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Economic freedom and veto players jointly affect entrepreneurship

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Abstract

Purpose – The purpose of this paper is to explore how the strength of political veto players affects the long-run credibility of economic institutions and how they jointly affect entrepreneurial activity.

Design/methodology/approach – The authors employ an annual panel covering 30 OECD countries from 1993 to 2011.

Findings – An error correction model identifies a positive and significant short-run effect on self-employment from large government spending at low levels of veto player strength. A static model conversely indicates that smaller government spending is positively associated with entrepreneurship at lower levels of veto player strength in the long run.

Originality/value – The authors are the first to explore the interaction of economic and political institutions in the development of entrepreneurship.

Keywords Entrepreneurship, Economic freedom, Veto institutions

Paper type Research paper

1. Introduction

Commentators, politicians, and pundits have in recent years begun to highlight the importance of entrepreneurs as prime movers of development. Politicians seek to promote entrepreneurship because they see entrepreneurs as the most important actors creating the foundation for future growth and welfare. It is easy to see the importance of single entrepreneurs: what would the modern world look like without Alexander Graham Bell, Henry Ford or Bill Gates, i.e. without the telephone, the first affordable automobile – Ford's Model T – and personal computer technology that does not require experts to operate it?

In economics, the interest in entrepreneurship research has also increased since the mid-1980s. Both theoretical studies such as the pioneering work in Baumol (1990), Aghion and Howitt (1992), and Kirzner (1997), as well as recent empirical studies, have verified the relationship between entrepreneurial activity and long-run development (Reynolds *et al.*, 1999; Audretsch and Thurik, 2000; Wennekers *et al.*, 2005; Bjørnskov and Foss, 2013). Such growth effects of entrepreneurship make policy-makers eager to promote entrepreneurial activity. The theoretical and empirical findings have therefore created further interest in studying the antecedents of entrepreneurship, and in particular institutions and policies affecting entrepreneurship.

However, while Baumol (1990) famously theorized that institutional differences effectively affect the allocation of entrepreneurship, most studies in institutional economics have instead investigated how institutions affect cross-country differences in economic growth or foreign direct investments. Comparatively, few studies have focused on the potentially growth-relevant connections between institutions and entrepreneurs, although a large literature documents the overall effects of institutions on long-run growth and productivity (Henisz, 2000; Klein and Luu, 2003; Rodrik *et al.*, 2004; Justesen and Kurrild-Klitgaard, 2013).



Bjørnskov and Foss (2016) survey the specific literature on institutions and entrepreneurship and identify a number of lacunae in existing studies. They note that a series of previous papers have credibly established the existence of a general association between economic institutions and various measures of entrepreneurship (e.g. Kreft and Sobel, 2005; Bjørnskov and Foss, 2008; Chowdhury *et al.*, 2015). However, one of the problems that have so far been ignored in the entrepreneurship literature is response heterogeneity, i.e. if similar institutions only produce similar outcomes when other conditions or institutions are also similar. We believe that this is a particularly important problem because the parallel literature on long-run development and institutions has recently shown that veto player institutions in particular affect the credibility and stability of economic institutions and thereby improve their economic effectiveness (Tsebelis, 2002; Justesen and Kurrild-Klitgaard, 2013; Justesen, 2014).

We therefore take a step further than the existing literature by applying these insights to the study of entrepreneurship, and explore the joint effects of economic freedom and political veto institutions on entrepreneurial activity. We do so in an annual panel of 30 OECD countries observed between 1993 and 2011 for which comparable data exist. We separate short- and long-run consequences by estimating both an error correction model (ECM) and a static panel. While the static model that identifies what we interpret as long-run equilibrium effects shows the well-known association with government size and exhibits larger effectiveness with weak veto player institutions, the short-run effects appear to run in the opposite direction.

The rest of the paper is structured as follows. Section 2 outlines an intuitive theoretical framework in which we integrate entrepreneurship and institutional mechanisms and derive a set of testable hypotheses. Section 3 presents the data and estimation strategies while Section 4 presents the empirical findings. We conclude the paper in Section 5 in which we discuss how to reconcile the findings with previous insights.

