.1368)	0.5672 (0.3833)
Population	0.1398* (0.0698)	0.1379* (0.0688)	11.3263 (25.5279)
Density	0.2753** (0.1065)	0.2721** (0.1078)	-10.5970 (25.7107)
Judges	0.4612*** (0.1262)	0.4525*** (0.1298)	0.7104 (0.4935)
Legal aid	0.0866 (0.0647)	0.0804 (0.0642)	0.1197 (0.0841)
GDP	-0.0000** (0.0000)	-0.0000** (0.0000)	0.0000 (0.0000)
Growth	0.5001 (0.4326)	0.3719 (0.3599)	-0.0227 (0.1891)
Courts	0.2047** (0.0896)	0.2186** (0.0979)	-0.0797 (0.0561)
Congestion	0.0016** (0.0006)	0.0016** (0.0007)	0.0069*** (0.0015)
Contingency	0.1156 (0.2448)	0.1196 (0.2416)	
Insurance	-0.2511 (0.1792)	-0.2539 (0.1795)	
Observations	118	118	130
Country FE	No	No	Yes
Year FE	Yes	No	Yes
Time trend	Yes	Yes	Yes

Dependent variable: Number of incoming civil (commercial) litigious cases at first instance courts. Heteroskedasticity-robust standard errors clustered at country level in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level respectively.

TABLE 4: IV-2SLS AND IV-GMM REGRESSION RESULTS

VARIABLES	(1) IV-2SLS	(2) IV-2SLS	(3) IV-2SLS	(4) IV-GMM
Lawyers	1.4679*** (0.4617)	1.4605*** (0.4683)	-5.5932 (40.3351)	1.1582* (0.5970)
Population	0.0698 (0.0474)	0.0648 (0.0479)	23.2423 (107.2015)	73.1735 (62.2124)
Density	0.1364 (0.1308)	0.1347 (0.1322)	-28.4442 (144.6248)	-73.0213 (61.6719)
Judges	-0.7899* (0.4501)	-0.7918* (0.4611)	0.3112 (1.1923)	1.3659 (1.0220)
Legal aid	0.2800*** (0.0617)	0.2775*** (0.0620)	0.0281 (0.1743)	0.0136 (0.1112)
GDP	-0.0000*** (0.0000)	-0.0000*** (0.0000)	0.0000 (0.0000)	-0.0000 (0.0000)
Growth	0.8122 (0.5342)	0.7330 (0.5318)	-0.7368 (2.3759)	-0.1434 (0.1289)
Courts	0.5457*** (0.2003)	0.5479*** (0.2062)	-0.0557 (0.1723)	-0.2748 (0.2781)
Congestion	0.0010 (0.0008)	0.0011 (0.0008)	0.0060 (0.0068)	0.0045* (0.0022)
Contingency	0.2284 (0.1646)	0.2139 (0.1624)		
Insurance	-0.1293 (0.1673)	-0.0919 (0.1743)		
Observations	115	115	95	90
Country FE	No	No	Yes	Yes
Region FE	Yes	Yes	No	No
Year FE	Yes	Yes	Yes	Yes
Time trend	No	Yes	No ¹⁵	No ¹⁶
Instrument	Law schools [1970]	Law schools [1970]	Law graduates [lagged]	Internal instruments
F statistic of excluded instruments	13.19	11.99	0.37	/
No. Instruments	1	1	1	31
Hansen over-id. test (p-value)	/	/	/	0.575
AR(2) Arellano–Bond test (p-value)	/	/	/	0.718

Dependent variable: Number of incoming civil (commercial) litigious cases at first instance courts. The results in column (4) are obtained using Arellano and Bond's (1991) 2-step robust GMM estimator in which level equation is excluded from the estimation, yielding difference rather than system GMM. Following Roodman (2009a), we use a collapsed instruments set based on the first, second and third lag of Lawyers, Population, Density, Judges, Legal aid, GDP, Growth, courts, congestion and differenced year dummies. Heteroscedasticity-robust standard errors clustered at country level are in parentheses. In the GMM model, the standard errors are calculated using small-sample corrections. *, **, and *** indicate significance at the 10%, 5%, and 1% level respectively. The null hypothesis for the Arellano–Bond serial correlation test is that the errors in the first-difference regression exhibit no second-order autocorrelation. The null hypothesis for the Hansen over-identification test is that the instruments and the residuals are uncorrelated.

