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Legal origin and financial development:

A propensity score matching analysis

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Abstract - We revisit the impact of legal origin on financial development using propensity score matching and a new financial development index on a sample of 178 countries over 1980-2016. German civil law countries are found to have the strongest positive impact on financial development. English common law countries follow. French and Scandinavian civil law countries have a negative impact on financial development while Socialist legal origin records no significant effect. When decomposing the measure of financial development into financial markets and financial institutions development, we find that German civil law promotes both while English common law only financial institutions development.

KEYWORDS - Legal origin; Financial development; Financial markets; Financial institutions; Propensity score matching

JEL CLASSIFICATION - C11; G28; K20; O16; P5

1 | INTRODUCTION

Two pioneering studies on the relationship between law and finance, La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1997, 1998), highlight that cross-country differences in legal origin associated with various effects of ‘rule of law’ are essential for explaining protection of investor, creditor and private property rights as well as the level of financial development across countries.

Their argument (see also La Porta et al., 2000, 2006, 2008) is that if the legal system can properly enforce the rights of investors (e.g., voting rights) and creditors (e.g., liquidation rights), investors and creditors will be willing to finance firms, as the agency costs can be reduced, for instance, by effectively monitoring insiders or properly regularising contracts that conform to the incentives of insiders and the motivation of shareholders. The protections of investors and creditors’ rights at firm level are often embodied in firms by financial contracts which, however, sometimes impede enforcement, due to their complexity. It follows that such protections should be provided by securities, company and bankruptcy laws instead. As such, we should expect financial development to be higher in countries which legislate for protections on investors and creditors’ rights. La Porta et al. (1997, 1998) test this hypothesis and show that English common law countries have the strongest legal protection of private investors’ rights as well as more developed capital markets. French civil law countries have the weakest protection and the least developed capital markets, with German and Scandinavian civil law countries ranked in the middle.

Beck, Demirguc-Kunt, and Levine (2001, 2003) identify two channels to rationalise the superior performance of English legal origin: the ‘political’ and ‘adaptability’ channels. The former highlights that while the English common law tradition of private property rights forms the basis of financial development, civil law systems, such as the French, promote the

development of institutions that strengthen State power with negative consequences for financial development. The adaptability channel stresses that legal systems that are able to adapt quickly foster financial development more effectively. Hence, French legal origin countries are more likely to develop inefficiently rigid legal systems than German civil law and especially English common law countries¹ with negative repercussions for financial development. Beck et al. (2003) find evidence more supportive of the ‘adaptability channel’.

Various extensions and new permutations of the legal origin hypothesis have emerged over the past two decades, themselves based on the theoretical premise that finance is legally determined or constructed. For example, Djankov, McLiesh, and Shleifer (2007) argued that legal origins are an important determinant of both creditor rights and information-sharing institutions and found that common law countries have more highly developed systems of private credit than civil law ones.² Pistor (2013) went as far as proposing a legal theory of finance as a foundation for a political economy of finance, arguing that the legal construct of finance is of first-order significance in describing and forecasting the behavior of financial markets globally. Sarpong and Deodutt (2019) recently applied network science to provide empirical evidence from global financial networks in support of Pistor’s (2013) theory, and they found that global financial networks are hierarchical.

Despite the above, the theoretical link between law and finance remains a controversial issue. Indeed, as observed by Beck and Levine (2008, p. 252), “Countervailing theories and evidence challenge both parts of the law and finance theory. Many researchers accept that effective investor protection facilitates efficient corporate financing and growth-enhancing financial development, but reject the law and finance’s view that legal origin is a central determinant of investor protection laws and financial development”. Along the same lines, Acheson, Campbell, and Turner (2019) substantiate the contentious nature of the law

and finance theory by stating that there has been substantial criticism, with many researchers questioning whether law actually does matter and, in particular, the theoretical premise that the common law system provides a more favourable basis for financial development than the civil law tradition (see, among others, Acemoglu & Johnson, 2005; Deakin, Sarkar, & Singh, 2011; Graff, 2008; Rajan & Zingales, 2003; Roe & Siegel, 2011).

The empirical side of the legal origin-financial development literature is also mixed. For example, Bordo and Rousseau (2006) test the influence of legal origin on financial development measured by broad money as a ratio to GDP for 17 countries over the period 1880-1997. Their results indicate that the relationship between a country's legal origin (English, French, German, or Scandinavian) is not persistent. However, as they themselves acknowledge, broad money is an imperfect measure of financial development, one which mainly captures bank sector development but neglects the development of other financial markets, e.g., the stock market. Graff (2008) questions the view that English common law provides a more favorable foundation for financial development than civil law and shows that although legal origin is essential in shaping corporate law, there is not much evidence that English common law countries protect financial investors better than civil law countries. Armour, Deakin, Sarkar, Siems, and Singh (2009) find no correlation between shareholder protection and stock market development. Their analysis fails to find a positive link even after controlling for legal origin and countries' positions on the World Bank 'rule of law' index. Oto-Peralías and Romero-Ávila (2014) find that English common law countries do not exhibit greater financial development than French civil law countries when initial endowments are sufficiently high.

In this paper we are not specifically concerned with the wider link between law and finance, with comparing the historical evolution of the two main secular traditions of

common law and civil law, or with the underlying theoretical mechanisms that for a variety of cultural, political or economic reasons underpin how several sub-traditions - French, German, Socialist and Scandinavian - within civil law differ among themselves. Rather, our central interest is to square the still unresolved empirical question of how each country-based legal origin compares with all others in affecting financial development. This primary objective allows for a straightforward empirical assessment of the original version of the legal origin hypothesis (La Porta et al., 1998; La Porta et al., 2008; Beck et al., 2001 and 2003) that postulates the supremacy of the common law system versus the civil law tradition in fostering financial development. Several important innovations underlie our contribution, which we discuss below.

One common element of all empirical studies to date is the use of standard, linear regression analysis. However, if there are any nonlinearities in the relationship between any independent variables other than non-legal origin and financial development, and if such variables are also correlated with legal origin, a linear regression failing to account for such nonlinearities and correlations will be mis-specified. The problem does exist, since there is evidence pointing to a nonlinear relationship between institutional quality and financial development (Law & Azman-Saini, 2012), and legal origin is a key determinant of institutional quality (Fergusson, 2006). Several prior studies rely on instrumental variables (IV) estimation (e.g., Beck et al., 2003), which treats legal origins as instruments to deal with endogeneity in the second-stage regression. But if such instruments are correlated with the second-stage residuals - leading to a violation of the exclusion restriction - results obtained from IV estimation will be inconsistent (Nunn, 2007).

Against this backdrop, we contribute to this debate by advancing on prior empirical work in several respects. First, as a major methodological advance over what has gone

before, we use propensity score matching (PSM) to investigate the effect of legal origin on financial development. PSM has been described as the most developed and popular approach for processing data to improve causal inference in observational studies (Pearl, 2010). The main advantage of PSM in our context is that it does not rely on classical regression assumptions such as linear functional form or serially non-correlated errors, and can satisfactorily deal with the endogeneity problem (Cushman & De Vita, 2017; Liu, Xiao, & Xie, 2019). The latter virtue could be particularly advantageous in application to the present study given that legal origin is a variable that is constant at the country level and there may be many other country-level factors correlated to it, so it is impossible to isolate the effect of legal origin from the effect of other country-wide institutions. Conceptually, this does not constitute an issue since, according to the updated definition by La Porta et al. (2008), legal origin is best viewed as a broad concept that includes how the state approaches the social control of the economy, literally, "legal origin as a style of social control of economic life (and maybe of other aspects of life as well)" (p. 285). However, for empirical estimation, this also means that differences between the French, German and Scandinavian law systems based on the civil law tradition may reflect influences of their respective cultures, and as recently shown by Ang (2019), national culture (measured by an orientation towards individualism or collectivism) can also be highly correlated with financial development, raising fresh concerns for potential endogeneity issues.

Second, whilst most empirical studies approximate financial development using very narrow and admittedly inadequate measures of financial depth (thus often plaguing the reliability of results reported), we adopt a newly developed (Svirydzenka, 2016), broad-based financial development index. By incorporating information on a broader range of financial development features and a wider array of financial agents and, in addition, by capturing how developed financial institutions and financial markets are in terms of their depth, access and

efficiency, such index better reflects the overall level of financial system development across countries.

Third, we further investigate the separate effect of legal origin on the two main components of aggregate financial development, namely, financial markets development and financial institutions development, an analysis that is absent in prior literature.

