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State history and the finance-growth nexus: Evidence from transition economies

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ABSTRACT

In this paper, we analyze the growth effects of state history and financial development in transition economies. We show that accumulated experience with established statehood yields significant results and transforms the impact of finance on growth in East-Central Europe, Southeastern Europe and the former Soviet Union. State history as a proxy for long-run ancestral exposure to institutions, political organization and centralization negatively affects the finance-growth nexus. We argue that a long state history is likely to generate extractive institutions that facilitate the provision of soft budget constraints and thereby impair the finance-growth nexus.

1. Introduction

This paper analyzes the impact of state history on the finance-growth relationship in post-socialist countries. We focus on the conditional effect of the banking sector given that capital markets are underdeveloped and relatively unimportant in transition economies. We identify structural breaks in the growth impact of banking sector development along the state history index of the last two millennia constructed by Bockstette et al. (2002). This measure has gained considerable leverage in research on contemporary economic development and is commonly used as a proxy for long-run ancestral exposure to institutions, political organization and centralization. However, its possible effects on the finance-growth nexus have not yet been investigated. To our knowledge, we are the first to do this.

The motivation behind this area of focus for our study is the historical flux that has naturally characterized the financial systems of transition countries since the collapse of socialism. The financial sector of transition economies has been transformed from a one-tier state-controlled banking system into an overwhelmingly bank-based and intermediately developed market financial system in the last three decades. This unparalleled dynamic allows the exploration of the role of state history in the finance-growth relationship. The development of the financial sector was one of the first market reforms in East-Central Europe, Southeastern Europe and the former Soviet Union, but with very different long-run outcomes across the post-socialist space. Therefore, it is of first-order importance to unravel the origins of these long-run differences. Our paper introduces the finance-growth nexus into the discussion on the long-run economic effects of ancestral institutions and historical legacies.

The role of long-run factors in economic development has shifted the focus of the literature from contemporaneous levels of

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institutional development toward deeply rooted cultural, historical and biological factors (Spolaore and Wacziarg, 2013). One of the most commonly used factors in the analysis of deep roots of economic development (DRD henceforth) is the state history index of Bockstette et al. (2002). The authors construct an indicator that provides a composite measure of population exposure to centralized statehood and organized institutional and societal frameworks on the territory of a given country in the last two millennia. They suggest that state history as a proxy for state capacity, ethnic homogeneity and dense population has a positive effect on economic development. In their study on transition economies, Iliev and Putterman (2007) observe that within the Eurasian socialist group of states, societies with a longer state history tend to perform better than others. This is the case both under socialism and in transition. In contrast to previous studies, Putterman and Weil (2010) adjust state history according to the ancestral affiliation of the present population based on their 1500 CE origins. They argue that countries with populations originating from states with a longer history of agricultural and political development tend to be wealthier than others. While state history is generally considered to be conducive to development, Borcan et al. (2018) show that excessive state experience can be detrimental to economic performance due to the emergence of deeply entrenched extractive institutions. This indicates a concave rather than a linear relationship between economic development and state history (ibid.).

In this paper, we offer comprehensive results and a discussion on the effect of state history on the finance-growth nexus in post-socialist countries. We show that state history seems to adversely affect the impact of financial development on growth in post-socialist countries. We argue that state history as a proxy for ancestral exposure to centralization, political organization and institutions facilitates the provision of soft budget constraints because of moral hazard and rent-seeking institutions. Ang (2013) shows that state history has a positive effect on financial development around the globe and this could have significant implications for the growth paths of contemporary states. He proposes that state history is associated with more efficient financial transactions, bureaucracies and taxation (ibid.). In contrast, we concentrate on transition economies and propose that, in this case, state history adversely affects the growth effect of financial development. This pivotal distinction between Ang's paper (2013) and ours is due to the following reasons. First, the institutional legacies of central planning, such as soft budget constraints and bureaucratic rent-seeking under conditions of imperfect monitoring, also perpetuate in the post-transition period. Second, Ang (2013) is inconclusive about the finance-growth nexus, as his results underscore the significant and positive effect of state history on financial development *per se*.

It is important to stress that our contribution to the current body of literature is two-fold. First, we expand the literature on the deep roots of economic development by offering a *finance-growth perspective*. Although our empirical results confine themselves to the post-socialist bloc, the proposed underlying mechanisms might reasonably apply to other countries as well. Second, we enrich the finance-growth literature along two dimensions.[1] For one thing, we provide a study that elaborates on transition economies and their institutions. Furthermore, we offer a new channel of nonlinearity with respect to the effect of financial development on economic growth. Broadly speaking, three potential channels have already been identified. Rioja and Valev (2004) and Huang and Lin (2009) argue that the effect of finance on growth depends on the stage of economic development. A second line of the literature emphasizes the nonlinearity according to the size of the financial sector (e.g. Law and Singh, 2014; Arcand et al., 2015). Finally, the conditioning effect of institutions and policies on the finance-growth nexus is emphasized in, among others, Rousseau and Yilmazkuday (2009), Law et al. (2013) and Herwartz and Walle (2014). Beyond these (development/finance/institutions and policy) perspectives, our paper establishes a fourth perspective in nonlinear finance by using state history as a threshold variable: the *legacy perspective*.[2]

The key transition debates on the optimal sequencing of economic reforms and the welfare differences between shock therapy and gradualism have missed the important role of financial development and its linkages to ancestral institutions. In our paper, we argue that the relative successes and failures in economic transformation and stabilization and the subsequent catching-up of East-Central Europe and the former Soviet Union have also depended on the ways that state history has shaped the finance-growth channel.

The paper is structured as follows. Section 2 provides an overview of the literature on finance and growth in transition economies. Section 3 presents the data and empirical strategy of the paper. In Section 4, we present the results. Section 5 performs relevant robustness checks and Section 6 offers an informed discussion of our results. Section 7 concludes.

2. Finance and growth in transition economies

Koivu (2002) was among the first to research the finance-growth nexus in transition. Her results show that the bank lending-deposit interest rate spread negatively affects growth, while the amount of private credit does not exert any significant effect. Dawson (2003) reveals that liquid liabilities were not a relevant growth determinant during the nineties in 13 transition countries. Fink et al. (2009) find that in post-socialist countries the amount of private credit and stock market capitalization are neutral for economic growth. Djalilov and Piesse (2011) conclude that credit to the private sector is not relevant for growth, while the interest rate spread affects growth negatively in 27 transition countries. Caporale et al. (2015) find that the average of the EBRD financial transition indicators, as a composite measure for financial development, exerts a large positive effect on growth. They also present some evidence on the positive effect of the size of the financial sector on growth. Cojocaru et al. (2016) consider alternative efficiency measures of the financial sector and arrive at the conclusion that efficiency is superior to size when it comes to the growth effect of

[1] For an overview of the general finance-growth literature see, e.g., Levine (2005).

[2] The legacy perspective has already had a few forerunners in the finance literature.. La Porta et al. (1998) provide invaluable insights into the decisive role of the legal origin for financial development. Grosjean (2011) presents evidence on the historical legacy of the Ottoman Empire on financial development in Southeastern Europe.

financial development, at least in transition economies.

These papers underscore the importance of financial development for economic growth in post-socialism: efficiency measures of the financial sector (e.g. interest rate spread) are important for growth, while size measures (e.g. private credit) are less so. The literature provides three, one general and two transition-related, explanations for this robust finding. According to the general reasoning, an increase in the size of the financial sector can have different growth effects in the short and long run because of the possible financial turmoil that an abrupt rise in private credit might trigger in the short run (Loayza and Rancière, 2006). Indeed, the results of Gaffeo and Garalova (2014) bolster the legitimacy of this timeframe argument in the case of transition economies. Regarding the transition-specific reasons, Koivu and Sutela (2005) emphasize two possible explanations. First, targeted loans by state-owned banks, state subsidies and soft budget constraints continued to prevail in some transition economies even several years after the collapse of the Soviet bloc. Under such circumstances, the increase in the amount of credit devoted to the private sector does not automatically entail a proliferation of efficient private investments. Second, banking crises were natural concomitants of economic transition during the nineties, which introduced temporary negative shocks into the finance-growth relationship.

The literature on transition economies almost completely ignores possible nonlinearities in the finance-growth nexus. Coricelli and Masten (2004) is an exception. These authors introduce the interaction of private credit growth with the average of EBRD financial transition indicators into their growth regression. According to their results, the growth of private credit exerts a negative direct effect and a positive interaction effect on economic growth. Consequently, the overall growth impact of a credit expansion depends on the institutional quality of the financial sector.

To sum up, the transition literature has revealed considerable growth effects in relation to the efficiency of the financial sector, but has failed to prove the importance of the size of the financial sector in the growth process. Beyond the previously discussed considerations, another explanation for the missing *growth-financial sector size* link may be the ignorance of possible nonlinearities in the finance-growth relationship when it comes to transition economies.