¹⁵ Time trends are dropped because of collinearity.

¹⁶ Time trends are dropped because of collinearity.

TABLE 5: IV-2SLS AND IV-GMM REGRESSION RESULTS FOR CRIMINAL CASES

VARIABLES	(1) IV-2SLS	(2) IV-2SLS	(3) IV-2SLS	(4) IV-GMM
Lawyers	-1.8452 (1.5107)	-1.8260 (1.4946)	0.4322 (1.9489)	2.4140 (2.6575)
Population	0.1950** (0.0839)	0.1923** (0.0831)	-113.9071* (63.1876)	-194.1299 (681.2119)
Density	-0.0823 (0.3086)	-0.0858 (0.3031)	115.1595* (60.3121)	201.3834 (681.6531)
Judges	2.7576*** (0.8803)	2.7538*** (0.8711)	0.7969** (0.3711)	1.0351 (4.3831)
Legal aid	-0.1386 (0.1497)	-0.1401 (0.1473)	-0.0103 (0.0625)	0.0725 (0.3232)
GDP	0.0001* (0.0000)	0.0001* (0.0000)	-0.0001*** (0.0000)	-0.0001 (0.0001)
Growth	-1.2566 (1.3359)	-1.2734 (1.3222)	0.5503 (0.3854)	0.9163 (1.7479)
Courts	-0.4957 (0.3474)	-0.5004 (0.3438)	-0.0645 (0.0423)	-0.1976 (0.2182)
Congestion	0.0042** (0.0021)	0.0041** (0.0021)	0.0097*** (0.0013)	0.0044 (0.0097)
Contingency	-0.1016 (0.3368)	-0.1048 (0.3320)		
Insurance	0.2180 (0.3958)	0.2149 (0.3946)		
Observations	76	76	64	55
Country FE	No	No	Yes	Yes
Region FE	Yes	Yes	No	No
Year FE	Yes	Yes	Yes	Yes
Time trend	No	Yes	No	Yes
Instrument	Law schools [1970]	Law schools [1970]	Law graduates [lagged]	Set of internal instruments

Dependent variable: Number of incoming criminal cases at first instance courts. Heteroskedasticity-robust standard errors clustered at country level in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level respectively.