Finally, our analysis benefits from the largest and most up-to-date dataset employed in the analysis of the legal origin-financial development nexus, covering 178 countries over up to 37 years of annual data (from 1980 through to 2016).

Our main results, which prove robust to a wide range of sensitivity tests, show that German civil law countries have the highest level of financial development and that German legal origin has a stronger positive impact on financial development than English common law. This is, by itself, an important result, but a significantly novel further contribution we make to this literature via our disaggregated analysis, is to unveil what drives this result. Specifically, whilst German legal origin is found to have a positive and significant effect on both financial market development and financial institutions development, English legal origin only has a positive effect on financial institutions development.

The paper is organized as follows. Section 2 describes the data used. Section 3 explains the methodology, PSM, and its implementation. Section 4 presents and discusses the empirical results, including robustness tests and a further analysis of the effect of legal origin on the two main components of financial development. Section 5 concludes.

2 | DATA

We use a large dataset comprising 178 countries over the period 1980-2014 (though as part of our robustness tests we extend the sample period to 2016). Our sample includes 60 countries with English common law, 76 countries with French civil law, 32 countries with Socialist law, five countries with German civil law, and five Scandinavian civil law countries.

In PSM one variable is the response or outcome variable - here, financial development - and another a treatment assigned variable, here, legal origin.³ Most previous empirical studies approximate financial development using one of two measures of financial depth: the ratio of private credit to Gross Domestic Product (GDP) (see, e.g., Ang, 2019, who uses the average ratio of private credit to GDP), and stock market capitalization, again as a ratio to GDP. Nevertheless, such narrow measures do not reflect the multi-faceted make-up of financial development, especially given the diversity of financial systems across countries. To overcome the drawbacks inherent in adopting single, narrow indicators as proxies for financial development, as our outcome variable we employ a newly proposed (Svirydzhenka, 2016), broad-based financial development index. In constructing this aggregate index, Svirydzhenka's (2016) methodology draws on Čihák, Demirgüç-Kunt, Feyen, and Levine's (2012) conceptual approach to defining financial development. However, as she notes, her dataset advances on Čihák et al.'s Global Financial Development Database (GFDD) by supplementing the World Bank FinStats (a more updated version of the GFDD introduced by Čihák et al., 2012) with additional data from the Bank of International Settlements (BIS) debt securities database, the Dealogic corporate debt database, and the IMF Financial Access Survey. Svirydzhenka aggregates numerous sub-indices (originally developed by Sahay, Čihák, N'Diaye, & Barajas, 2015) that summarize how developed financial institutions and financial markets are in terms of their depth, access and efficiency.⁴ Financial institutions include banks, insurance companies, mutual and pension funds, while financial markets comprise of stock and bond markets. Another significant advantage of this recently

developed financial development measure vis-à-vis alternatives lies in its vast coverage of country-year observations. Unlike alternative proxies that exhibit considerable limitations in terms of data availability, with many missing markets and observations, this broad-based index offers a large sample of adjusted series constructed through a robust procedure that avoids the creation of artificial data. The procedure entails applying data treatment to actual, observed data, winsorizing at the 5th and 95th percentiles cut off levels to avoid extreme observations, ranking countries for each indicator using the min-max technique, and constructing sub-indices as a weighted average of the underlying series, where the weights are squared factor loadings (sum to 1) from principal component analysis of the underlying series (Svirydzenka, 2016).

Additionally, we use disaggregated measures of the two main components of this aggregate financial development index - financial markets development and financial institutions development - separately for further analysis.

For our treatment variable, we use the updated version of the legal origin dataset of La Porta et al. (1999).⁵ This dataset, which extends that of La Porta et al. (1998), identifies the legal origin of the Company Law or Commercial Code of each country in our sample. There are five different possible origins: (i) English Common Law; (ii) French Commercial Code; (iii) German Commercial Code; (iv) Scandinavian Commercial Code; (v) Socialist/Communist Law. We then employ dummy variables to capture the local origin effect between control and treatment groups.

In the section below we discuss decisions regarding additional variables (covariates or confounders) to be included (or otherwise) in the model. The definition of each variable used, and their respective sources, are detailed in the on-line Appendix A, Table A1. Table A2 of

Appendix A additionally presents summary statistics for the full sample (Panel A), also with respect to the mean value of each variable by legal origin (Panel B).

From Table A2 we can see that over the full sample (Panel A), average financial development is 0.249, with a standard deviation of 0.211. On average, the level of financial institutions development (mean = 0.336) is higher than the level of financial markets development (mean = 0.159). As shown, the standard deviations for financial development, financial institutions development and financial markets development, are quite dispersed around their mean. Panel B of Table A2 shows that German legal origin countries have the highest level of financial development (0.679), followed by Scandinavian (0.565), English (0.267), French (0.221), and Socialist legal origin countries (0.168).

3 | METHODOLOGY

Despite its technical complexity, PSM (Rosenbaum & Rubin, 1983) has become a popular strategy for causal analysis of treatment effects. In addition to the aforementioned advantages (of not having to rely on regression assumptions such as linear functional form, serially non-correlated errors or lack of endogeneity), PSM satisfactorily deals with the selection bias problem. PSM searches for a dataset that might have resulted from a randomized trial but is hidden in the data at hand, the observational dataset (Austin, 2011). Its matching process ensures that both the treatment and control groups have a similar distribution of covariates, and that a treatment impact mimics that of a randomized control trial.⁶ By reducing the imbalance in the distribution of the pre-treatment confounders (covariates) between the treated and control groups (Stuart, 2010), PSM reduces the degree of model dependence in estimation of causal effects, thereby lowering both inefficiency and bias (Iacus, King, & Porro, 2011).

PSM begins with a model estimating the fitted probability or ‘propensity score’ to receive treatment, controlling for other covariates. Next, it matches observations sharing a similar propensity score to be in either the treated or non-treated (control) groups with any difference in the mean outcome being attributable to the treatment. Finally, the mean difference in outcome between treated and controls across groups gives the estimate of the treatment effect, which is analogous to the interpretation of the coefficient for a legal origin dummy variable in a regression model.

When estimating the propensity score, two initial decisions must be made: (i) the model to be used for estimation; and (ii) the variables to be included in the model. With respect to (i), we estimate a logit model to obtain the propensity score.⁷ The superiority of logit (or probit) models over linear probability models lies in the well-known shortcomings of the latter (see discussion in Caliendo & Kopeinig, 2008).

In terms of variable choice, i.e. the inclusion (or exclusion) of covariates in the propensity score model, our matching strategy builds on the Conditional Independence Assumption (CIA), requiring the outcome variable to be independent of treatment conditional on the propensity score. Our matching approach, therefore, starts by choosing a set of variables that credibly satisfy the CIA condition. In choosing the right-hand variables for the specification of the logit equation, as discussed in Cushman and De Vita (2017), we consider both ‘potential confounders’ that affect outcome (i.e., financial development), and ‘true confounders’ that affect both treatment assignment and outcome (legal origin and financial development). To this end, we began by considering the variables used in Beck et al. (2003). They use two-stage least squares IV regressions of financial development on political indicators such as ‘tenure of supreme-court judges’ and ‘supreme-court power’ in the second-stage regression, and legal origin variables as instruments for political indicators in the first-

stage regression. Nevertheless, to avoid post-treatment bias - stemming from the inclusion of a consequence-of-treatment variable as a covariate - we exclude any institutional quality variables such as supreme-court power and democracy. For example, Beck et al. (2003) indicate that civil law countries are less likely to grant lifelong tenure to judges than common law countries. Moreover, variables related to a supreme court could be just a proxy for non-civil law systems. Indeed, one frequent characteristic of the civil law tradition is to have a dual legal system, a private and an administrative one, without a common supreme court but two ‘supreme courts’, for instance, in France, *Cour de Cassation* and *Conseil d'Etat*. Hence, exclusion is warranted. We then reviewed the list of variables used by Chinn and Ito (2006). They investigate various factors expected to influence financial development and find that both financial openness and trade openness are important determinants. Next, macroeconomic variables such as GDP growth and inflation, and a time trend, are commonly used as right-hand variables of regression models in previous financial development studies (see, e.g., Yang, 2011). Therefore, we include the following covariates: *inflation, financial openness, GDP per capita, trade openness* and a *time trend*.