2. Institutions and entrepreneurship

During the last few decades, an increasing theoretical interest in the effect of institutions on entrepreneurship has emerged (Bjørnskov and Foss, 2016). Most studies apply a version of North's (1991, p. 97) definition of institutions as "humanly devised constraints that structure political, economic and social interaction." According to North (1991), institutions can be formal (laws, bureaucracy, regulations, etc.) or informal (social norms, values, beliefs, etc.). This distinction implies that formal institutions may be easier to quantify than the "softer" informal institutions, as they are in principle directly quantifiable via regulative measures (Voigt, 2013). The institutions set the rules of the game in society and influence the behavior of individuals and organizations and can thereby affect entrepreneurship in multiple ways. As Baumol (1990) emphasized, institutional differences may either affect the supply of potential entrepreneurs or their incentives.

Bjørnskov and Foss (2016) provide a survey of the existing literature on the links between entrepreneurship and the institutional environment, which we build on in the following. Within the tradition from Baumol (1990), the institutional setting mainly affects the allocation of entrepreneurship, which can be productive (wealth creating), unproductive (purely redistributive), and destructive (rent seeking), depending on institutional characteristics. The omnipresence of entrepreneurship implies that the effects of institutional differences must be major determinants of the different directions in which entrepreneurial activity is channeled such that, as argued by Boettke and Coyne (2003), economic development as a consequence of particularly productive entrepreneurship is incentivized by supporting institutions. As such, the institutional environment deters or reinforces risk-taking behavior and influences the coordination of human transactions (North, 1991). The profit-seeking activities of entrepreneurs are thereby influenced by the

institutional framework, which enables agents to engage in higher-level economic activity, because good institutions create greater certainty and a clearer prediction of the future.

According to Kirzner (1973), the institutional environment and its quality for entrepreneurs thereby provide a way for policy-makers to unleash (or limit) an inherent entrepreneurial spirit, which has implications for future economic development. Indeed, entrepreneurs are often conceptualized as economic actors that have better foresight or comparative advantages in handling and benefitting from substantial uncertainty although they will also be more sensitive to uncertainty than established actors (Foss and Klein, 2012). Yet, different types of institutions may affect entrepreneurial activity and the information processing ability of entrepreneurs through several separate mechanisms.

First, Schumpeter (1934) specifically identified property rights as a financial motive for pursuing entrepreneurial business. Referring to the economics of information pioneered by Knight (1921), he recognized that changing conditions via economic institutions cause higher uncertainty and described how entrepreneurs by nature of their particular function work in a dynamic and uncertain environment with changing opportunities to make profits. Schumpeter's main concern was how secure property is, i.e. the quality of judicial institutions protecting private property, as a high risk of theft or expropriation would reduce the incentives to entrepreneurship. Similarly, Bjørnskov and Foss (2008) emphasize the role of the stability and predictability of prices – known as institutions securing “sound money” – as monetary fluctuations and surprises also introduce substantial insecurity in future earnings.

Second, Kirzner (1973) sees entry barriers to the market as an important limit for entrepreneurs and the development of the economy. While poor property rights institutions and unpredictable monetary institutions might affect the willingness of potential entrepreneurs to act on their ideas, regulatory entry barriers directly affect their ability to, for example, start up a new business (Klapper *et al.*, 2006). In public choice, this function of regulations has been a stable theme since Stigler (1971) illustrated how many regulations effectively protect existing firms from competition from new entrants. Regulations providing entry barriers include particular capital requirements, financial restrictions, technological requirements, as well as closed shop legislation that would increase the costs of entrepreneurial firms.

These types of regulations are often chosen by existing firms with political influence in “crony capitalist” regimes where political connections are necessary to establish activity within an apparently free market economy (Holcombe, 2013). However, as argued by Dreher and Gassebner (2013), when regulations are particularly tight, it may be economically optimal for judicial institutions to allow for some degree of corruption, as limited corrupt practices can serve as a way around excessive regulations. Very similar mechanisms apply to trade restrictions and access to international markets, such that corruption can serve as “greasing the wheels” in the presence of poor institutions (cf. Bologna and Ross, 2015).