3. Data and empirical strategy

This section introduces the data and the empirical strategy used to explore the effect of state history on the finance-growth relationship in post-socialist economies. Table 1 summarizes the basic information on our dataset with regard to the notations, units of measurement and data sources. We use two financial development measures: private credit and domestic credit. Private credit is the amount of financial resources provided to the private sector by deposit money banks. By contrast, domestic credit is the amount of financial resources provided to the private sector by deposit money banks, other financial corporations and monetary authorities. In most economies, the two series evolve very similarly to each other and are highly correlated, which actually turns out be the case for the countries in our sample too (see Fig. 1 and Table 3).[3]

In our analysis, we utilize three different measures of state history. In the baseline case, we use the SH1950 state history index of the last two millennia constructed by Bockstette et al. (2002) as a measure of accumulated experience with political centralization and ancestral institutions. This index provides a composite value on the existence, independence and territorial coverage of statehood within the present borders of a country between 1 and 1950 CE.[4,5]

In the DRD literature, it is common to explain contemporary economic development as a result of pre-Columbian institutions and development. In the related state history literature, it is also prevalent to consider the accumulated state history up until 1500 CE rather than up to the present. While we are convinced that the post-Columbian era matters for the finance-growth nexus, we also run our estimations with SH1500.[6] As a second alternative measure of state history, we use the ancestry-adjusted state history of the last two millennia (SH1950adj) as per Putterman and Weil (2010). This state history measure is calculated as the weighted average of the (1–1950 CE) state history of the year-1500CE origin countries of the current population. The weights are determined according to the ancestral composition of the present population, i.e. the proportions of year-1500CE origin countries in the present population, compiled in the World Migration Matrix of Putterman and Weil (2010).[7] The adjustment is according to ancestry controls for the post-

[3] Note that we focus only on size-based measures of financial development. The reason is that efficiency-based measures, such as the interest rate spread, are assumed to be less reliable in some post-socialist countries due to government-controlled banking sectors, especially in the 1990s.

[4] After the conclusion of this paper, Borcan et al. (2018) provided an update of this state history index for the last six millennia (3500 BCE - 2000 CE). However, since in most post-socialist transition countries centralized statehood was absent or existed only for a short period of time before the common era, the country ranking according to the extended state history index is very similar to that in this paper. Therefore, using the extended index would not alter the main conclusions of the paper.

[5] The index is calculated as follows: $SH1950_i = (\sum_{t=0}^{38} (1+\delta)^{t-38} \cdot s_{it}) / (\sum_{t=0}^{38} (1+\delta)^{t-38} \cdot 50) \in (0; 1)$, where $s \in (0; 50)$ is the state history score of the t half-century (e.g., 1901-50 when t = 38) and δ is the depreciation rate. The state history score is a composite measure of the type and territorial extent of statehood in the given 50-year period assessed by Bockstette et al. (2002) for each country and half-century based on historical records. Concisely speaking, the more independent and centralized the government was and the larger the territory of the present country this government ruled in the underlying half-century, the higher the respective state history score is calibrated. The depreciation rate represents the extent to which past experiences with organized statehood are assumed to lose impact on current socio-economic outcomes. Our preferred depreciation rate is 5 percent, similar to Bockstette et al. (2002).

[6] $SH1500_i = (\sum_{t=0}^{29} (1+\delta)^{t-29} \cdot s_{it}) / (\sum_{t=0}^{29} (1+\delta)^{t-29} \cdot 50)$, where t = 29 refers to 1451-1500 CE and t = 0 refers to 1-50 CE.

[7] More precisely, $SH1950adj_{\mathrm{i}} = X_{\mathrm{I}} \cdot \overline{SH1950}$, where $SH\bar{1}950$ is the vector of the SH1950 indexes and X is the migration matrix and i is the row index.. The rows in the migration matrix contain the proportions of year-1500 CE origin countries of countries' present population. For further details, see Putterman and Weil (2010). Table A1 reports SH1950, SH1950adj and SH1500 country values.

Table 1
Data description and sources.

Variable	Notation	Unit	Data source	Notes
GDP per worker	y	USD (PPP, 2011 prices)	PWT9.0	Own calculation based on the series of *rgdpna*[a] and *emp*[b]
Enrollment ratio in tertiary education	TER	%	UIS	Gross ratio, both sexes included
Gross fixed capital formation	GFCF	% of GDP	WDI	
Inflation	Inflation	%	WEO	Change of consumer prices (ann. avr.)
Natural resource rents	NR_rents	% of GDP	WDI	Sum of *Oil rents* and *Natural gas rents*
Global crisis dummy	GlobalCrisis	1 for 2009–2011, 0 otherwise		
Average Worldwide Governance Indicators	WGI	index [-2.5 ; 2.5][d]	Worldwide Governance Indicators (World Bank)	Arithmetic average of the 6 WGIs
State history (1–1950CE)	SH1950	index [0 ; 1][d]	State Antiquity Index (v.3.1) (‘*statehistn05v3*’)[c]	Accumulated state history of the period 1 and 1950 CE (depreciation rate: 5 %)
State history (1–1500CE)	SH1500	index [0 ; 1][d]	Own calculation based on State Antiquity Index (v.3.1)	Accumulated state history of the period 1 and 1500 CE (depreciation rate: 5 %)
Ancestry-adjusted SH1950	SH1950adj	index [0 ; 1][d]	*X*: World Migration matrix, 1500-2000 (v.1.1)[c]	Own calculation: $X_i \cdot SH\bar{1}950$, where $SH\bar{1}950$ is the vector of the SH1950 indexes and *i* is the row index.
Private credit by deposit money banks	PCB	% of GDP	GFDD (code: GFDD.DI.01)	
Domestic credit	DC	% of GDP	GFDD (code: GFDD.DI.14)	Private credit by deposit money banks, other financial corporations and monetary authorities

Abbreviations: PWT9.0 – Penn World Table 9.0 (Feenstra et al., 2015), UIS – UNESCO, Institute for Statistic, WDI – World Development Indicators (The World Bank), WEO – World Economic Outlook (April 2016, IMF), GFDD – Global Financial Development Database (June 2016, The World Bank).

[a] Real GDP at PPP using national accounts growth rates.

[b] Number of people employed.

[c] For the *Migration Matrix* and the *State Antiquity Index*, see Louis Putterman’s website (http://www.brown.edu/Departments/Economics/Faculty/Louis_Putterman/).

[d] Government quality/State history increases with the index.

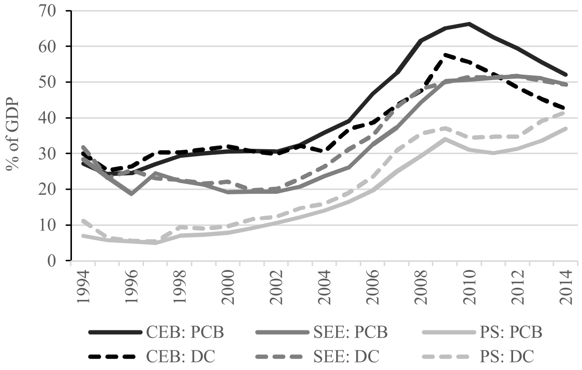

Fig. 1. Private credit and domestic credit (regional averages).
Notes: CEB: Central Europe & the Baltics, SEE: Southeastern Europe, PS: post-Soviet countries (excl. the Baltics) and Mongolia.
Source: Own graph. For the data source, see Table 1.

1500CE population flows and the imputed heterogeneity with respect to the historical exposure to organized statehood.[8]

Our panel data covers the period between 1993 and 2014 and includes 26 transition countries. These countries are Albania, Armenia, Azerbaijan, Belarus, Bosnia and Hercegovina, Bulgaria, Croatia, the Czech Republic, Estonia, North Macedonia, Georgia, Hungary, Kazakhstan, the Kyrgyz Republic, Latvia, Lithuania, Moldova, Mongolia, Poland, Romania, Russia, Serbia, Slovakia, Slovenia, Tajikistan and Ukraine. The descriptive statistics, tailored to the major regions of transition economies, and the correlation matrix of the annual observations are depicted in Tables 2 and 3. As can be observed in Table 3, state history and the development of the banking sector are positively related to each other, matching the result of Ang (2013).

In the case of both private credit and domestic credit, financial development is at the highest level in Central Europe and the Baltics, followed by Southeastern Europe in the second place, whereas the remaining post-Soviet economies and Mongolia lag behind considerably (Table 2). This relative ranking of transition regions characterizes the whole period under consideration (Fig. 1). Regional patterns are also observable with respect to the state history of the last two millennia (Fig. 2): SH1950 tends to be higher in the western than in the eastern part of the post-socialist bloc, with the notable exception of the young Baltic States. In other words, people are more accustomed to an organized state framework in Central and Southeastern Europe than in most parts of the post-Soviet space. This observation also holds for pre-Columbian state history (Fig. 3). Moreover, given that post-1500CE population flows have been moderate in transition economies, ancestry-adjustment implies only limited changes in terms of state history (see Table A1 in the Appendix).