Following the ‘sequential elimination of regressors’ procedure (as in Cushman & De Vita, 2017), we rely on statistical significance and general-to-specific modelling in our search for a parsimonious specification of the model.⁸ We begin this process by dropping the least statistically significant variable. Next, we estimate the resulting model and, once again, drop the least significant variable until all remaining variables are significant at the 0.10 level.

Consistent with Rosenbaum and Rubin (1983), we define the propensity score for unit i as the conditional probability of assignment to treatment ($T = 1$) vs. comparison ($T = 0$) given covariates, X :

$$(1) \quad PS(X_i) = \Pr(T = 1|X_i)$$

Once the propensity score (*PS*) is calculated, we check if it is balanced (equal mean *PS*) within blocks across treatment and control groups. That is, whether the two groups are balanced in the observables to the treated sample. Next, we check each covariate across the two groups within blocks of the *PS*. If the balancing test fails (at the Simes-adjusted 0.05 level), following Dehejia and Wahba (2002), we revise the *PS* specification by adding higher order terms of the covariates until the test is passed.

A further requirement is to check the overlap condition, i.e. the region of common support between treatment and comparison groups.⁹ Following Lechner (2008), we use visual analysis of the density distribution of the *PS* graph to verify the common support condition.¹⁰ Figures B1 to B5 in the on-line Appendix B, show that there is a good overlap between our treatment and control groups.

Finally, we compute the average treatment effect on the treated (ATT). ATT refers to the mean value of the difference in outcome for each matched treated and control unit after conditioning on the *PS*. For comprehensiveness and to verify robustness, we apply four distinct matching algorithms to compute ATT: $E(Y_1 - Y_0/X, T=1)$, namely, nearest neighbor (ATTnd), radius (ATTTr), stratification (ATTs), and kernel (ATTk).

There are inevitable trade-offs involved in each algorithm, particularly with respect to bias and variance. ‘ATTnd’ poses the risk of bad matches if the nearest neighbor (in terms of propensity score) is far away. ATTTr reduces the risk of bad matches by imposing a tolerance limit on the maximum propensity score distance (radius) and then using not only the nearest neighbor, but all the comparison members within the radius. Here we use a radius of 0.1, thus increasing matching quality. ATTs entails partitioning the common support of the propensity score into a set of strata and then computing the effect within each stratum by taking the mean difference in outcomes between treated and control units. The resulting advantage of

this algorithm is a reduction of bias associated with the covariates. The drawback is that the number of strata required to remove most if not all of such bias, is not known *a priori*. We choose five strata following the advice of Cochran and Chambers (1965). They show that five strata are often enough to remove 95% of the bias associated with a single covariate.¹¹ Unlike the matching algorithms discussed above - which only use a few observations to generate the counterfactual outcome of treatment - ATTK is a non-parametric matching estimator that uses weighted averages of all units in the control group to construct the outcome. Hence, by using more data, the kernel algorithm has the advantage of a lower variance, at the cost of potentially using observations that are ‘bad matches’. Because of this, the reliability of ATTK relies more heavily on the satisfaction of the common support condition.

4 | EMPIRICAL RESULTS

4.1 | Main PSM results

This section presents and discusses our main PSM results. The ATT results from different matching methods are reported in Fig. 1 to 5 and Table 1 to 5. For each method, bootstrapping is based on 1,000 replications. Each figure presents four confidence intervals with different ATT values (from left to right, ATTK, ATTnd, ATTr, and ATTs). We analyze graphs using both the Bayesian posterior probabilities and the standard statistical approach. As noted by Greenland and Poole (2013), and Cushman and De Vita (2017, p. 151), “A classical x percent confidence interval is approximately a Bayesian x percent posterior probability interval under weak priors”. The heading of each figure and table gives the pair of treatment and control groups. For example, the heading of Fig. 1, ‘English vs. Non-English’

refers to countries with English legal origin as the treatment group vs. countries with Non-English legal origin as the control group.

Fig. 1 shows that the confidence intervals (CIs) lie largely on the positive side of zero implying Bayesian posterior probabilities substantially in excess of 0.50 for a positive effect of English legal origin on financial development. However, all ATT 0.95 CIs include zero, suggesting that, statistically, the significance of English legal origin is weak. In Fig. 2 all CIs lie below zero. Both the Bayesian perspective and standard statistical significance suggest that French legal origin has a negative effect on financial development. In Fig. 3, the CIs lie entirely above zero, offering strong evidence in favor of a positive effect of German legal origin. Fig. 4 exhibits a negative effect of Scandinavian legal origin while Fig. 5 suggests that Socialist law has no statistically significant effect.

The results reported in Table 1 to 5, are consistent with those illustrated by the graphs. The mean of the four ATT values is reported in the bottom row of each table. Table 1 shows that English legal origin countries have a 1% higher level of financial development than Non-English legal origin countries. In Table 3, the level of financial development in German legal origin countries is 19.6% higher than Non-German legal origin countries. By contrast, countries with French and Scandinavian legal origin (see Table 2 and 4, respectively) have a 7.9% and 5.8% lower level of financial development, respectively, compared to other countries. The impact of Socialist law on financial development, shown in Table 5 (average ATT = 0.002), appears to be insignificant. Although consistent with the results inferred from the graph, this is a surprising finding when considering that, at least for the stock market development component of financial system development, the 32 Socialist law countries included in our sample exhibited a very weak level that is difficult to reconcile with the absence of an effect of Socialist legal origin; an issue we will return to in our robustness tests.

King and Nielsen (2016) recommend that when using PSM, the researcher should closely follow the advice in the literature and provide full information and diagnostics to readers so that they can understand what was done. They also argue that PSM is more efficient in cases where relatively large sample sizes are available after matching so as to both avoid going past the point of complete randomization and evidence that the difference after matching between PSM and a completely randomized experiment is small and inconsequential. We follow their advice by also reporting as a diagnostic measure in Table 1 to 5 the number of observations (or units) ‘after matching’, for each algorithm. When compared to the sample sizes before matching (reported in our summary statistics of the Appendix Table A2), it is evident that ‘after matching’ we are still availing ourselves of a large number of observations (relatively large ‘matched’ sample sizes) distributed across the respective treated and control groups. For example, for each legal origin, before matching we have 6,230 observations between treated and control groups, with the number of ‘treated’ units ranging from 175 for German and Scandinavian legal origin to 2,660 for French civil law. After matching, taking kernel (ATTk) as the algorithm of reference, the ‘matched’ sample sizes for the various legal origins examined remain relatively large, ranging from 667 observations for Scandinavian civil law (156 ‘treated’ and 511 ‘control’ units) to 4,763 observations for French civil law (2,083 ‘treated’ and ‘2,680’ control units).

4.2 | Robustness and sensitivity tests

To reassure as to the robustness of our results, we subject them to a battery of sensitivity tests. First, still using PSM, we directly compare the strength of the effect of German civil law *vs.* English common law on financial development. The results from this permutation, shown in Table 6, indicate that the level of financial development in German civil law

countries is 21.5% higher than in English common law countries. Fig. 6 corroborates this result.

Second, we wish to investigate further the finding of no effect of the Socialist law group of countries. As noted by La Porta et al. (2008), the Socialist legal tradition originates in the Soviet Union, and was spread by the Soviet armies first to the former Soviet republics and later to Eastern European countries under the communist regime. Yet, after the fall of the Berlin Wall, many such Eastern European countries reverted-back to their pre-World War II legal systems based on German or French civil law. The updated legal origin data set by La Porta et al. (1999) that we have used thus far, classifies these transition economies as having the Socialist legal system, a choice susceptible to objections. Accordingly, we now re-estimate our model using an alternative data set developed by La Porta et al. (2008), which re-classifies such countries based on their new commercial laws, precisely to address these criticisms. In practice, this results in a drop in our sample of Socialist law countries from 32 to one, namely, Myanmar. Additionally, we lose three countries that due to data unavailability no longer feature in the La Porta et al. (2008) data set (Marshall Islands, Republic of Kiribati, and Brunei Darussalam). This new sample, from 1980 to 2014 for 175 countries, now includes 57 countries with English common law, 19 countries with German civil law, 93 countries with French civil law, one country with Socialist law, and five Scandinavian civil law countries. Despite a slightly smaller sample of English common law countries (from 60 to 57) and a considerably larger sample of German civil law countries (from five to 19), the results (not reported to conserve space but available from the authors) confirm that German legal origin countries have the highest positive impact on financial development, with an effect higher than English legal origin countries by 6.5% (0.065). In terms of the effect of Socialist legal origin, the average ATT value is negative (-0.099), but it is impossible to ascertain whether such an effect is statistically significant since there is only

one country (Myanmar) in our sample and we are, therefore, unable to obtain 95% confidence intervals or p-values via bootstrapping for all our ATT matching methods. Given such uncertainty, we additionally re-estimate the data set based on La Porta et al.'s (1999) Socialist legal origin countries while restricting the sample period from 1980 to 1989, the year marking the fall of the Berlin Wall. As shown in Fig. 7 and Table 7, also these PSM results based on a larger sample of Socialist law countries from 1980 to 1989, confirm the lack of a statistically significant effect of the Socialist/Communist legal origin on financial development, according to both the Bayesian and the conventional statistical approach.