Finally, welfare state institutions could theoretically affect entrepreneurship in both positive and negative direction. Henrekson (2005) notes that a popular argument in favor of, for example, Danish flexicurity institutions that combine flexible labor market institutions with substantial unemployment benefits is that they further job matching and entrepreneurship. Generous welfare institutions that allow unemployed individuals to stay on benefits for an extended period might also allow them to search for a job that matches their qualifications, which includes entrepreneurial choices. Conversely, generous benefits may undermine individuals' willingness to start up a new business from which they may receive limited income in the first year. Likewise, high marginal taxes de-incentivize innovative entrepreneurship by substantially reducing the benefits should the new firm or other endeavor succeed.

In addition, a large government sector reduces entrepreneurship in two other ways. First, very few government sectors are innovative and entrepreneurial *per se*, and their

employees are therefore highly unlikely to become entrepreneurs. Second, most government services and production are subject to soft budget constraints and particular access to politicians (e.g. Niskanen, 1975; Kornai, 1979). The larger a share of the economy is controlled or dominated by the government sector, the larger a share is effectively protected from competition because government activity is not subject to regular profit and bankruptcy considerations. Most theoretical arguments thus speak in favor of limited government. A consensus has also formed in the empirical literature that government size, the tax burden, and the generosity of welfare state institutions are robustly negatively associated with entrepreneurial activity (Bjørnskov and Foss, 2008; Nyström, 2008; Boudreaux and Nikolaev, 2016).

While previous studies document how institutions and institutional choices affect entrepreneurial activity in several ways, recent development in political economy and institutional economics suggests a potentially important qualification to any general association. This qualification relates to the expected permanence of institutional choices and in particular to the role of veto players and veto institutions.

Tsebelis (2002, p. 36) defines veto players as “individual or collective actors whose agreement [by majority rule for collective actors] is necessary for a change of the status quo.” The veto players exist either as part of the political system (partisan veto players) or the constitutional order (institutional veto players) and are theorized to enable stable and credible policies because changing the status quo is difficult when multiple veto players can block changes. A constrained political environment via multiple veto players increases predictability and reduces risk, because politicians constrained by political veto institutions cannot easily pursue opportunistic policies. Conversely, unconstrained politicians and governments have discretionary power to decide and implement significant policy changes if they have the motive and the opportunity to make the change (Tsebelis, 2002).

Building on Northian institutional theory, the institutional environment of veto players and their power can thus either deter or reinforce the risk-taking behavior and influence the coordination of human transactions, and stable veto institutions are expected to reduce the uncertainty in coordinated transactions (North, 1991). For example, institutional veto players arguably create more credibility compared to partisan veto players since the constitution is harder to change deliberately than regular political compromises that are subject to log-rolling negotiations (Justesen and Kurrild-Klitgaard, 2013).

Yet, their effects may be either direct or indirect. A direct effect would occur if, for example, strong veto players reduce the transaction costs associated with arbitrary political changes in business regulations that create uncertainty and lower the incentive for private investments (Henisz, 2000). Multiple veto players could also block unpopular policies, and make government promises more trustworthy and predictable, which has growth-enhancing effects (cf. North and Weingast, 1989). A central point is that the likelihood of policy changes decreases with the number of veto players, less congruence, unique positions, and a lack of cohesion of veto players (Tsebelis, 2002; Keefer and Stasavage, 2003). Political constraints and a strong separation of political power create diverse and distinct policy preferences, and increase the transaction costs of decision making and limit decisions to those giving expected Pareto improvements (Justesen, 2014). Conversely, politics with weak veto institutions tend to be negotiated in an insecure and uncertain political environment with the risk of significant and non-optimal changes. With strong veto institutions, the policy changes that one observes at the margin may therefore be substantially more beneficial and less subject to special interest influence than those observed in polities without similar veto institutions. In addition, strong veto institutions tend to reduce policy uncertainty, and thereby the transaction costs associated with planning without knowing the future regulatory environment (Baker *et al.*, 2016).

However, veto institutions also come with indirect effects as they are likely to create log-rolling problems and lock in any policy and institutional decision (Tsebelis, 2002). A collective action problem arises when decentralized governments are motivated to over-spend to please veto players' own constituencies, which may lead to budget deficits because of the internalized costs for each veto player. In the longer run, Tullock (1981) argues that any policy package will become more expensive, the more interests will be necessary to include to negotiate a workable majority in favor of it. Strong veto institutions might therefore increase the size of government and the associated policy inertia implies that in the case of multiple veto players, it becomes harder to reduce the size of the government. More veto players may thereby slow down the pace of potentially beneficial adjustments of the size of government.