The annual data are noisy because of business cycles. To address this problem, the growth literature usually operates with 5-year periods. However, the time series on transition countries are naturally constrained to the last two and a half decades. In order to maintain a reasonably long time dimension for our panel sample, we work with 3-year periods (1994-96, 1997-99, 2000-02, 2003-05, 2006-08, 2009-11, 2012-14), which permits t = 7 time observations. Thus, our 3-year panel sample consists of 182 data points. Most of the variables in our regressions are period averages, meaning that at time *t* they take the average value of the underlying period. The only exceptions are, on the one hand, the time-invariant state history variable and, on the other hand, the (log) GDP per worker, with the GDP per worker at time *t* referring to the end year of the underlying period.

3.1. Empirical strategy

We estimate typical growth-regressions extended with a financial development measure and the interaction of the latter with a quantile dummy that controls for the potential regime effect of the underlying state history variable. Our objectives are twofold. First, we intend to find evidence of the presence of a structural break in the finance-growth nexus along the accumulated experience with established statehood. Second, we intend to reveal the differential nature of the effect of financial development on growth in the imputed lower and upper regimes of the distribution of the state history variable under consideration. Hence, we build on the following baseline model specification:

$$\ln(y_{it}) = \alpha_i + \beta_1 FD_{it} + \beta_2 SH_i + \beta_3 (FD_{it} \cdot SH(\#)_i) + \bar{\gamma}^T [conditioningset_{it}] + \varepsilon_{it}, \qquad (1)$$

where the idiosyncratic disturbances are supposed to be non-correlated within and across units but their variance is allowed to be country-specific: $\varepsilon_{it} \sim N(0, \sigma_i^2)$, $Cov(\varepsilon_{it}, \varepsilon_{il}) = 0 \; \forall \; t \neq l$ and $Cov(\varepsilon_{it}, \varepsilon_{jl}) = 0 \; \forall \; i \neq j$. In Eq. (1), i indexes the countries, t indexes the time, *ln(y)* is the log GPD per worker, α_i is the fixed country effect, *FD* denotes financial development, *SH* is the state history measure

[8] The rationale for the latter is that the experience of the current population and its ancestors with a centralized political and social framework may matter more for contemporary socio-economic outcomes than the respective experience of the country's current territory. Indeed, the DRD literature has postulated from the very beginning that cultural traits transmitted across generations constitute the main link between past and present. In this sense, we follow the standard practice of the literature by also considering ancestry-adjusted state history as an alternative measure in our estimations.

Table 2
Descriptive statistics tailored to the major country groups (annual data: 1994–2014).

	Central Europe & the Baltics[a]					Southeastern Europe[b]					post-Soviet countries (excl. the Baltics) & Mongolia[c]				
	Obs	Mean	SD	Min	Max	Obs	Mean	SD	Min	Max	Obs	Mean	SD	Min	Max
y	168	42654	10622	17762	62802	147	32780	12289	9548	57569	231	19193	11341	4665	48592
TER	166	54.1	19.7	17.0	88.5	131	38.0	15.3	10.2	71.3	202	43.2	19.1	12.8	91.0
GFCF	164	24.8	4.7	14.3	37.1	144	22.4	6.4	5.4	40.5	230	23.2	7.7	2.6	57.7
Inflation	164	6.6	7.5	−1.2	47.7	140	21.0	92.6	−1.6	1061	230	84.1	411.1	−8.5	5273
WGI	128	0.778	0.181	0.245	1.214	112	−0.143	0.348	−1.222	0.447	176	−0.618	0.351	−1.659	0.391
NR_rents	158	0.17	0.24	0.00	1.12	141	1.2	1.5	0.0	6.8	225	9.6	15.8	0.0	68.4
PCB	166	41.8	20.5	6.7	102.5	138	33.2	18.6	2.8	73.3	225	18.4	14.9	0.9	73.9
DC	167	38.1	23.8	1.1	136.0	139	34.9	18.6	3.2	70.3	225	21.5	17.5	1.2	90.6
SH1950	168	0.470	0.117	0.290	0.601	147	0.572	0.069	0.462	0.652	231	0.447	0.078	0.295	0.553
SH1950adj	168	0.487	0.096	0.338	0.597	147	0.576	0.062	0.467	0.652	231	0.463	0.070	0.336	0.581
SH1500	168	0.389	0.187	0.133	0.632	147	0.558	0.086	0.397	0.650	231	0.355	0.165	0.119	0.623

Notes: SD is standard deviation.
[a] Central Europe & the Baltics (CZE, EST, HUN, LTU, LVA, POL, SVK, SVN).
[b] Southeastern Europe (ALB, BGR, BIH, HRV, MKD, ROM, SRB).
[c] Post-Soviet countries & Mongolia (ARM, AZE, BLR, GEO, KAZ, KGZ, MDA, MNG, RUS, TJK, UKR).

Table 3
Correlation matrix (all countries, annual data: 1994–2014).

	y	TER	GFCF	Inflation.	WGI	NR_rents	PCB	DC	SH1950	SH1950adj
TER	0.548*	1.00								
GFCF	0.119*	0.170*	1.000							
Inflation	−0.167*	−0.11	−0.032	1.000						
WGI	0.695*	0.382*	0.134*	−0.265*	1.000					
NR_rents	−0.046	−0.067	0.072	0.015	−0.367*	1.000				
PCB	0.729*	0.566*	0.150*	−0.132*	0.599*	−0.199*	1.000			
DC	0.580*	0.498*	0.114*	−0.109	0.367*	−0.154*	0.824*	1.000		
SH1950	0.192*	−0.169*	−0.119*	−0.011	0.085	−0.151*	0.105	0.175*	1.000	
SH1950adj	0.194*	−0.183*	−0.121*	−0.019	0.115	−0.170*	0.124*	0.189*	0.993*	1.000
SH1500	0.074	−0.275*	−0.064	0.019	0.048	−0.259*	0.071	0.129*	0.885*	0.883*

* Significant at the 1 percent level.

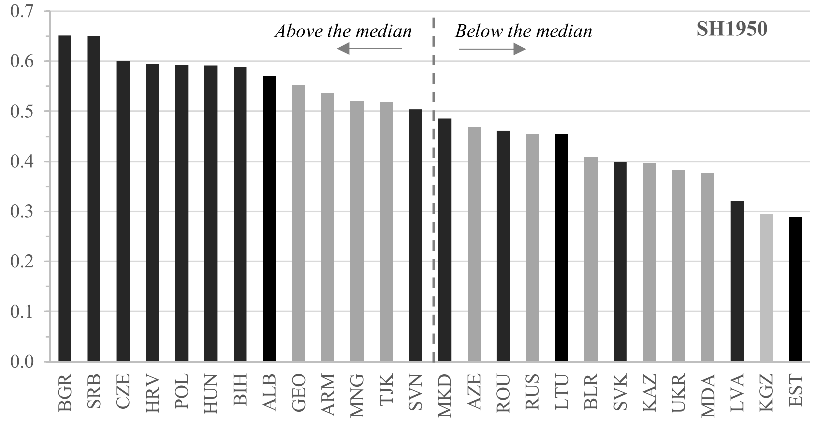

Fig. 2. State history (1–1950 CE) in transition economies.
Notes: Black columns: Central European, Baltic and Southeastern European countries.
Grey columns: post-Soviet countries (excl. the Baltics) and Mongolia.
Source: Own graph. For the data source, see Table 1.

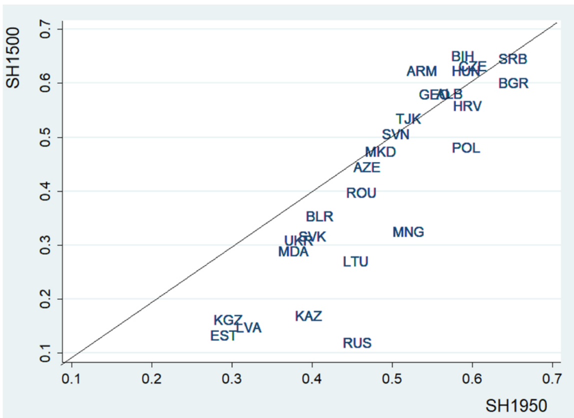

Fig. 3. State history (1–1950 CE) versus state history (1–1500 CE).
Notes: Own graph. For the data source, see Table 1.

and *SH(#)* is a dummy that indicates the position of a country in the distribution of transition countries along the given state history variable.[9] The value of *SH(#)* is set to 1 if the country falls into the upper (100 – #) percent of the distribution, and to zero otherwise.

[9] Note that Eq. (1) can indeed be considered as a growth regression since the lagged GDP per worker is included in the conditioning set (see below). Using the average growth rate as a dependent variable delivers the same result with the exception of the coefficient of lagged GDP per worker, which becomes (1 + original coeff.) (Durlauf et al., 2005).

For example, in the case of $\# = 30$, *SH(30)* is 1 if the country disposes of a quantile value related to the state history variable that is larger than 30 percent. If this is not the case, that is, if the value of the state history measure falls into the lower 30 percent of the underlying distribution, then $SH(30) = 0$. Note that by virtue of its construction, *SH(#)* is just a quantile dummy.

We consider two size-based measures of financial development in our estimations, the amount of credit granted by deposit money banks to the private sector (private credit) and the total domestic credit to the private sector (domestic credit). With respect to state history, three different measures are considered. The baseline measure is the accumulated state history of the last two millennia, SH1950. Furthermore, we also consider the state history of the pre-Columbian era, SH1500, and the ancestry-adjusted state history of the last two millennia, SH1950adj.