Third, availing ourselves (at March 2019) of a further updated version of Svirydzenka's (2016) financial development index with data up to 2016 (see <http://data.imf.org/?sk=F8032E80-B36C-43B1-AC26-493C5B1CD33B>), we re-estimate the main PSM results reported in the section above using two additional years of data (1980 to 2016). The results from this extended sample (not reported here to conserve space but available from the authors) are similar to the earlier PSM results based on the period 1980-2014, leading to the same conclusions. They show that English legal origin countries have a 1.6% (0.016) higher level of financial development than Non-English legal origin countries (compared to 1% higher in Table 1) while the level of financial development in German legal origin countries is 14.9% higher (0.149) than Non-German legal origin countries (compared to 19.6% in Table 3). Countries with French and Scandinavian legal origin have a 6.5% (-0.065) and 9.1% (-0.091) lower level of financial development, respectively, compared to other countries - results which again are fairly consistent with the 7.9% (-0.079) and 5.8% (-0.058) lower level of financial development reported for these countries in Table 2 and 4, respectively. The effect of Socialist law on financial development, also in this extended sample period, is statistically insignificant, registering a negative percentage value of 0.8 (-0.008) compared to a positive 0.2% in Table 5 (0.002).

Finally, to determine the sensitivity of our results to the methodology employed (PSM), we also re-estimate the model using pooled Ordinary Least Square (OLS) (columns 1 and 2 of Table 8), a regression method commonly employed in empirical studies linking legal origin to financial, economic or social outcomes (see, e.g., D'Amico & Williamson, 2017; Rambaccussing & Power, 2018), and cross-section OLS (columns 3 and 4 of Table 8). At the cost of losing many country-year observations for the covariates, the latter cross-section OLS empirical exercise may be justified by the fact that our treatment variable (legal origin) is constant over time, hence we take averages (over the full sample period) of the outcome variable and covariates and use a cross-section of countries rather than a panel, as done, for example, by Beck et al. (2003). Akin to our PSM estimations of Table 6, the pooled OLS estimations reported in columns (1) and (3) of Table 8 only use observations for German legal origin countries and English common law countries, with their distinct effect this time captured by means of a dummy variable ('Ger-Eng') which takes the value of one for German legal origin and zero for English legal origin. Moreover, without the constraints posed by PSM in terms of potential post-treatment bias stemming from the inclusion of consequence-of-treatment variables as covariates, we are now able to add several additional control variables. Specifically, following Acemoglu, Johnson, Robinson, and Yared (2008), we now include the year of independence ('Ind-Year') to the regressions, with data sourced from the world factbook. We also now include 'Population', measured as the natural logarithm of total population, and institutional quality ('Ins-Quality'). Following Khan, Khan, Abdulahi, Liaqat, and Shah (2019), 'Ins-Quality' is measured by taking the average of six indicators (namely, government stability, corruption, democratic accountability, bureaucratic quality, law and order, and investment profile) with data sourced from the International Country Risk Guide (ICRG) database.

As shown in column (1) of Table 8, ‘Ger-Eng’ is positive (with an estimated value of 0.033) and statistically significant at the 1% level, thus corroborating our PSM results.¹² In column (2) of Table 8, we also report pooled OLS estimations using the full sample, where we included two dummies in the regression model, ‘English vs. Non-English’ and ‘German vs. Non-German’ legal origin. Once again, the results support our earlier findings by indicating that whilst both dummies record positive values, the estimated coefficient for the ‘German vs. Non-German’ legal origin dummy (with a value of 0.116, statistically significant at 1%) is considerably larger than the coefficient for ‘English vs. Non-English’ legal origin (equal to 0.064, also significant at 1%). Almost identical conclusions can be drawn with respect to the cross-section OLS estimates reported in columns (3) and (4) of Table 8, obtained using averages of the outcome variable and covariates.

While we consider the PSM results as the most reliable, it is somewhat reassuring that despite their susceptibility to violation of standard regression analysis assumptions, pooled as well as cross-section OLS estimations yield results broadly consistent with those obtained using PSM. Contrary to several prior studies (see, e.g., Beck et al., 2003; La Porta et al., 1998), we consistently find that German civil law has the highest positive impact on financial development. English common law countries follow. However, given our robustness tests, the discrepancy cannot be accounted for by our use of PSM. Rather we take our use of the newly developed, broad-based, financial development index, and the largest and most comprehensive dataset employed to date in the analysis of the legal origin-financial development nexus, as the defining features that explain the difference between our results and those obtained in existing studies. For example, Beck et al. (2003) use private credits over GDP (financial intermediary credits to the private sector divided by GDP) and stock market development (total value of outstanding equity shares as a fraction of GDP) as proxies for financial development. Both these measures are very narrow, and incapable of capturing

the depth, access and efficiency of financial institutions and financial markets, especially across diverse financial systems across countries. For example, private credit excludes credit to the public sector and cross claims of one group of intermediaries on another. Hence, it captures solely the amount of savings channelled through debt-issuing financial intermediaries to private borrowers. Similarly, stock market development equals the total value of outstanding equity shares as a fraction of GDP, a measure suitable to approximate the overall size of the equity market relative to the size of the economy but one that neglects banking sector development, a critical component of financial system development. Moreover, in Beck et al. (2003) both measures are averaged from 1990 to 1995 in their cross-section estimations, a relatively short sample period that compares rather unfavourably with our panel data estimations of 178 countries of country-year observations over an extended sample period running from 1980 through to 2016.

4.3 | Further analysis

The results presented above indicate that German civil law countries have the highest level of financial development and English common law countries follow. A further question begs: Which component of financial development is driving such effect?

To address this question, we interrogate the data further by separately testing the effect of legal origin on the two main components of financial development, namely, financial markets development and financial institutions development. The ATT results using financial markets development as the outcome variable are shown in Fig. 8 to 12 and Table 9 to 13. The results using financial institutions development as the outcome variable are shown in Fig. 13 to 17 and Table 14 to 18.

Fig. 8 and Table 9 suggest that English legal origin has no effect on financial markets development. In Fig. 9, all CIs lie below zero, indicating that French legal origin has a negative effect on financial markets development based on the Bayesian perspective but according to the standard statistical approach, as shown in Table 10, French legal origin has no statistically significant effect on financial markets development. Fig. 10 and Table 11 provide strong evidence in favor of a positive effect of German legal origin on financial markets development based on both the Bayesian perspective and the standard statistical approach. In Fig. 11 and Table 12, and Fig. 12 and Table 13, the results suggest that both Scandinavian legal origin and Socialist law have no effect on financial markets development.

Turning to the results for financial institutions development, Fig. 13 and Table 14 show that according to both the Bayesian perspective and standard statistical significance, English legal origin countries have higher financial institutions development than Non-English legal origin countries. The Bayesian interpretation of the CIs in Fig. 14 and 16 would suggest a negative effect of French and Scandinavian legal origin on financial institutions development. However, large p-values in Table 15 and 17 indicate that such negative effects can be discounted at statistically significant levels with any credibility. Fig. 15 and Table 16 both show a positive effect of German legal origin on financial institutions development. Finally, Fig. 17 and Table 18 concur in indicating that Socialist law has no significant effect.

The results presented in this section provide further evidence in support of our main findings and, additionally, unveil what drives them. The higher level of financial development in German civil law countries stems from both high levels of financial markets development and financial institutions development. On the other hand, English legal origin only has a positive effect on financial institutions development, with no effect on financial markets development according to our data. This result goes some way towards explaining

why German civil law countries emerge as having a higher level of (aggregate) financial development than English common law countries.