A final related point thus also is that veto institutions can lock in any policy decisions and perpetuate their consequences, whether good or bad. In summary, our main theoretical expectations that we test in the following are two-fold. First, we expect any clear institutional differences to have stronger consequences in the long run when veto institutions are stronger because the differences are permanent. Second, we expect that institutional changes may have stronger immediate effects – or more readily observable effects – when veto institutions are strong. Our reason to extrapolate this expectation from the considerations above is that veto institutions would ensure that policy decisions – bad or good – are unlikely to be reversed. Any existing firm or potential start up would therefore have incentives to immediately bear the transaction costs associated with the change, while firms in polities with weak veto institutions should rationally wait to observe if the policy change is permanent and therefore implement any required changes over a longer time period. We thereby also expect the lag structure of any transaction costs associated with policy and institutional changes to differ, depending on the likely permanence of the changes.

3. Data and empirical strategies

To test these implications, we start by following recent studies by broadly defining entrepreneurship as “the manifest ability and willingness of individuals on their own, in teams, within and outside existing organizations” to create new opportunities and ideas in the face of uncertainty (Wennekers and Thurik, 1999, pp. 46-47). Matching this definition to an actual, practical measure nevertheless remains a challenge when internationally comparable data series are required (van Stel, 2005).

Accepting the use of self-employment as a viable proxy for entrepreneurship, the COMPENDIA database provides a harmonized data set from 1970 to 2011, measuring business ownership rates for 30 OECD countries (Panteia, 2013)[1]. The COMPENDIA data set corrects and harmonizes data from the OECD Labor Force Statistics, to take deviating definitions of self-employment into account (van Stel, 2005). Panteia (2013) defines the business ownership rate as the number of business owners in society, divided by the total labor force; business owners are defined as persons who are owning and managing a business. The measure includes owners and managers from incorporated (owner/managers of own businesses) and unincorporated firms (sole proprietors or partners) but excludes unpaid family members and business owners who own as a secondary activity.

The database also distinguishes between self-employment in non-primary and primary industries because of the structural differences. In agriculture, forestry, hunting, and fishery, self-employment is a quasi-natural status of employment. In the following, we therefore use self-employment excluding agriculture, forestry, hunting, and fishing industries to capture the closest definition of real entrepreneurship (cf. Nyström, 2008). Our choice alleviates two particular problems. First, rural self-employment is often a result of limited alternative employment options, and therefore does not correlate with new

business density or measures of innovation (Faggio and Silva, 2014). Second, the measure corresponds to a Knightian definition of entrepreneurship instead of, e.g., Schumpeterian definitions as used in Henrekson and Sanandaji (2014) that require new firms to be successful *ex post*.

As such, COMPENDIA is one of the most reliable data sets on self-employment statistics, but can still be criticized, for example for using a single correction factor for each country, which is applied to all years, instead of a yearly correction factor (van Stel, 2005). We also emphasize that non-agricultural self-employment mainly reflects the risk-taking aspect of entrepreneurship since self-employed have uncertain remuneration and future business opportunities (van Stel, 2005). This implies that the measure is especially aligned with the Knightian entrepreneur, which assumes that the self-employed carry all the uncertainty (Iversen *et al.*, 2008). Conversely, innovative actions are not a common activity among the majority of self-employed. As such, while not all self-employed are entrepreneurs, entrepreneurs in a Knightian sense are a subset of the self-employed, and productive and unproductive entrepreneurs are a further subset of the Knightian entrepreneurs. Conversely, our measure is unlikely to capture many directly destructive entrepreneurs that are both rare and less likely to be self-employed[2]. While the measure therefore is far from optimal, the availability of alternative time series measures of entrepreneurship and their ability to proxy for innovation are nevertheless lacking or non-existing[3].

To proxy for institutional business characteristics and policies, the Index of Economic Freedom from the Heritage Foundation (2016) provides a yearly index available for 186 countries since 1993. We scale each index to 0-10, with higher scores entailing higher freedom[4]. With all indices, the main idea behind the index is that a free market is probably superior to central planning and strongly regulated firms.