The covariates (conditioning) set consists of the following usual policy, structural and production factor measures: the first-order lag of the dependent variable, the (logged) gross enrollment ratio in tertiary education, the (logged) gross fixed capital formation, the inflation rate, the natural resource rents, the average Worldwide Governance Indicators and the *GlobalCrisis* dummy to control for the external shock caused by the global economic crisis. Other policy and structural variables were also considered, but they proved to be insignificant (see robustness checks).

The introduction of the $FD{\cdot}SH(\#)$ interaction term into the model splits the sample into two parts in relation to the effect of financial development on economic growth. In the lower # percent of the sample distribution of the state history variable (lower regime), where $SH(\#) = 0$, the marginal effect of financial development on (end of period) GDP is β_1, which is simply the direct effect of *FD* in Eq. (1). On the other hand, in the upper (100 – #) percent of the sample distribution (upper regime), where $SH(\#) = 1$, the marginal effect of financial development on economic output is $(\beta_1 + \beta_3)$, i.e. the sum of *FD*'s direct effect and interaction effect. The estimations are performed for five threshold values, $\# = 30$, 40, 50, 60 and 70; i.e., we split the sample in terms of the effect of financial development on growth at the 30th, 40th, 50th, 60th and 70th percentile of the *SH* variable.[10] The default scenario is $\# = 50$.

In order to explore the effect of state history on the finance-growth nexus, we concentrate on two results: the significance of the $FD{\cdot}SH(\#)$ interaction term and the significance of the overall effect of financial development in the upper and bottom regimes. A significant (nonsignificant) $FD{\cdot}SH(\#)$ coefficient conveys a significant (nonsignificant) break in the finance-growth nexus between the two regimes provided that the effect of financial development is significant in either (neither or both) regimes (Kingsley, 2017).[11]

Eq. (1) postulates a discontinuous break in the finance-growth relationship according to which the sample countries constitute two regimes with potentially different growth effects of financial development. To allow for continuous conditionality, we also run our estimations by replacing the SH quantile dummy with the respective SH index in the interaction term of financial development:

$$\ln(y_{it}) = \alpha_i + \beta_1 FD_{it} + \beta_2 SH_i + \beta_3 (FD_{it}{\cdot}SH_i) + \bar{\gamma}^T [conditioningset_{it}] + \varepsilon_{it} \tag{2}$$

Eqs. (1) and (2) are estimated by system GMM, developed by Arellano and Bover (1995) and Blundell and Bond (1998).[12] The right-hand-side variables in Eqs. (1) and (2) are considered to be endogenous to contemporary shocks, with the exception of the lagged GDP per worker, the global crisis dummy, the state history variable and the natural resource rents.

The relatively low number of cross-sectional observations in our sample suggests applying GMM in the following way. First, we prefer first-step GMM over two-step GMM. Second, we constrain the number of GMM instruments by setting their maximum lag order to 2 at the (transformed) first-differenced equation.[13] Two-step GMM is asymptotically more efficient than one-step GMM. Nevertheless, the estimation of the optimal weighting matrix is a source of considerable uncertainty with regard to the finite sample superiority of the former over the latter. In fact, Monte Carlo studies tend to show that, on the one hand, two-step GMM does not outperform one-step GMM in terms of accuracy and precision, while, on the other hand, the supposed efficiency gain of going one step further is tiny in moderate samples and almost non-existent in such small samples as ours (Blundell and Bond, 1998; Windmeijer, 2005; Soto, 2009). The small sample size causes another serious problem with the system GMM, the proliferation of instruments (Roodman, 2009b). Instrument proliferation has many costs (e.g. the low power of the Hansen test of instrument validity), and so its mitigation is highly recommended (ibid.). To sum up, the containment of the number of instruments and the preference of one-step over two-step estimation are common in the (system) GMM literature in the case of small samples (e.g. Bond et al., 2001; Beck and Levine, 2004). Nevertheless, we check the robustness of the results with respect to the estimation methodology later.

Another peculiarity of our GMM estimations is that in addition to the standard system GMM instruments, we also involve external non-GMM instruments that are traditionally used to control for the exogenous evolution of financial development in the instrument matrix. These instrumental variables are related to the legacies of empires and the legal origin (see, e.g., La Porta et al., 2008; Grosjean, 2011). The imperial legacy instruments are composed of dummy variables indicating the imperial affiliation of post-socialist countries in 1900.[14] The legal origin instruments embrace a French legal origin dummy from La Porta et al. (2008) and a '*CIS*

[10] Our model belongs to the family of threshold models assuming nonlinearity in the form of regime-dependent effects (e.g. Hansen, 1999). The only difference is that, because of the low number of possible outcomes (26), we fix the threshold arbitrarily instead of resorting to a search algorithm.

[11] Kingsley et al. (2017) point out that in order to avoid the overstatement or understatement of the interaction effect, the significance of the conditioned marginal effect of FD must be also taken into account (see also Brambor et al., 2006). The key insight is that the standard error of the $(\hat{\beta}_1 + \hat{\beta}_3)$ combination depends not just on the standard errors of $\hat{\beta}_1$ and $\hat{\beta}_3$ but also on the covariance of these two coefficients.

[12] The system GMM estimations are conducted by the *xtabond2* command, developed by Roodman (2009a), in Stata.

[13] We exploit the following moment conditions: $E[\bar{x}_{i(t-2)}\Delta\varepsilon_{it}|t] = 0$ and $E[\Delta\bar{x}_{i(t-1)}\varepsilon_{it}|t] = 0 \quad \forall\ t \geq 3$, where $\bar{x}$ is the vector of explanatory variables with the exception of *GlobalCrisis*, *SH* and *NR_rents*.

[14] We use separate dummies for the Austro-Hungarian, German, Russian and Ottoman empires. The independent countries and the countries not affiliated with any of the previous four empires in 1900 constitute the control group. In those cases when the present territory of a country was

&MNG' dummy (1 for Mongolia and the Commonwealth of Independent States, 0 otherwise) to control for the potentially differential effect of Russian legal origin on financial development discussed in Harper and McNulty (2008).[15]

Finally, we would like to highlight the differences between the empirical strategy proposed by most of the standard DRD literature and that proposed in our paper. In the DRD literature, we usually observe cross-sectional long-run output regressions in which contemporary GDP per capita is explained by some geographic and deeply-rooted cultural and historical determinants (see, e.g., Spolaore and Wacziarg, 2013). By contrast, we use panel growth regressions with contemporary control variables, also including the regime-specific financial development, plus the time-invariant state history measure.[16] To put it differently, the DRD literature is primarily interested in how deeply rooted factors explain contemporary economic development, while we are interested in how (and why) deeply rooted factors, such as state history, have transformed the contemporary finance-growth nexus. As a result, the two approaches rely on completely different time frames: the focus of DRD literature inevitably captures the past centuries/millennia, while our focus is on short-run dynamics. In any case, we also perform the OLS estimation of Eq. (1) on the cross-section of countries in our sample by taking 20-year averages of the period 1995-2014. However, these results should be treated with caution due to the well-known methodological deficiencies of OLS.

4. Results

We present the estimation results of equations (1) and (2) in Table 4a and 4b. Table 4a contains the results on private credit, while Table 4b presents those on domestic credit. Each table consists of ten models. Models 1–8 operate with the preferred state history measure, SH1950. Model 1 is the linear case when financial development affects economic growth unconditionally. Models 2–6 are based on equation (1) with different threshold percentiles of state history, while model 7 corresponds to equation (2). Model 8 is the cross-sectional OLS estimation of equation (1) using the 20-year averages of the period 1995-2014.[17] Models 9 and 10 differ from model 2 only in terms of the underlying state history variable.

With both financial development measures, our baseline estimation is model 2, that is, when we estimate Eq. (1) on the 3-year panel sample using the state history of the last two millennia and splitting the sample in terms of the finance-growth nexus at the median value of SH1950. The subsequent models are the respective sensitivity checks of the baseline results. Models 3–6 demonstrate how the results depend on the cut-off point of the threshold variable. Model 7 checks the sensitivity to the discontinuity in the conditioning effect of state history with respect to the finance-growth nexus. Model 8 considers the robustness to the short-run macroeconomic dynamics under investigation in the baseline case. Models 9 and 10 present robustness to the preferred state history measure.

At first, we briefly summarize the general characteristics of these estimations. First, the coefficients of the covariates have the expected sign and are mostly significant. Second, the growth effect of state history is in line with the literature: longer state history induces higher economic growth. This result is established at a global scale in the seminal papers of Bockstette et al. (2002) and Putterman and Weil (2010). Third, in the linear model, the direct effect of outstanding credit, according to both measures, tends to be non-significant (or only marginally significant) for growth. This is in line with the results of the finance-growth literature on transition economies, according to which, in the banking sector, efficiency is much more important than size for economic growth. Finally, the tests on the first- and second-order residual autocorrelations of the (transformed) first-differenced equations (AR(1) and AR(2)) show that the model specification is correct and the GMM moment conditions are not violated. The Hansen test results in a 100 percent *p*-value in each case, which clearly indicates that the test is artificially deflated by the number of instruments still being too large and thus cannot be trusted. Nevertheless, the Sargan test and basic economic reasoning suggest the validity of our instrument set.[18,19] This suggestion is further corroborated by the Hansen test of those estimations that operate with very much constrained GMM instruments in the robustness checks (see Section 5).