5 | CONCLUSION

Whilst it is generally acknowledged that financial development can promote economic growth (see, e.g., Levine, 1997), the theoretical link between law and finance remains a controversial issue and existing empirical evidence is conflicting. In this paper our interest exclusively centers in testing empirically how each country-based legal origin compares with all others in affecting financial development. This endeavor constitutes a straightforward empirical assessment of the initial legal origin hypothesis (La Porta et al., 1998; La Porta et al., 2008; and Beck et al., 2001 and 2003) that postulates the supremacy of the common law system versus the civil law tradition in fostering financial development. We use propensity score matching (PSM) and a new, broad-based financial development index (Svirydzenka, 2016) on a sample of 178 countries over the period 1980-2014. Contrary to several prior studies (see, e.g., Beck et al., 2003; La Porta et al., 1998), we find that German legal origin has a stronger positive impact on financial development than any other legal origin, including English common law. The discrepancy may be plausibly accounted for by our use of a newly developed, broad-based, financial development index, and the largest, most comprehensive and up-to-date data set employed in the analysis of the legal origin-financial development nexus. We also find that French and Scandinavian legal origins adversely influence financial development while Socialist law has no significant effect. Our results survive a wide range of sensitivity and robustness checks that also include an extension of the sample period (through to 2016), re-estimation by alternative methodologies, namely pooled and cross-section OLS, and the use of an updated legal origin data set developed by La Porta et al. (2008) as well as a

restricted sample period that accounts for changes in legal origin of Eastern European countries following the fall of the Berlin Wall in 1989.

We then interrogate the data further to assess empirically which component of financial development - financial markets development or financial institutions development - is mostly driving such effects. Our disaggregated analysis reveals that German civil law promotes both while English common law only financial institutions development. This finding goes some way towards explaining why German civil law countries are found to have a higher level of aggregate financial development than English common law countries.

The meaning of our findings on how each country-based legal origin compares with all others in affecting financial development is of particular theoretical significance in casting further doubt on the original version of the legal origin hypothesis (see La Porta et al., 1998; La Porta et al., 2008; and Beck et al., 2001 and 2003), which postulates that countries based on a common law system benefit from greater financial development than countries based on the civil law tradition.

Akin to other studies linking legal origin to financial, economic or even social outcomes (see, e.g., D'Amico & Williamson, 2015; Rambaccussing & Power, 2018), no normative statements or specific policy implications can be derived from our findings. This is because how nations' legal processes are organized and their historical traditions of governance (i.e., how they were founded or colonized by English common law or French, German, or Scandinavian civil law) - which is what the legal origin construct measures - clearly cannot be rewritten solely on the basis of evidence of how legal origin influences financial development or its main components (financial markets and financial institutions). Moreover, financial development is only one of many economic outcomes that distinct legal origins can influence (trade and foreign direct investment, are a case in point). That said,

national legacies of law and governance are not carved in stone, they evolve. For example, since the Xinhai Revolution of 1911, which marked the end of the imperial rule and the beginning of China's republican era, and more vehemently over the past four decades to adapt its legal system for economic reform, Chinese law has undergone gradual but incessant reform (Behr, 2007).

Similarly, the prospect of Brexit in the UK has raised fresh questions on the merits and demerits of the English common law legal system, and about whether English governing law and jurisdiction clauses are still an attractive proposition (Allen & Overy, 2018). While Brexit may not be in itself a good enough reason for the UK to move away from a choice of English law in commercial contracts, it has been advanced that is most likely to “tear” Ireland away from the influence of common law and possibly “even rupture” centuries-long ties between the Irish and English legal systems due to the significant pressure for Ireland, if the UK leaves the EU, to become part of a homogenous system of civil law over the next decade or two (The Irish Times, 2019).

Against this backcloth, and given that past academic results are subject to uncertainty, our study makes an important contribution by providing more robust empirical evidence on the extent to which different legal origins affect financial development and, most importantly, what drives such effects within the financial sector, hence informing the policy debate.

We are inevitably constrained in our ability to infer more precise optimality implications from our findings, which would require knowledge of the economic and social costs of changing the fundamental law order and of the wider benefits to be accrued from improving the financial system. Furthermore, such assessments are intuitively unnecessary for countries that already have a well-functioning legal system and developed financial markets and institutions. Nevertheless, questions pertaining to the relative costs and benefits

of national law system's reform (which were beyond the scope of the present study), especially in relation to countries considering gradual transition of their system of legal governance, provide a stimulating agenda for future research.

Endnotes

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1. As explained by Beck et al. (2003, p. 660), "The main argument focuses on jurisprudence and not adhering too rigidly to statutory law."
 2. The legal origin and property rights literature also connects with work linking financial development to economic growth (see, e.g., Claessens & Laeven, 2003; Levine, 1999). We do not consider these studies investigating links with economic growth since here our focus centers exclusively on the relationship between legal origin and financial development.
 3. The standard PSM tests for the binary case of treatment versus non-treatment but here we deal with multiple legal origin possibilities of treatment, namely, English common law, French, German and Scandinavian civil laws, and Socialist/Communist law. Whilst PSM can also be applied to multiple simultaneous treatments (Imbens, 2000), an equally effective approach is to estimate all the possible binary cases, which is the approach that, for empirical tractability, we use for the main estimations. As part of our robustness tests, we also compare directly 'German civil law vs. English common law'.
 4. Svirydzenka's (2016) original dataset contained annual data from 1980 to 2013 for 183 advanced, emerging, and low-income developing countries. For a detailed breakdown of the sub-indices used to measure the depth, access and efficiency of financial institutions and financial markets, see Svirydzenka (2016). We originally downloaded this index for all the countries in our sample at the end of 2017, from an updated version (with data up to 2014) available from the IMF (see <http://data.imf.org/?sk=F8032E80-B36C-43B1-AC26-493C5B1CD33B>).
 5. Available at: <https://scholar.harvard.edu/shleifer/publications/quality-government>.
 6. As stated by Austin (2011, p. 399), randomized controlled trials are regarded as "the gold standard approach" for estimating the effects of treatments on outcomes.
 7. Re-estimations using a probit model yield similar results.
 8. In dealing with the question of whether it is better to include too many rather than too few variables, our parsimonious specification is consistent with the arguments by Bryson, Dorsett, and Purdon (2002). They warn strongly against over-parameterized models when using PSM. First, because over-parameterization based on including extraneous variables in the model

increases susceptibility to the common support problem. Second, because although inclusion of insignificant variables does not lead to biased (or inconsistent) estimates, it does increase their variance.

9. This is the region (range) of propensity scores for which there is a positive probability of observing both control and treatment observations.

10. As noted by Caliendo and Kopeinig (2008), since lack of overlap can be seen by visually inspecting the propensity score distribution, there is no requirement to test for the existence of the common support problem by employing a sophisticated formal estimator.

11. Indeed, since all bias under unconfoundedness is associated with the propensity score, we can reasonably expect that, under normality, the use of five strata removes most of the bias associated with all covariates (see Caliendo & Kopeinig, 2008).

12. We obviously consider the PSM results as the most reliable. Nevertheless, it is somewhat reassuring that despite their susceptibility to violation of standard regression analysis assumptions, pooled as well as cross-section OLS estimations yield results broadly consistent with those obtained using PSM.

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TABLE 1 “English” vs. “Non-English”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	1,735	3,035	0.011		1.773	0.076
ATTnd	1,735	1,171	0.012	1.306	1.284	0.192
ATTr	1,735	3,035	0.011	1.721	1.829	0.067
ATTs	1,735	3,035	0.006	0.943	0.964	0.335
Average ATT			0.010			

Notes: The outcome variable is financial development. ‘Tr’ = number of treated units used to compute ATT. ‘Co’ = number of matched control units used to compute ATT. ATT = average treatment effect on the treated. t-stat-a = analytical t-statistic (blank if not available). t-stat-b = bootstrapped t-statistic. p-value = p-value using t-stat-b. First step covariates are: Lngdpp, inflation, inflation², inflation³, Time, Time², Chinn-Ito, Lntrade².

TABLE 2 “French” vs. “Non-French”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	2,083	2,680	-0.078		-11.583	0.000
ATTnd	2,083	1,176	-0.084	-8.886	-8.883	0.000
ATTr	2,082	2,680	-0.077	-12.480	-12.406	0.000
ATTs	2,083	2,680	-0.077	-11.251	-11.118	0.000
Average ATT			-0.079			

Notes: See Note of Table 1. First step covariates are: Lngdpp², inflation², Lntrade, Lntrade², Lntrade³, Lntrade⁴, Time², Chinn-Ito³.