The Index of Economic Freedom consists of nine primary indices, which can be aggregated to either the full index or four “pillars” of economic freedom: rule of law; government size; regulatory efficiency; and market openness. All indices are created from easily verifiable data from a number of different sources and have been scaled to be distributed from 0 (the lowest possible level) to 100 (the highest possible level) (Heritage Foundation, 2016). The choice of the Heritage index over alternative indices from, e.g., the Fraser Institute is a matter of practicality, as it is the only index covering a large sample of countries that is available on an annual basis further back in time than 2000.

Our second main variable of interest is a particular measure of the institutional strength of veto players with which we test our hypothesis about institutional permanence. We use an index of political constraints, known as PolCon III, which is developed by Henisz (2000) as a response to existing and dubiously performing measures of veto players and political constraints. The index is based on counting the number of branches of government at the executive and legislative levels with veto power over decisions of changing policy. These data are then modified to take into account that the executive and legislative branches may be politically aligned – which reduces veto powers – and modified to take into account preference heterogeneity within the branches, in which case veto powers increase. The resulting index is scaled between 0 and 1 where a value of 0 represents a single (effectively autocratic) executive who is unconstrained in his/her political work by any checks and balances, after which the score increases with a diminishing value when multiple veto players are added and the more polarized they are.[5] In the present sample, the index is distributed between values of 0.12 (in Poland in 1993) and 0.72 (in Belgium in the early 2000s) and a median around 0.44 (typical of Canada and Ireland), and with a within-country standard deviation about half of the full-sample standard deviation. As such, depending on the political circumstances, we observe substantial veto power changes within countries.

With respect to the further control variables, we follow past empirical studies even though the literature has yet to converge on a standard specification (Bjørnskov and Foss, 2016).

We include the one-year lagged real GDP per capita in purchasing power US dollars, the unemployment rate, and a dummy for being a post-communist country in the baseline regression. GDP and unemployment derive from Panteia (2013) and originally stem from the OECD National Accounts and Main Economic Indicators.

We use the logarithm to GDP per capita as a necessary control for economic development and sophistication (cf. Ovaska and Sobel, 2005; Dreher and Gassebner, 2013). Unemployment is included as a check on the unemployment-push hypothesis, which predicts that self-employment may increase in situations of high unemployment (Carree, 2002). Although self-employment is a natural opportunity out of unemployment and thereby reflects necessity entrepreneurship, the empirical evidence for the unemployment-push hypothesis is mixed (Nyström, 2008; Kreft and Sobel, 2005). Finally, we include a post-communist dummy following several studies indicating that these countries remain characterized by decades of oppression and central economic planning.

Table I presents the descriptive statistics. The full data set is a slightly unbalanced panel of 560 observations consisting of up to 19 observations for each of the 30 countries from 1993 to 2011. While the COMPENDIA data on self-employment are in principle available from the early 1970s, the sample is restricted by the availability of the economic freedom data. Although the Heritage Foundation index provides the longest unbroken annual series, the first data are from 1993.

We use these data in two separate ways. We first estimate a static model, which we argue is likely to capture long-run effects of institutions. Second, we estimate an ECM, which from a methodological point of view is an efficient way to overcome problems arising from non-stationarity in panel data (De Boef and Keele, 2008). However, the ECM is more sensitive to short-run effects that can differ substantially from longer-run consequences. As such, the results of the static model can be compared to common practice in the field of entrepreneurship research (e.g. Bjørnskov and Foss, 2008; Nyström, 2008; Dreher and Gassebner, 2013; Chowdhury *et al.*, 2015). We thus estimate the model in (1), where the self-employment rate SER is regressed on a vector of control variables C_{it} , economic freedom/institutions I_{it} , the strength of veto players V_{it} , and where YEAR represent a full set of annual fixed effects[6]. The main innovation is the model in (2) where we introduce an interaction term between I_{it} and V_{it} , following Justesen and Kurrild-Klitgaard (2013). The parameter γ thereby represents the heterogeneity of the institutional effects on self-employment, conditional on veto players, and vice versa (Brambor *et al.*, 2006):

$$SER_{it} = \alpha_0 + \beta_1 C_{it} + \beta_2 I_{it} + \beta_3 V_{it} + \beta_5 YEAR_i + \varepsilon_{it} \quad (1)$$