Only the results on private credit (Table 4a) are presented in detail as the results on domestic credit (Table 4b) are very similar to them. Our baseline model (model 2A) shows that there is a significant difference between countries with a short state history and those whose state history is long when it comes to the growth impact of private credit. In the bottom 50 percent of the sample in terms

(*footnote continued*)
partitioned by more than one empire in 1900, the respective dummies are jointly set to 1. In other words, the dummies are not adjusted according to the proportion of territory ruled by the individual empires (see Table A1 in the Appendix).

[15] According to La Porta et al. (2008), post-socialist countries have either German or French legal origin. However, Harper and McNulty (2008) also identify a separate type of legal family, Russian legal origin, which applies mainly, but not exclusively, to the former territories of the Russian empire. They describe legal systems of Russian origin as being "…characterized by conflicts, contradictions, undefined or poorly defined principles, and major gaps." (ibid., pp.1267). In coding legal origin, we retain the original classification of post-socialist countries in La Porta et al. (2008), but supplement the former with the conditioning effect of Russian legal traditions by introducing the *CIS&MNG* dummy (see Table A1 in the Appendix).

[16] In this respect, our paper relates more to the conditional convergence literature (see, e.g., Durlauf et al., 2005).

[17] In this case, the *GlobalCrisis* dummy is dropped.

[18] Recall that only those variables that are hardly impacted by the idiosyncratic shocks to economic growth (*ln(y)(-1)*, *GlobalCrisis*, *SH* and *NR_rents*) are treated as being exogenous. Similarly, endogeneity is certainly not an issue for legal origin and imperial affiliation dummies.

[19] The Sargan test statistic is equivalent to the objective function of one-step GMM when disturbances are bound to be spherical, that is, non-correlated and homoskedastic (Sargan, 1958). If disturbances are non-spherical, the Sargan statistic is not capable of assessing instrument validity in a reliable way. Nevertheless, as it is not plagued by instrument proliferation, in contrast to the Hansen test, it is indicative in our case (Roodman, 2009a).

Table 4a
Estimation results for private credit.

Model *SH*	*(1A)* *SH1950*	*(2A)*	*(3A)*	*(4A)*	*(5A)*	*(6A)*	*(7A)*	*(8A)*	*(9A)* *SH1950adj*	*(10A)* *SH1500*
Sample (method)	*3-year panel (GMM)*							*Cross-sectional (OLS)*	*3-year panel (GMM)*	
Break (percentile)	*Linear*	*50th*	*30th*	*40th*	*60th*	*70th*	*Interaction*	*50th*	*50th*	*50th*
ln(y)(-1)	0.8902***	0.8721***	0.8936***	0.8783***	0.8926***	0.8958***	0.8976***	0.1192	0.8783***	0.8763***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.450)	(0.000)	(0.000)
ln(TER)	0.0341	0.0416	0.0370	0.0257	0.0374	0.0367	0.0386	0.4591**	0.0430	0.0405
	(0.180)	(0.126)	(0.176)	(0.357)	(0.167)	(0.160)	(0.138)	(0.021)	(0.115)	(0.156)
ln(GFCF)	0.0688*	0.0820**	0.0674*	0.0863***	0.0647*	0.0556	0.0665*	0.6243*	0.0780**	0.0680*
	(0.073)	(0.011)	(0.053)	(0.008)	(0.076)	(0.111)	(0.058)	(0.087)	(0.014)	(0.072)
Inflation	−0.0007**	−0.0006**	−0.0007**	−0.0007**	−0.0006**	−0.0007**	−0.0007**	−0.0033	−0.0006**	−0.0006**
	(0.019)	(0.021)	(0.021)	(0.018)	(0.022)	(0.022)	(0.021)	(0.419)	(0.020)	(0.025)
NR_rents	0.0032***	0.0032***	0.0034***	0.0032**	0.0031***	0.0031**	0.0031***	0.0130***	0.0032**	0.0042***
	(0.007)	(0.007)	(0.004)	(0.011)	(0.009)	(0.010)	(0.008)	(0.006)	(0.011)	(0.002)
GlobalCrisis	−0.0488***	−0.0482***	−0.0438***	−0.0467***	−0.0486***	−0.0509***	−0.0460***		−0.0478***	−0.0508***
	(0.002)	(0.003)	(0.006)	(0.004)	(0.002)	(0.002)	(0.005)		(0.003)	(0.006)
WGI	0.0593*	0.0711**	0.0626**	0.0651**	0.0607*	0.0623*	0.0549*	0.0703	0.0669**	0.0797**
	(0.060)	(0.040)	(0.042)	(0.041)	(0.065)	(0.066)	(0.067)	(0.726)	(0.047)	(0.012)
PCB	−0.0011*	0.0000	−0.0004	0.0003	−0.0008	−0.0008	0.0035	0.0197*	−0.0001	0.0003
	(0.094)	(0.975)	(0.642)	(0.702)	(0.244)	(0.216)	(0.138)	(0.064)	(0.861)	(0.689)
SH	0.2389	0.5489**	0.4609*	0.5856**	0.3614	0.3583	0.5981**	3.4734***	0.6041*	0.4081***
	(0.158)	(0.043)	(0.050)	(0.024)	(0.166)	(0.105)	(0.036)	(0.000)	(0.051)	(0.003)
PCB·SH(#)		−0.0021**	−0.0018*	−0.0025***	−0.0010	−0.0011		−0.0158***	−0.0020**	−0.0028***
		(0.030)	(0.060)	(0.007)	(0.320)	(0.172)		(0.000)	(0.035)	(0.001)
PCB·SH							−0.0104**			
							(0.027)			
-										
PCB_upper		−0.0021***	−0.0021***	−0.0022***	−0.0018**	−0.0020***		0.0039	−0.0022***	−0.002***
		(0.002)	(0.005)	(0.001)	(0.026)	(0.006)		(0.694)	(0.001)	(0.000)
Sargan test (pv)	0.284	0.647	0.647	0.779	0.526	0.574	0.781		0.690	0.792
Hansen test (pv)	1.000	1.000	1.000	1.000	1.000	1.000	1.000		1.000	1.000
AR(1) (pv)	0.100	0.110	0.108	0.111	0.103	0.097	0.110		0.109	0.106
AR(2) (pv)	0.267	0.302	0.289	0.368	0.268	0.245	0.308		0.296	0.304
no. of instruments	74	84	84	84	84	84	84		84	84
n	174	174	174	174	174	174	174	26	174	174
R2								0.840		

Notes: Dependent variable: ln(y). Model 1A is the linear version of Eq. (1) omitting the PCB·SH(#) interaction term. Models 1A–7A and models 9A–10A are estimated by one-step system GMM on the 3-year panel sample. Model 8A is estimated by OLS on the cross-sectional sample of the 20-year averages of the period 1995-2014. In case of the GMM estimations the maximum lag order of GMM level instruments is set to 2 and legal origin and imperial legacy dummies are included as standard instruments. The constant term is not presented. *P*-values calculated according to robust standard errors are in parentheses. Robust standard errors: *1-step system GMM* (heteroskedasticity and autocorrelation robust s.e.), *OLS* (White s.e.). Asterisks denote the significance level (* 10 %, ** 5 %, *** 1 %). AR(1) and AR(2) are the Arellano-Bond tests for first-order residual autocorrelation and second-order residual autocorrelation in the first-difference equation, respectively (Arellano and Bond, 1991). In the model statistics, only the *p*-values (pv) are presented. The number of complete observations in untransformed data is denoted by *n.PCB_upper* is the computed marginal effect of private credit on growth in the upper $(100 - \#)$ percent of the sample distribution of the respective state history variable. It is the sum of the coefficients of *PCB* and *PCB·SH(#)* $(\hat{\beta}_1 + \hat{\beta}_3)$ with a standard error of $SE(\hat{\beta}_1 + \hat{\beta}_3) = \sqrt{Var(\hat{\beta}_1) + Var(\hat{\beta}_3) + 2Cov(\hat{\beta}_1, \hat{\beta}_3)}$.

Table 4b
Estimation results for domestic credit.