FIGURE 1: LITIGATION RATES IN 2014

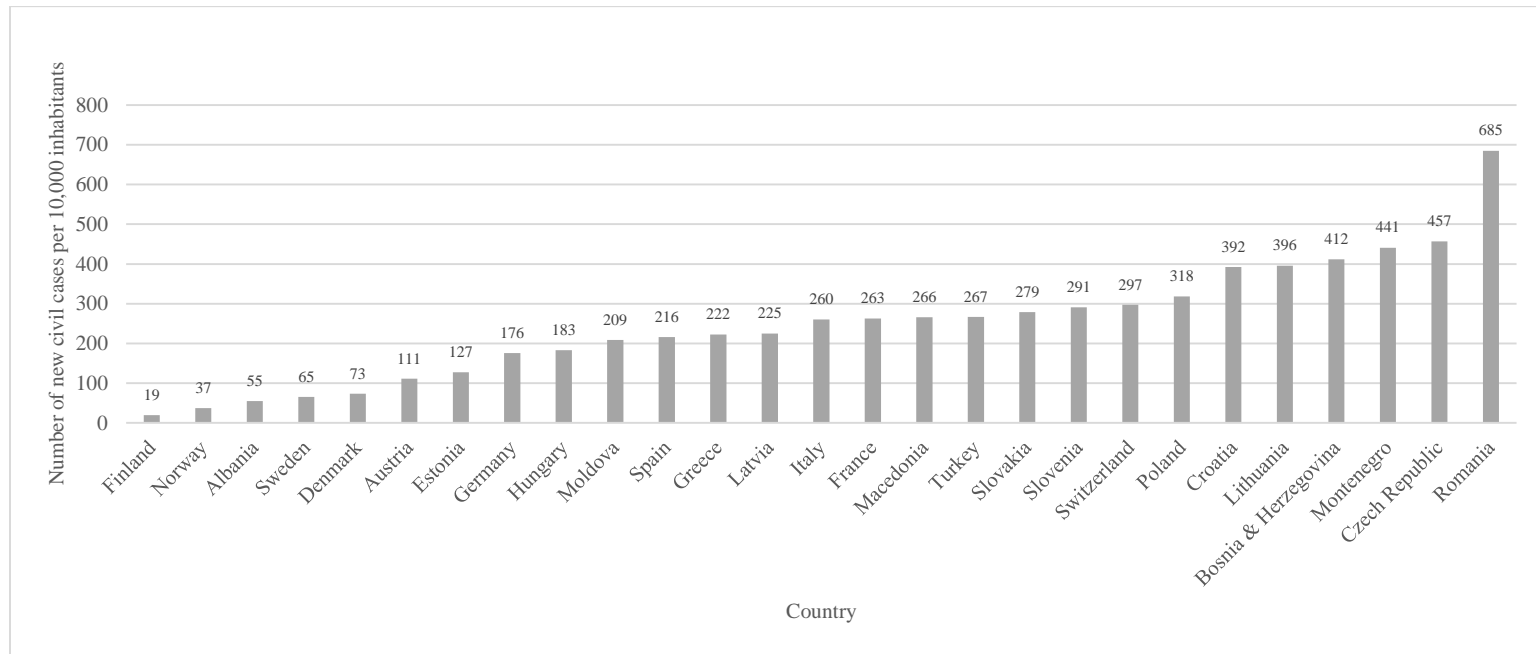


FIGURE 2: TIME EVOLUTION OF LITIGATION RATES BETWEEN 2006 AND 2014

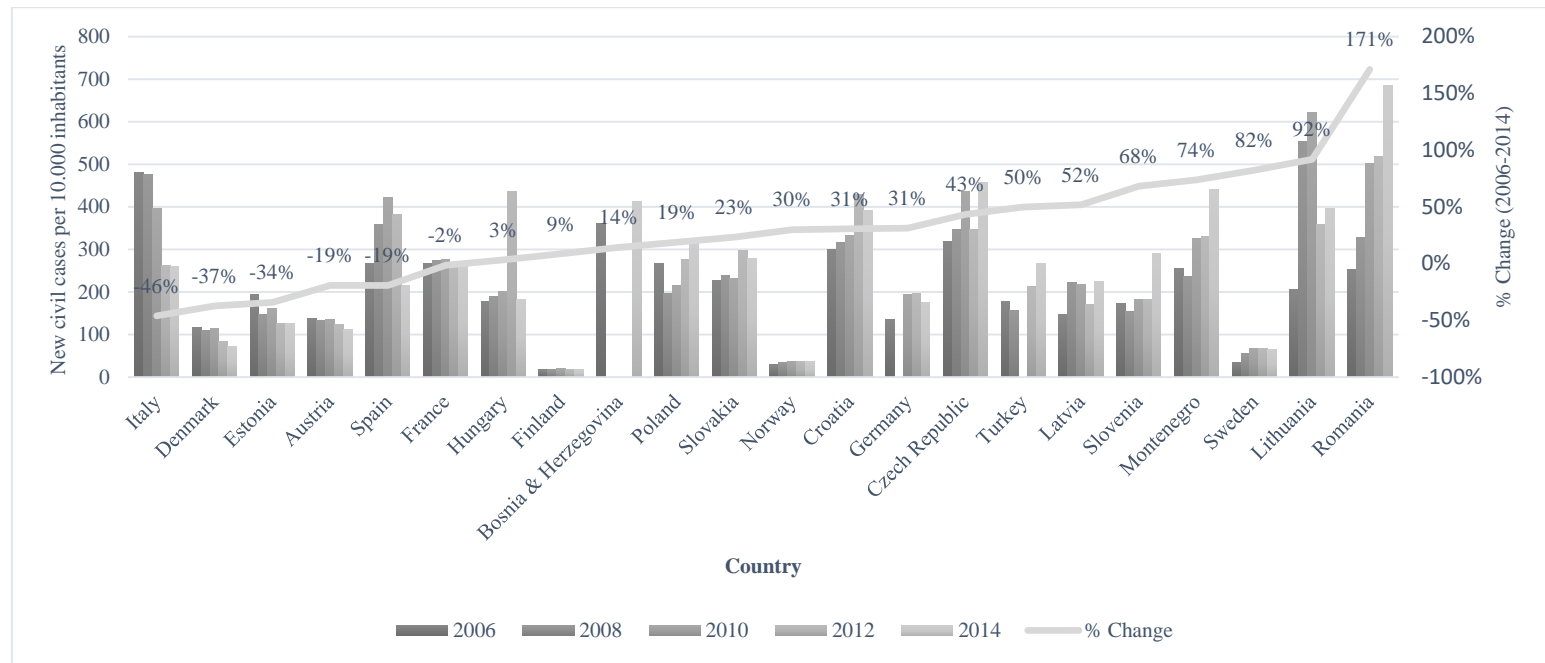