	Mean	SD	Observations
Entrepreneurship	11.525	4.209	570
Log GDP per capita	4.359	0.182	570
Unemployment rate	7.277	3.723	570
Post-communist	0.133	0.340	570
Political constraints	0.453	0.118	570
Rule of law	7.325	0.883	560
Government size	4.986	1.594	560
Regulation	7.958	0.771	560
Market openness	7.325	0.883	560

Table I.
Descriptive statistics

Notes: All variables used in the study are shown above. The total sample consists of observations from 30 OECD countries from 1993 to 2011

$$SER_{it} = \alpha_0 + \beta_1 C_{it} + \beta_2 I_{it} + \beta_3 V_{it} + \gamma I_{it} V_{it} + \beta_5 YEAR_i + \varepsilon_{it} \quad (2)$$

We primarily employ the ECM as analyzing yearly panel data over 19 years can potentially lead to problems in panel applications, because non-stationary variables regressed on each other can cause spurious regressions. To test for non-stationary variables, we conduct a unit root analysis. In spurious regressions, a high explanatory power seems to exist even without true causality between the variables. Non-stationarity also questions the potential co-integration of variables, which can arise if a stationary linear combination of order I(0) exists for non-stationary variables. We use Fischer's unit root test to test for non-stationarity for which we calculate four p -values, which follow different distributions; all test against the null hypothesis of non-stationarity.

In line with Sobel and Coyne (2011), Table II shows that some institutional variables suffer from unit roots such that institutional changes tend to be persistent and non-stationary. The exceptions are the rule of law and government size, and probably also market openness. In other words, these tests imply that changes in veto institutions and regulatory institutions tend to be permanent and do not revert to the mean[7]. As such, tests on panel data sets with small T ($= 18$) and higher N ($= 30$) generally question the reliability and assumptions applied in some versions of our static model. However, the test is used in combination with previous empirical findings as the best available solution. The favored case is to test time series data on a single unit with a large T (Plümper *et al.*, 2005). With the ECM, the potential sensitivity of the results is no problem since it also yields consistent estimates with stationary data (De Boef and Keele, 2008).

Yet, while the ECM in principle enables us to separate short-run changes within a business cycle from long-run equilibrium effects of policy changes, which affect self-employment, the practical separation is difficult. For instance, a reduction in government size could lead to a negative short-run effect on self-employment if it is associated with a slowdown in the economy or if small enterprises have previously supplied the now reduced public sector, while resulting in a long-run increase. In such cases, the temporal structure of the full effects will resemble a J-curve. Because our primary interest is the long-run effects, separating those correctly from short-run consequences requires that the lag structure is known and correctly modeled.

If the lag structure is not modeled correctly, applying an ECM comes with the inherent risk that some of the short-run adaption costs are assigned to the long run, causing the main estimates to be inconsistent. This problem is exacerbated if institutional differences affect the lag structure or if the lag structure for some other reason differs systematically across countries. Given that we add an interaction term that separates effects conditional on

	Inverse χ^2 (P)	Inverse normal (Z)	Inverse logit (L^*)	Modified inverse χ^2 (P_m)	Unit root?
Self-employment	0.253	0.679	0.673	0.265	Yes
GDP per capita	0.024**	0.691	0.505	0.016***	No
Unemployment rate	0.380	0.874	0.821	0.402	Yes
Political constraints	0.498	0.993	0.974	0.522	Yes
Rule of law	0.000***	0.000***	0.000***	0.000***	No
Government size	0.003***	0.010**	0.007***	0.001***	No
Regulation	0.353	0.155	0.158	0.373	Yes
Market openness	0.088*	0.298	0.298	0.081*	No

Notes: Fischer's unit root test. P -values reported. H_0 "all panels contain unit roots." Augmented Dicky Fuller (ADF) test performed with assumed lags of 1 and yearly adaptation. Demeaning is applied to avoid potential bias from cross-sectional dependence. * $p < 10$ percent; ** $p < 5$ percent; *** $p < 1$ percent

Table II.
Fischer's unit root
test of variables

veto player institutions that exactly affect the permanence of institutions, we emphasize that the likely existence of a heterogeneous lag structure across countries and time is a major problem in the ECM. We therefore prefer to interpret the ECM estimates as approximately correct in the short run but potentially misleading in the long run. We also add an ECM with two-year lags instead of a purely annual model. The sample size used for these tests is therefore smaller as the data set consists of a two-year panel in order not to have overlapping observations.