Model	***(1B)***	***(2B)***	***(3B)***	***(4B)***	***(5B)***	***(6B)***	***(7B)***	***(8B)***	***(9B)***	***(10B)***
SH	*SH1950*								*SH1950adj*	*SH1500*
Sample (method)	*3-year panel (GMM)*							*Cross-sectional (OLS)*	*3-year panel (GMM)*	
Break (percentile)	*Linear*	*50th*	*30th*	*40th*	*60th*	*70th*	*Interaction*	*50th*	*50th*	*50th*
ln(y)(-1)	0.8841***	0.8692***	0.8894***	0.8798***	0.8867***	0.8891***	0.8957***	0.1957	0.8735***	0.8786***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.196)	(0.000)	(0.000)
ln(TER)	0.0351	0.0456*	0.0375	0.0306	0.0386	0.0367	0.0400	0.3828**	0.0468*	0.0415
	(0.184)	(0.095)	(0.164)	(0.283)	(0.169)	(0.175)	(0.110)	(0.031)	(0.090)	(0.154)
ln(GFCF)	0.0783**	0.0928***	0.0830**	0.0952***	0.0773*	0.0693*	0.0816**	0.5440	0.0904***	0.0705*
	(0.043)	(0.006)	(0.018)	(0.004)	(0.050)	(0.065)	(0.023)	(0.135)	(0.008)	(0.076)
Inflation	−0.0006**	−0.0006**	−0.0006**	−0.0006**	−0.0006**	−0.0006**	−0.0006**	−0.0029	−0.0006**	−0.0006**
	(0.017)	(0.019)	(0.018)	(0.017)	(0.020)	(0.021)	(0.020)	(0.455)	(0.019)	(0.022)
NR_rents	0.0033***	0.0032***	0.0035***	0.0032**	0.0032***	0.0033***	0.0031***	0.0123***	0.0032***	0.0040***
	(0.005)	(0.006)	(0.002)	(0.013)	(0.005)	(0.006)	(0.007)	(0.006)	(0.008)	(0.001)
GlobalCrisis	−0.0552***	−0.0498***	−0.0493***	−0.0477***	−0.0533***	−0.0562***	−0.0489***		−0.0504***	−0.0521***
	(0.000)	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)	(0.003)		(0.002)	(0.002)
WGI	0.0517*	0.0706*	0.0587*	0.0637*	0.0556	0.0589	0.0528*	0.2310	0.0658*	0.0757**
	(0.098)	(0.052)	(0.054)	(0.051)	(0.109)	(0.112)	(0.099)	(0.203)	(0.063)	(0.019)
DC	−0.0007	0.0002	0.0000	0.0003	−0.0006	−0.0005	0.0046***	0.0087	0.0001	0.0004
	(0.222)	(0.789)	(0.971)	(0.547)	(0.343)	(0.441)	(0.009)	(0.286)	(0.888)	(0.539)
SH	0.2620	0.5754**	0.4928**	0.6011***	0.3851	0.3940*	0.6601***	2.6854***	0.6460**	0.4080***
	(0.116)	(0.016)	(0.021)	(0.008)	(0.112)	(0.062)	(0.009)	(0.007)	(0.020)	(0.002)
DC·SH(#)		−0.0022**	−0.0019**	−0.0026***	−0.0010	−0.0012		−0.0112**	−0.0022**	−0.0027***
		(0.011)	(0.036)	(0.002)	(0.306)	(0.148)		(0.043)	(0.013)	(0.001)
DC·SH							−0.0123***			
							(0.002)			
-										
DC_upper		−0.0021***	−0.0019**	−0.0022***	−0.0015	−0.0017**		−0.0025	−0.0021***	−0.0024***
		(0.005)	(0.016)	(0.002)	(0.114)	(0.040)		(0.782)	(0.006)	(0.000)
Sargan test (pv)	0.256	0.600	0.580	0.765	0.485	0.516	0.744		0.643	0.721
Hansen test (pv)	1.000	1.000	1.000	1.000	1.000	1.000	1.000		1.000	1.000
AR(1) (pv)	0.097	0.114	0.108	0.115	0.104	0.099	0.117		0.112	0.114
AR(2) (pv)	0.271	0.347	0.342	0.430	0.287	0.252	0.356		0.341	0.345
no. of instruments	74	84	84	84	84	84	84		84	84
n	174	174	174	174	174	174	174	26	174	174
R2								0.798		

Notes: See Table 4a.

of SH1950, private credit is neutral for economic growth. However, in the upper 50 percent of countries, private credit has a significant negative effect on economic growth with an estimated coefficient of -0.0021. The significantly negative $PCB \cdot SH1950(50)$ interaction term also signals this break in the finance-growth nexus. The adverse effect of financial development under an excessively long state history seems to be important: a 10 percentage point increase in the credit-to-GDP ratio induces, ceteris paribus, a 2.1 percent decrease in GDP per worker by the end of the underlying 3-year period, implying a 0.7 percentage point decrease in the respective average annual growth rate. This result particularly captures East-Central and Southeastern European economies, which exhibit levels of accumulated state history considerably above the median (Table A1).

When the sample is split at the 30th and 40th percentiles of state history with respect to the growth effect of financial development (models 3A–4A), the results are very similar to the baseline case in terms of both the sign and the significance of the private credit-related coefficients (i.e. PCB, $PCB \cdot SH1950(\#)$, PCB_upper). When the threshold is set at the 60$^{\text{th}}$ and 70$^{\text{th}}$ percentiles (models 5A–6A), the interaction term loses significance while keeping its negative sign. Nevertheless, models 5A and 6A deliver the same conclusions with respect to the conditioning effect of state history since the coefficient of private credit is significantly negative in the upper regime and statistically zero in the bottom regime.[20,21]

Model 7A demonstrates that the switch from discontinuous to continuous conditionality does not change the baseline results: the direct effect of private credit remains non-significant while the interaction term keeps its significance with a negative sign. As Fig. 4 also indicates, state history induces a negative conditionality in the effect of private credit on economic growth, which becomes significant only above a certain threshold of state history. In model 8A, with a focus on long-term dynamics, the direct effect of private credit is significantly positive, while the interaction term is significantly negative, resulting in a non-significant PCB coefficient in the upper regime of transition economies. Consequently, the negative effect of state history on the finance-growth nexus remains unchanged. Finally, both ancestry-adjusted and pre-Columbian state history (models 9A–10A) produce results similar to the baseline ones. Hence, state history also affects the finance-growth nexus negatively in post-socialist countries when we account for pre-Columbian ancestry or ignore post-Columbian state history. The results with respect to domestic credit lead to similar conclusions.

5. Further robustness checks

This section conducts further robustness checks on our results. In advance, the main conclusion of the section is that the results are robust to all robustness checks. The summary tables of the estimations are presented in the Appendix. For each regression only those coefficients that stand at the center of our interest in the given sensitivity scenario are depicted in the tables.[22] The first robustness check considers the depreciation rate used for the computation of state history. Although the baseline rate of 5 percent is commonly used in the literature, it is unambiguously an arbitrary choice. In Table A2, we present the estimations of models 2A and 2B, when the depreciation rate is set to either 0 or 1 percent. The results are unchanged.

Second, we explore robustness with respect to the estimation method. Our baseline method can be criticized on the basis of three points. First, GMM is a large sample technique. Second, two-step GMM is usually preferred to one-step GMM. Third, although the number of instruments is already constrained in our estimations, it is still high. These concerns are valid, and we address them by estimating Eq. (1) using four alternative methods. First, we run pooled OLS estimation. The criticisms related to the small size of our sample would be alleviated if the OLS results were to convey the same conclusions on the presence and the nature of nonlinearity in the finance-growth nexus. Second, we run one-step system GMM estimation in accordance with the two alternative approaches to instrument containment suggested in Roodman (2009b). The first method collapses the instrument matrix, meaning all available lags of GMM level instruments are retained but they are constrained to having the same coefficients in each period when projecting the regressors onto them. The second method merges the previous two approaches of instrument containment by collapsing the instruments and, at the same time, maximizing the lag order of GMM level instruments at 2.[23] As a final check of robustness to the estimation methodology, we run a two-step system GMM estimation with the most parsimonious set of GMM instruments (collapsed instrument matrix and lag order of GMM level instruments maximized at 2). As can be observed in Table A3, the alternative estimation methods overwhelmingly support the baseline results. Clearly, there is no evidence that any of them could systematically undermine the significance of state history as a threshold variable. An interesting by-result is that the Hansen test continues to have large p-values even when the weakness of the test is already less of a concern (*maxL&cl* estimations). This observation corroborates the validity of our instruments in the other estimations as well.

The third robustness check focuses on contemporary threshold effects related to the stage of economic development, the quality of institutions and the size of the financial sector. We augment our baseline model with suitable interaction terms to control for these

[20] Models 5A and 6A are the typical cases when the interaction term understates conditionality.

[21] Due to the low number of countries in our sample, it is not possible to identify the exact location of threshold state history. While we select the median as threshold in the baseline model, the robustness checks suggest that our results also hold at alternative thresholds in the interquartile range.

[22] For details, consult the notes attached to the tables.

[23] Beyond the moment conditions related to the standard instruments (legal origin, imperial affiliation) and the exogenous regressors, we exploit the following moment conditions in the case of collapsing the instrument matrix: $E[\bar{x}_{i(t-l)} \Delta\varepsilon_{it} | l] = 0 \; \forall \; l \geq 2$ and $E[\Delta\bar{x}_{i(t-1)} \varepsilon_{it}] = 0$, where $t \geq 3$. When the lag order of the collapsed GMM instruments is also constrained, the moment conditions related to the transformed (first-differenced) equation changes to $E[\bar{x}_{i(t-2)} \Delta\varepsilon_{it}] = 0$, while the moment conditions related to the level equation remain unchanged.