TABLE 3 “German” vs. “Non-German”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	171	3,661	0.185		7.922	0.000
ATTnd	171	128	0.134	4.996	5.862	0.000
ATTr	165	3,661	0.341	26.319	5.396	0.000
ATTs	170	3,662	0.125		8.584	0.000
Average ATT			0.196			

Notes: See Note of Table 1. First step covariates are: Lngdpp, Inflation, Inflation², Time, Time², Chinn-Ito, Lntrade, Lntrade².

TABLE 4 “Scandinavian” vs. “Non-Scandinavian”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	156	511	-0.044		-1.668	0.092
ATTnd	156	98	-0.094	-3.132	-2.668	0.008
ATTr	156	511	-0.047	-2.937	-3.024	0.003
ATTs	156	511	-0.045	-2.164	-1.759	0.079
Average ATT			-0.058			

Notes: See Note of Table 1. First step covariates are: Lngdpp, Inflation³, Time², Chinn-Ito², Lntrade².

TABLE 5 “Socialist” vs. “Non-Socialist”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	586	3,451	-0.001		-0.133	0.894
ATTnd	586	517	0.016	1.523	1.348	0.178
ATTTr	586	3,451	-0.014	-2.131	-2.017	0.035
ATTs	586	3,451	0.007	0.991	1.000	0.317
Average ATT			0.002			

Notes: See Note of Table 1. First step covariates are: Lngdpp², Inflation, Time², Chinn-Ito³, Lntrade².

TABLE 6 “German” vs. “English”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	171	1,337	0.187		7.582	0.000
ATTnd	171	111	0.186	5.755	5.595	0.000
ATTTr	171	1,337	0.301	21.859	6.392	0.000
ATTs	171	1,337	0.180	9.397	6.091	0.000
Average ATT			0.214			

Notes: See Note of Table 1. First step covariates are: Lngdpp, Inflation, Time, Chinn-Ito, Lntrade².

TABLE 7 “Socialist” vs. “Non-Socialist”, 1980-1989

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	15	65	0.044		0.883	0.380
ATTnd	15	10	0.049	0.900	0.850	0.404
ATTTr	15	65	0.028	0.594	0.586	0.560
ATTs	15	65	0.025		0.451	0.653
Average ATT	15	65	0.044		0.883	0.380

Notes: Sample period from 1980 to 1989. Treatment equals Socialist legal origin. First step covariates are: Lngdpp², Inflation, Time², Chinn-Ito³, Lntrade².

TABLE 8 Pooled and Cross-section OLS estimations

	(1)	(2)	(3)	(4)
English		0.064*** (0.004)		0.072*** (0.016)
German		0.116*** (0.012)		0.130*** (0.042)
Ger-Eng	0.033*** (0.012)		0.148* (0.083)	
Inflation	-0.002*** (0.000)	-0.002*** (0.000)	-0.014*** (0.003)	-0.000*** (0.000)
GDPP	0.108*** (0.003)	0.093*** (0.002)	-0.016 (0.026)	0.064*** (0.008)
Trade	0.069*** (0.006)	0.031*** (0.006)	-0.040 (0.062)	0.005 (0.016)
Chinn-Ito	-0.083*** (0.009)	-0.009 (0.006)	-0.048 (0.096)	-0.015 (0.033)

Ind-Year	-0.076*** (0.007)	-0.024*** (0.003)	-0.029 (0.055)	-0.051*** (0.012)
Population	0.042*** (0.002)	0.038*** (0.002)	0.035** (0.013)	0.028*** (0.006)
Ins-Quality	0.024*** (0.005)	0.042*** (0.003)	0.082* (0.047)	0.074*** (0.014)
Constant	-0.078 (0.171)	-0.934*** (0.079)	0.389 (1.121)	-0.084 (0.286)
N	1133	3136	44	129
R-squared	0.869	0.806	0.616	0.854
Year Dummy	Yes	Yes		

Notes: Columns (1) and (2) are estimated using pooled OLS estimations with robust clustered (by country) standard errors in parentheses. Columns (3) and (4) are estimated using cross-section OLS taking averages of the outcome variable and covariates. In all regressions, column (1), (2), (3) and (4), the dependent variable is Svirydzenka’s (2016) aggregate financial development index. In columns (1) and (3), ‘Ger-Eng’ is a dummy variable taking value 1 if countries have German legal origin and 0 if countries have English legal origin. In columns (2) and (4), the dummy ‘English’ represents countries with English legal origin vs. Non-English legal origin countries, and the dummy ‘German’ represents countries with German legal origin vs. Non-German legal origin countries. ‘Chinn-Ito’ refers to the measure of financial openness by Chinn and Ito (2008). ‘Trade’ denotes the natural logarithm of trade openness. ‘Inflation’ is the inflation rate. ‘GDPP’ is the natural logarithm of GDP per capita. ‘Ind-Year’ denotes the year when the country became independent, with data sourced from the world factbook. ‘Population’ is the natural logarithm of total population. ‘Ins-Quality’ stands for institutional quality, measured as the average of six indicators (namely, government stability, corruption, democratic accountability, bureaucratic quality, law and order, and investment profile), with data sourced from the International Country Risk Guide (ICRG) database. Year dummy included. *, ** and ***, denote statistical significance at the 10, 5 and 1% level, respectively.

TABLE 9 “English” vs. “Non-English”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	1735	3035	0.004		0.581	0.561
ATTnd	1735	1171	0.004	0.388	0.406	0.677
ATTr	1735	3035	0.003	0.401	0.416	0.677
ATTs	1735	3035	0.000		-0.032	1.026
Average ATT			0.003			

Notes: See Note of Table 1. First step covariates are: Lngdpp, inflation, inflation², inflation³, Time, Time², Chinn-Ito, Lntrade².

TABLE 10 “French” vs. “Non-French”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	2083	2680	-0.093		-12.993	2.000
ATTnd	2083	1209	-0.096	-9.346	-9.248	2.000
ATTr	2082	2680	-0.084	-12.589	-12.277	2.000
ATTs	2083	2680	-0.094		-12.811	2.000
Average ATT			-0.092			

Notes: See Note of Table 1. First step covariates are: Lngdpp², inflation², Lntrade, Lntrade², Lntrade³, Lntrade⁴, Time², Chinn-Ito³.

TABLE 11 “German” vs. “Non-German”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	171	3661	0.218		9.063	0.000
ATTnd	171	128	0.161	4.698	5.293	0.000
ATTTr	165	3661	0.383	23.907	6.490	0.000
ATTs	170	3662	0.162		7.977	0.000
Average ATT			0.231			

Notes: See Note of TABLE 1. First step covariates are: Lngdpp, Inflation, Inflation², Time, Time², Chinn-Ito, Lntrade, Lntrade².

TABLE 12 “Scandinavian” vs. “Non-Scandinavian”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	156	511	-0.005		-0.135	0.107
ATTnd	156	98	-0.062	-1.578	-1.347	1.829
ATTTr	156	511	-0.011	-0.490	-0.500	1.383
ATTs	156	511	-0.006	-0.194	-0.172	1.137
Average ATT			-0.021			

Notes: See Note of Table 1. First step covariates are: Lngdpp, Inflation³, Time², Chinn-Ito², Lntrade².

TABLE 13 “Socialist” vs. “Non-Socialist”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	586	3451	0.002		0.315	0.753
ATTnd	586	517	0.017	1.379	1.210	0.226
ATTTr	586	3451	-0.012	-1.423	-1.340	1.820
ATTs	586	3451	0.008	1.020	1.054	0.292
Average ATT			0.004			

Notes: See Note of Table 1. First step covariates are: Lngdpp², Inflation, Time², Chinn-Ito³, Lntrade².

TABLE 14 “English” vs. “Non-English”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	1735	3035	0.018		2.629	0.009
ATTnd	1735	1171	0.020	2.076	1.988	0.047
ATTTr	1735	3035	0.020	2.898	2.996	0.003
ATTs	1735	3035	0.013	1.823	1.854	0.064
Average ATT			0.018			

Notes: See Note of Table 1. First step covariates are: Lngdpp, inflation, inflation², inflation³, Time, Time², Chinn-Ito, Lntrade².

TABLE 15 “French” vs. “Non-French”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	2083	2680	-0.066		-9.372	2.000
ATTnd	2083	1209	-0.068	-7.085	-6.972	2.000

ATTr	2083	2680	-0.071	-10.948	-11.317	2.000
ATTs	2083	2680	-0.063		-8.682	2.000
Average ATT			-0.067			

Notes: See *Note* of Table 1. First step covariates are: Lngdpp², inflation², Lntrade, Lntrade², Lntrade³, Lntrade⁴, Time², Chinn-Ito³.