FIGURE 3: NUMBER OF LAWYERS PER 10,000 INHABITANTS IN 2014

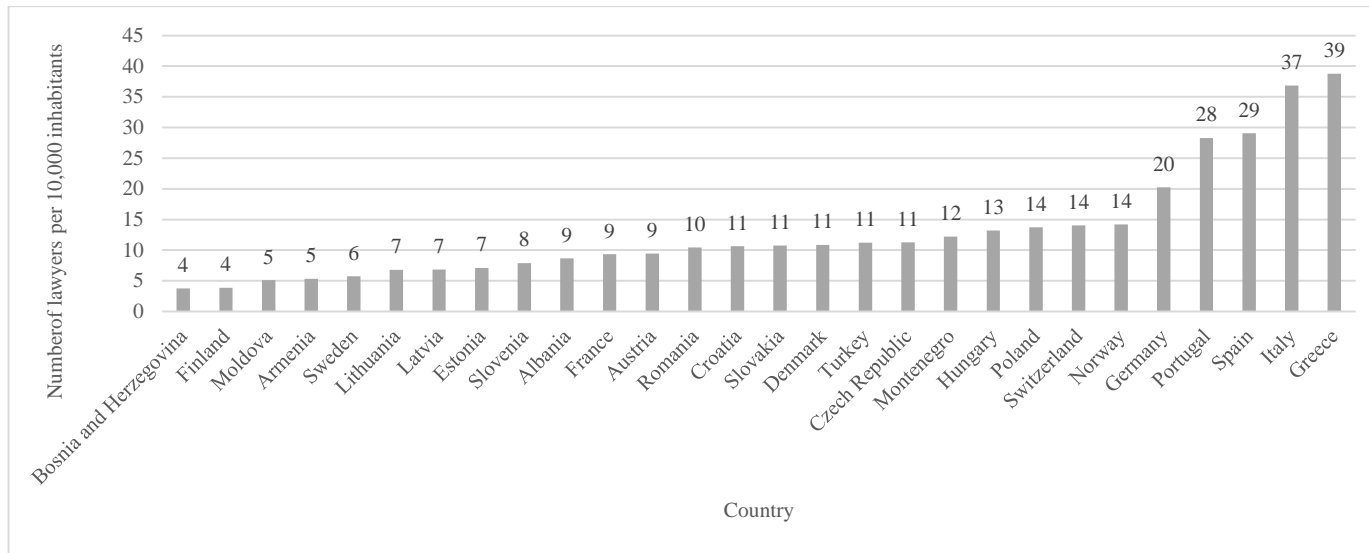


FIGURE 4: TIME EVOLUTION OF THE NUMBER OF LAWYERS PER 10,000 INHABITANTS BETWEEN 2006 AND 2014