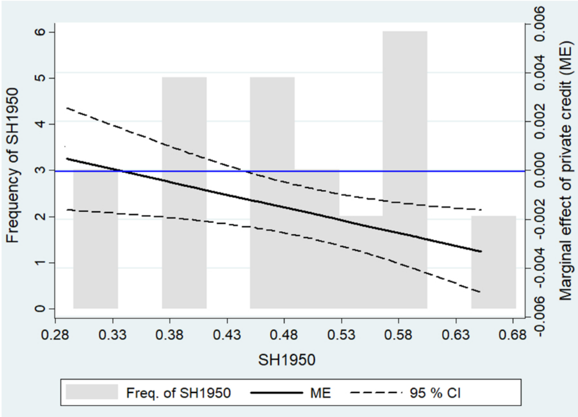

Fig. 4. The marginal effect of private credit on GDP per worker in case of continuous conditionality.
Notes: Estimated marginal effect of private credit in model 7(A): $\hat{\beta}_1 + \hat{\beta}_3 SH1950$. The dashed lines represent the 95 percent confidence interval.

channels. In the case of the institutional quality channel, the *FD·WGI* interaction is included in the model. The *FD·ln(y)(-1)* interaction controls for the development channel. Finally, we augment the model with the quadratic effect of the underlying financial development measure to take into account the financial channel.[24] Table A4 presents the results. The most important observation is that the *FD·SH1950(50*) term is hardly affected by the inclusion of contemporary thresholds. It preserves its sign, magnitude and significance robustly. These results suggest that breaks in the finance-growth relationship observed in our estimations are caused by ancestral institutions related to accumulated state history and are not driven by omitted contemporary threshold effects.

Finally, we consider the robustness to the sample, the dependent variable and the covariates.[25] We rerun the estimations on six constrained subsamples omitting the major regions of the post-socialist bloc – i.e., Central Asia, the Caucasus, Eastern Europe, the Baltics, Central Europe and Southeastern Europe – one at a time. We find no evidence that the threshold effect of state history would hinge upon the inclusion of a small number of countries in the sample. Regarding the dependent variable, the estimations are run on GDP per capita as well. The results are similar to the baseline results and deliver the same conclusions. As far as the conditioning set of Eq. (1) is concerned, we augment the model with several other conventional regressors such as government consumption, foreign direct investments, foreign trade, a banking crisis dummy and an EU membership dummy, one at a time. All of them prove to be consistently and overwhelmingly insignificant and they do not change our results. Moreover, we also control for time-fixed effects with similar outcomes: the included period dummies are mostly insignificant, leaving the previously explored nonlinearities unaffected.

6. Discussion

The results in the previous section underscore the transformative effect of state history on the finance-growth nexus and present novel implications for the relevance of experience with established statehood for financial development. Our results suggest that a long state history negatively affects the impact of finance on growth. We argue that state history as a proxy for exposure to institutions, centralization and political organization induces a negative effect of financial development on growth because it eases the provision of credit under conditions of adverse selection and moral hazard. It can lead to the formation of deeply entrenched interest groups and therefore to extractive institutions. Indeed, this is the argument of Borcan et al. (2018) underlying the proposed inverted U-shaped relationship between state history and economic development. As Dewatripont and Maskin (1995) indicate, credit decentralization resolves the commitment problem of the creditor and allows him not to refinance an unprofitable project. However, when the banking sector is excessively concentrated or under the tight control of the government, corruption in bank lending can become acute, entailing the inefficient allocation of financial resources and the refinancing of unprofitable projects in the form of soft budget constraints (Barth et al., 2009; Beck et al., 2006). This problem is further aggravated, even up to the level of “crony capitalism”, if the financial and political systems are captured by business elites (Morck et al., 2011).

Spolaore and Wacziarg (2013) identify a strong positive effect of ancestral institutions on economic development. Nevertheless, they do not consider any intermediate mechanisms that could condition the long-run impact of institutions on growth. As Ang (2015) indicates, state antiquity in itself remains a black box if one does not resort to more detailed analytical grounding including the role of financial instruments and government capacity. In a previous paper, Ang (2013) establishes the link between state antiquity and the level of financial development. Our results help to further open the black box of state history. They offer a channel, the finance-growth nexus, which mediates the negative effects of an excessively long state history.

[24] The inclusion of these FD interaction terms renders the conditionality of the finance-growth nexus continuous.
[25] The results of these sensitivity analyses are available upon request.

In this paper, we imply that countries with a longer state tradition are less likely to grow through their financial sector. This is particularly the case from the limited yet indicative nature of our sample. In East-Central Europe and the former Soviet Union, we observe financial and political systems that may frequently be captured by interest groups (Hellman et al., 2003). On the one hand, incumbent politicians can buy off the support of private interest groups through the provision of soft budget constraints – either directly or by exerting pressure on the financial system. On the other hand, financial development renders the prevention of market entry of new competitors costlier for incumbent business groups, thus spurring them to entrench themselves more deeply, raise entry barriers and capture the financial sector (Rajan and Zingales, 2003). In transition economies, political rent-seeking, long-run survival of governments and the preservation of corporate oligarchies have been crucial aspects of the political-economic process and inherently linked with financial development (Campos et al., 2012; Chen et al., 2015; Weill, 2011). Hence, ancestral statehood facilitates the provision of soft budget constraints as a result of business-government bargains, which also bolster bank concentration. Becerra et al. (2012) identify a tradeoff between state capacity and the influence of incumbent interest groups toward financial development. What we suggest is that the long-term survival of governments in the post-socialist region is likely to rely on bailouts of interest groups through the financial sector.

Within transition economies, the successor states of the Russian Empire show intermediate to low levels of state history, whereas the opposite holds for many of the successor states of the Austro-Hungarian and Ottoman Empires (intermediate to high levels of state history) (see Fig. 2). Our results show that financial development and credit provision inhibited rather than advanced growth in transition economies with a high ancestral institutional legacy. We propose that the competition of entrenched interest groups for easy credit contributed crucially to this unfavorable outcome. However, the evolution of the banking sector in post-socialist countries could also provide further impetus for the provision of soft budget constraints from the credit supply side. The transformation of socialist financial systems in the 1990s allowed bank consolidation, which in turn facilitated bank concentration and the proliferation of foreign banks (Bonin et al., 2015). Foreign banks, which are particularly dominant in East-Central Europe, have been much more inclined to lend to households and large companies, rather than to small and medium-size enterprises (ibid.). Moreover, bank concentration is likely to lead to inefficient bargains between banks and large enterprises, as well as between government and the overall private sector (Barth et al., 2009). Hence, ancestral statehood facilitates the provision of soft budget constraints in transition economies under the mutually enforcing pressure of interest group competition and bank concentration.

To sum up, while we confirm the positive direct effect of state history on growth, we also underscore its negative effect on financial development as a growth determinant *per se*. Institutions matter for growth, but not always in a positive way. The ambiguous role of finance in the economic transformation of East-Central Europe and the former Soviet Union can therefore to some extent be attributed to the divergent state history paths of historical Europe and Eurasia.

7. Conclusions

In this paper, we have identified the significance of ancestral institutions for the impact of finance on growth in transition economies. Our main result is that a solid tradition of ancient statehood can be detrimental to the effect of finance on growth in post-socialist countries, contrary to what one would expect from conventional wisdom. While our results do not contradict the main literature finding on the positive growth effect of state history, they also propose an important downside to the role of institutions for economic performance. When exposed to a long history of political organization and centralization that gives rise to extractive institutions, the financial channel becomes a non-optimal path for economic transformation due to the potential provision of soft budget constraints.

The role of ancestral statehood and its institutions in the finance-growth nexus, as it has been introduced in this paper, opens at least two new promising lines of research. First, it lends itself to the study of antiquity and its effects on global capitalism through the finance-growth perspective. Second, it suggests that other potential growth-inducing mechanisms such as the investment-growth nexus or the trade-growth nexus may also be worthy of a similar empirical investigation.

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Appendix A

Table A1
State history indexes, imperial affiliation in 1900 and legal origin.
Source: For the sources of state history indexes see Table 1. For each state history series, values below the median of the sample countries are in italics. Imperial affiliation dummies are own classifications. Independent is understood either literally or as non-occupied by any of the four empires considered (i.e., Russian, German, Ottoman, Austro-Hungarian). The French legal origin dummy is from La Porta et al. (2008). The Russian legal origin dummy takes on a value of one for (present or past) Commonwealth of Independent States member countries and Mongolia.