TABLE 16 “German” vs. “Non-German”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	171	3661	0.149		6.542	0.000
ATTnd	171	128	0.106	4.369	5.410	0.000
ATTr	165	3661	0.295	25.828	4.505	0.000
ATTs	170	3662	0.087		6.691	0.000
Average ATT			0.159			

Notes: See *Note* of Table 1. First step covariates are: Lngdpp, Inflation, Inflation², Time, Time², Chinn-Ito, Lntrade, Lntrade².

TABLE 17 “Scandinavian” vs. “Non-Scandinavian”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	156	511	-0.083		-3.474	1.999
ATTnd	156	98	-0.125	-4.768	-4.052	1.999
ATTr	156	511	-0.083	-5.431	-5.577	2.000
ATTs	156	511	-0.083	-4.919	-3.620	1.999
Average ATT			-0.094			

Notes: See *Note* of Table 1. First step covariates are: Lngdpp, Inflation³, Time², Chinn-Ito², Lntrade².

TABLE 18 “Socialist” vs. “Non-Socialist”

	Tr	Co	ATT	t-stat-a	t-stat-b	p-value
ATTk	586	3451	-0.004		-0.544	1.414
ATTnd	586	517	0.015	1.368	1.214	0.225
ATTr	586	3451	-0.017	-2.261	-2.223	1.974
ATTs	586	3451	0.005	0.666	0.651	0.515
Average ATT			0.000			

Notes: See *Note* of Table 1. First step covariates are: Lngdpp², Inflation, Time², Chinn-Ito³, Lntrade².

APPENDIX A

TABLE A1 Variable, Definition and Source

Variable	Variable Definition	Source
Financial development	It represents countries on depth, access, and efficiency of their financial institutions and financial markets.	Svirydzenka (2016), http://data.imf.org/?sk=F8032E80-B36C-43B1-AC26-493C5B1CD33B
Financial market development	An aggregate of financial markets depth, access, and efficiency.	Svirydzenka (2016)
Financial institutional development	An aggregate of financial institutions depth, access, and efficiency.	Svirydzenka (2016)
Financial openness	It measures the degree of capital account openness. Construction of the measure is based on transactions from the IMF's Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER).	Chinn and Ito (2008), available from: http://web.pdx.edu/ito/Chinn-Ito-website.htm
Trade openness	Ratio of imports plus exports to GDP.	World Development Indicators
GDP per capita	Gross domestic product per capita.	World Development Indicators
Inflation	Inflation rate (annual percentage change of average consumer price index).	World Development Indicators
Independence year	The year when the country became independent.	The world factbook, available from: https://www.cia.gov/library/publications/the-world-factbook/fields/305.html
Population	The natural logarithm of total population.	World Development Indicators
Institutional quality	The average of six indicators: government stability, corruption, democratic accountability, bureaucratic quality, law and order, and investment profile.	International Country Risk Guide (ICRG)

TABLE A2 Summary Statistics

Panel A: Summary statistics (full sample)					
Variable	Obs	Mean	Std. Dev.	Min	Max
English	6,230	0.337	0.473	0.000	1.000
French	6,230	0.427	0.495	0.000	1.000
Socialist	6,230	0.180	0.384	0.000	1.000
German	6,230	0.028	0.165	0.000	1.000
Scandinavian	6,230	0.028	0.165	0.000	1.000
FinDev	6,230	0.249	0.211	0.000	1.000
FinMarDev	6,230	0.159	0.222	0.000	1.000
FinInsDev	6,230	0.336	0.228	0.000	1.000
Chinn-Ito	5,483	0.465	0.364	0.000	1.000
Trade	5,453	4.266	0.643	-3.863	6.276
Inflation	5,027	8.548	9.086	-1.582	54.290
GDPP	5,684	7.821	1.591	4.175	11.689
Time	6,230	18.000	10.100	1.000	35.000
Ind-Year	6,230	19.170	1.578	18.000	19.960
Population	6,224	15.453	2.022	10.327	21.033
Ins-Quality	3,829	4.648	1.219	0.472	7.381
Panel B: Mean by legal origins					
	English	French	Socialist	German	Scandinavian
FinDev	0.267	0.221	0.168	0.679	0.565
FinMarDev	0.165	0.126	0.105	0.609	0.480
FinInsDev	0.365	0.312	0.229	0.742	0.643
Chinn-Ito	0.484	0.422	0.415	0.836	0.747
Trade	4.376	4.164	4.354	4.054	4.270
Inflation	7.799	9.571	9.984	2.371	4.048
GDPP	7.815	7.578	7.555	10.031	10.324
Time	18.000	18.000	18.000	18.000	18.000
Ind-Year	19.169	18.822	19.611	18.726	18.680
Population	14.736	15.699	16.017	17.227	14.951
Ins-Quality	4.730	4.328	4.645	6.803	6.468
Observations	2,100	2,660	1,120	175	175

Notes: ‘English’ represents countries with English legal origin. ‘French’ represents countries with French legal origin. ‘Socialist’ represents countries with Socialist legal origin. ‘German’ represents countries with German legal origin. ‘Scandinavian’ represents countries with Scandinavian legal origin. ‘FinDev’ denotes financial development. ‘FinMarDev’ denotes financial market development. ‘FinInsDev’ denotes financial institutions development. ‘Chinn-Ito’ refers to the measure of financial openness (Chinn and Ito 2008). ‘Trade’ refers to natural logarithm of trade openness. ‘Inflation’ refers to the inflation rate. ‘GDPP’ is the natural logarithm of GDP per capita. ‘Time’ is the time trend. ‘Ind-Year’ is the independence year/100. ‘Population’ is the natural logarithm of total population. ‘Ins-Quality’ stands for institutional quality, measured as the average of six indicators (namely, government stability, corruption, democratic accountability, bureaucratic quality, law and order, and investment profile).

APPENDIX B

FIGURE B1 Density Distribution of Propensity Scores for English vs. Non-English

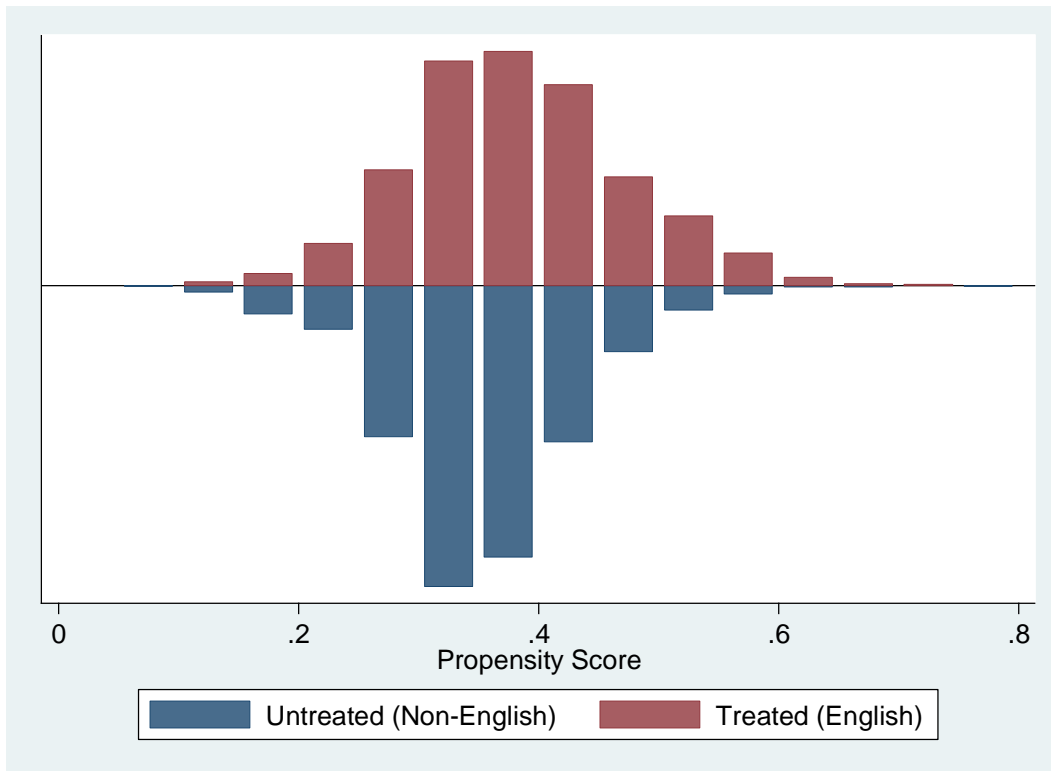


FIGURE B2 Density Distribution of the Propensity Scores for French vs. Non-French

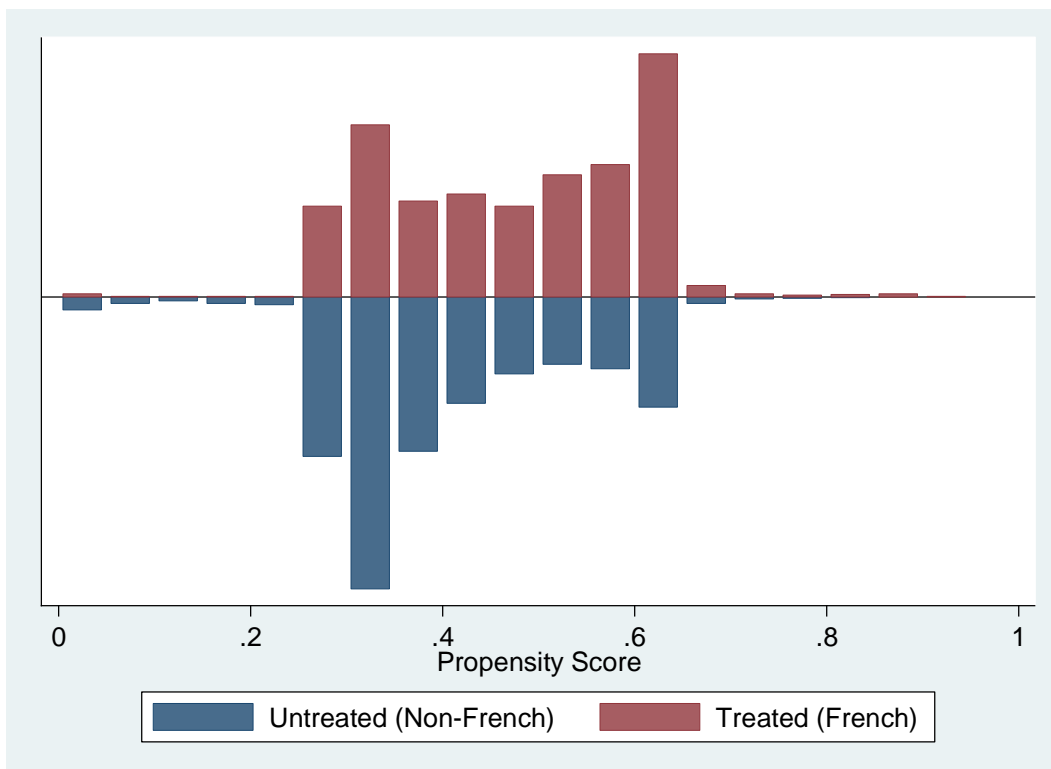


FIGURE B3 Density Distribution of the Propensity Scores for German vs. Non-German

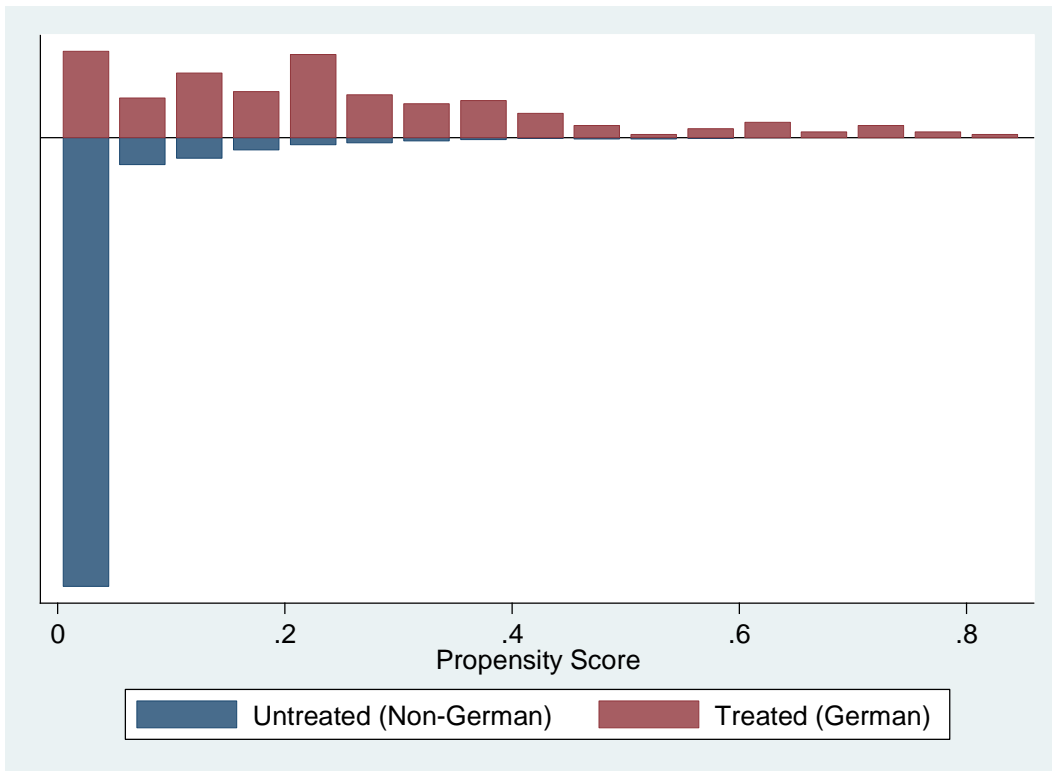


FIGURE B4 Density Distribution of the Propensity Scores for Socialist vs. Non-Socialist

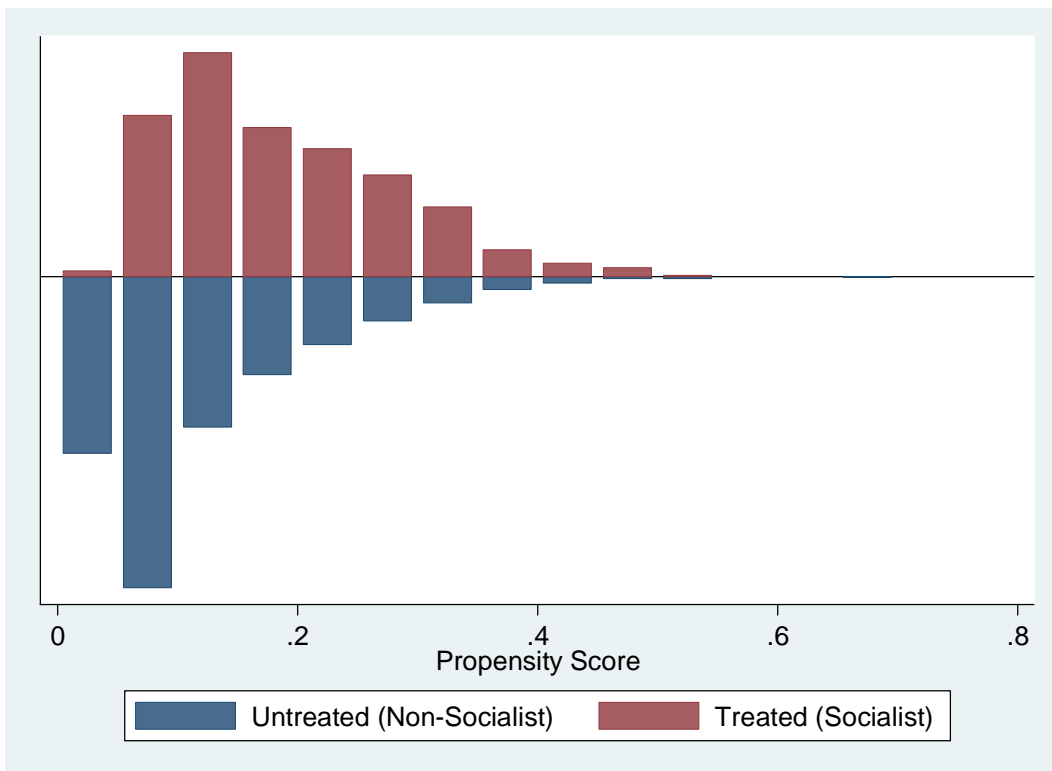


FIGURE B5 Density Distribution of the Propensity Scores for Scandinavian vs. Non-Scandinavian