		State history indexes			*Dummies reflecting imperial affiliation in 1900*					*Legal origin dummies*	
Country	Code	SH1950	SH1500	SH1950adj	Russian	Austro-Hungarian	German	Ottoman	Independent	French	Russian (CIS& MNG)
Albania	ALB	0.572	0.580	0.572	0	0	0	1	0	1	0
Armenia	ARM	0.537	0.623	0.542	1	0	0	0	0	1	1
Azerbaijan	AZE	*0.469*	*0.445*	*0.475*	1	0	0	0	0	1	1
Belarus	BLR	*0.409*	*0.354*	*0.415*	1	0	0	0	0	0	1
Bosnia & Hercegovina	BIH	0.589	0.650	0.589	0	1	0	0	0	0	0
Bulgaria	BGR	0.652	0.600	0.652	0	0	0	1	1	0	0
Croatia	HRV	0.595	0.558	0.598	0	1	0	0	0	0	0
Czech Rep.	CZE	0.601	0.632	0.597	0	1	0	0	0	0	0
Estonia	EST	*0.290*	*0.133*	*0.338*	1	0	0	0	0	0	0
North Macedonia	MKD	*0.486*	0.474	*0.509*	0	0	0	1	0	1	0
Georgia	GEO	0.553	0.579	0.581	1	0	0	0	0	0	1
Hungary	HUN	0.592	0.623	0.590	0	1	0	0	0	0	0
Kazakhstan	KAZ	*0.396*	*0.169*	*0.437*	1	0	0	0	0	1	1
Kyrgyz Rep.	KGZ	*0.295*	*0.162*	*0.336*	1	0	0	0	0	1	1
Latvia	LVA	*0.321*	*0.147*	*0.376*	1	0	0	0	0	0	0
Lithuania	LTU	*0.455*	*0.271*	*0.463*	1	0	0	0	0	1	0
Moldova	MDA	*0.377*	*0.288*	*0.413*	1	0	0	0	0	1	1
Mongolia	MNG	0.520	*0.324*	0.528	0	0	0	0	1	0	1
Poland	POL	0.593	0.481	0.593	1	1	1	0	0	0	0
Romania	ROU	*0.462*	*0.397*	*0.467*	0	1	0	0	1	1	0
Russia	RUS	*0.456*	*0.119*	*0.456*	1	0	0	0	0	1	1
Serbia	SRB	0.651	0.645	0.642	0	1	0	1	1	1	0
Slovakia	SVK	*0.400*	*0.316*	*0.424*	0	1	0	0	0	0	0
Slovenia	SVN	0.505	0.506	0.514	0	1	0	0	0	0	0
Tajikistan	TJK	0.520	0.535	0.517	1	0	0	0	0	1	1
Ukraine	UKR	*0.384*	*0.309*	*0.397*	1	1	0	0	0	1	1

Notes: The data of Serbia on the French legal origin dummy correspond to the data of 'Serbia and Montenegro' in the source dataset.

Table A2
Sensitivity to the depreciation rate of state history.

FD variable	PCB		DC	
SH depreciation rate	*0 %*	*1 %*	*0 %*	*1 %*
FD	0.0005	0.0004	0.0004	0.0004
	(0.611)	(0.620)	(0.502)	(0.544)
SH1950	0.6094***	0.6269***	0.6208***	0.6312***
	(0.001)	(0.002)	(0.000)	(0.001)
FD·SH1950(50)	−0.0030***	−0.0030***	−0.0030***	−0.0030***
	(0.001)	(0.001)	(0.000)	(0.000)
-				
FD_upper	−0.0026***	−0.0026***	−0.0026***	−0.0026***
	(0.000)	(0.000)	(0.000)	(0.000)
Sargan test (pv)	0.689	0.669	0.662	0.635
Hansen test (pv)	1.000	1.000	1.000	1.000
AR(1) (pv)	0.112	0.112	0.122	0.121
AR(2) (pv)	0.262	0.268	0.304	0.309
no. of instruments	84	84	84	84
n	174	174	174	174

Notes: Dependent variable: ln(y). Results of models 2A and 2B when the SH1950 state history index is calculated according to alternative depreciation rates (0 % and 1 %). The constant and the coefficients of the conditioning set are omitted. *FD_upper* is computed as in Tables 4a and 4b. *P*-values calculated according to robust standard errors are in parentheses. Asterisks denote the significance level (*: 10 %, **: 5 %, ***: 1 %).

Table A3
Sensitivity to the estimation method.

FD variable	PCB				DC			
Method	*SGMM-1cl*	*SGMM-1 maxL & cl*	*SGMM-2 maxL & cl*	*OLS*	*SGMM-1 cl*	*SGMM-1 maxL & cl*	*SGMM-2 maxL & cl*	*OLS*
FD	0.0004	0.0003	0.0005	0.0012	0.0004	0.0001	0.0006	0.0010*
	(0.658)	(0.791)	(0.634)	(0.201)	(0.539)	(0.882)	(0.399)	(0.086)
SH1950	0.6174**	0.5820**	0.5757*	0.6067***	0.6660***	0.6254***	0.6047**	0.5646***
	(0.024)	(0.031)	(0.096)	(0.001)	(0.007)	(0.009)	(0.042)	(0.000)
FD·SH1950(50)	−0.0023**	−0.0023**	−0.0023**	−0.0026***	−0.0026***	−0.0025***	−0.0025**	−0.0024***
	(0.016)	(0.014)	(0.039)	(0.000)	(0.002)	(0.004)	(0.017)	(0.000)
-								
FD_upper	−0.0018**	−0.0021**	−0.0017*	−0.0014**	−0.0022**	−0.0024***	−0.0019*	−0.0014***
	(0.027)	(0.011)	(0.068)	(0.023)	(0.012)	(0.005)	(0.065)	(0.008)
Hansen test (pv)	1.000	0.178	0.178		0.999	0.205	0.205	
AR(1) (pv)	0.121	0.112	0.166		0.131	0.120	0.177	
AR(2) (pv)	0.248	0.193	0.399		0.263	0.222	0.363	
no. of instruments	54	24	24		54	24	24	
n	174	174	174	174	174	174	174	174

Notes: Dependent variable: ln(y). Results of models 2A and 2B according to alternative estimation methods. *SGMM-1 cl* – one-step system GMM with collapsed instruments (Roodman, 2009b), *SGMM-1 maxL&cl* – one-step system GMM with collapsed instruments and with lag order of GMM level instruments maximized at 2, *SGMM-2 maxL&cl* – two-step system GMM with collapsed instruments and with lag order of GMM level instruments maximized at 2. The constant and the coefficients of the conditioning set are omitted. *FD_upper* is computed as in Tables 4a and 4b. *P*-values calculated according to robust standard errors are in parentheses. Robust standard errors: two-step GMM (robust *s.e.* of Windmeijer, 2005), OLS (White s.e.), one-step GMM (see the notes of Table 4a). Asterisks denote the significance level (*: 10 %, **: 5 %, ***: 1 %).

Table A4
Sensitivity to the inclusion of contemporary threshold effects.

FD variable	PCB			DC		
CT: Contemporary threshold effect	Institutional channel	Development channel	Financial channel	Institutional channel	Development channel	Financial channel
CT variable (CTV)	*PCB·WGI*	*PCB·ln(y)(-1)*	*PCB·PCB*	*DC·WGI*	*DC·ln(y)(-1)*	*DC·DC*
ln(y)(-1)	0.8801***	0.8970***	0.8678***	0.8742***	0.8931***	0.8645***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
WGI	0.0666	0.0708**	0.0738**	0.0684	0.0663*	0.0758**
	(0.155)	(0.031)	(0.034)	(0.129)	(0.060)	(0.039)
FD	0.0000	0.0121	0.0010	0.0001	0.0095	0.0012
	(0.982)	(0.441)	(0.533)	(0.869)	(0.526)	(0.345)
SH1950	0.5302*	0.5085*	0.5636**	0.5538**	0.5342**	0.5910**
	(0.062)	(0.078)	(0.041)	(0.028)	(0.024)	(0.016)
FD·SH1950(50)	−0.0021**	−0.0019*	−0.0023**	−0.0021**	−0.0020**	−0.0025***
	(0.043)	(0.075)	(0.023)	(0.029)	(0.031)	(0.005)
CTV	−0.0001	−0.0012	0.0000	0.0000	−0.0009	0.0000
	(0.896)	(0.447)	(0.417)	(0.954)	(0.532)	(0.389)
-						
Sargan test (pv)	0.781	0.798	0.685	0.771	0.771	0.725
Hansen test (pv)	1.000	1.000	1.000	1.000	1.000	1.000
AR(1) (pv)	0.107	0.096	0.101	0.112	0.100	0.104
AR(2) (pv)	0.265	0.270	0.286	0.298	0.307	0.313
no. of instruments	94	94	94	94	94	94
n	174	174	174	174	174	174

Notes: Dependent variable: ln(y). Results of models 2A and 2B augmented by contemporary threshold effect: $\ln(y_{it}) = \alpha_i + \beta_1 FD_{it} + \beta_2 SH1950_i + \beta_3(FD_{it}\cdot SH1950(50)_i) + \bar{\gamma}^T[conditioning\ set_{it}] + \beta_4 CTV_{it} + \varepsilon_{it}$, where *CTV* is the interaction controlling for the respective contemporary threshold effect. The constant and the coefficients of the conditioning set – with the exception of *WGI* and *ln(y)(-1)* – are omitted. *P*-values calculated according to robust standard errors are in parentheses. Asterisks denote the significance level (*: 10 %, **: 5 %, ***: 1 %).

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