In both the static model and the ECM, we estimate all effects using OLS with panel-corrected standard errors (Beck and Katz, 1995). We thus follow previous research that also notes that this choice is a good “average” estimator (e.g. Dreher and Gassebner, 2013; Chowdhury *et al.*, 2015). Beck and Katz (1995) for example note that the estimator is able to account for the most severe problems in political panel data sets, such as heterogeneity, cross-sectional dependence, and autocorrelation. Additionally, the estimator does not destroy relevant variation in the data and is often used as an alternative for panel data, when many assumptions are broken (cf. Cochrane, 2012). We therefore apply a configuration with a Prais-Winsten regression to account for the AR(1) process with panel-corrected standard errors that accounts for both panel-specific autoregressive processes and heteroscedasticity.

Finally, for all estimates of specification (2), we perform a full country jackknife test. In other words, we repeat all estimates, removing all observations from each single country at a time, which allows us to test the robustness of our general results. We do not report all of these estimates (which would be $30 \times 4 \times 3 = 360$ regressions), but simply refer to the results in the text.

4. Empirical results

We start by showing the simple cross-country association between two elements of economic freedom – the rule of law and government size – that previous studies have found are correlated with entrepreneurship (e.g. Nyström, 2008; Dreher and Gassebner, 2013). As illustrated in Figures 1 and 2, the simple correlation across the 30 country averages between the rule of law index and self-employment rates is -0.54 , and -0.73 when excluding

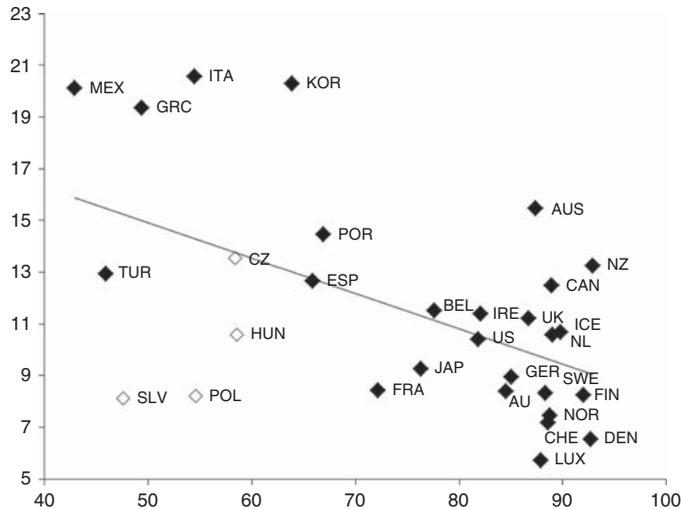


Figure 1.
Self-employment and
the rule of law

Note: The white dots denote post-communist countries

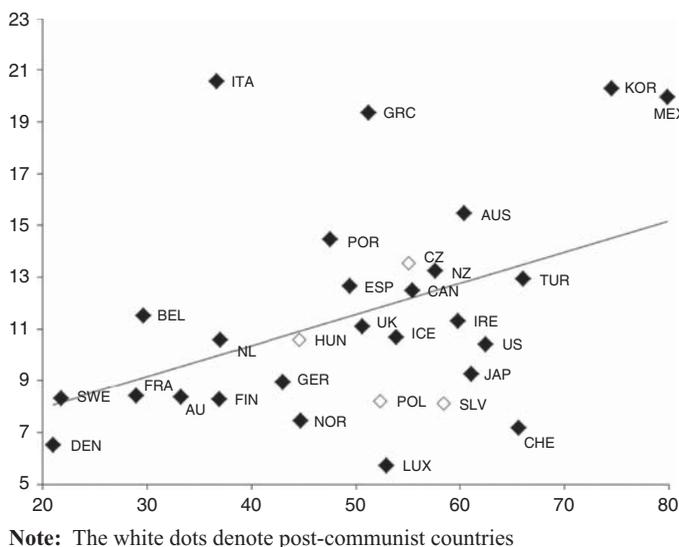


Figure 2.
Self-employment and government size

the four post-communist countries. The correlations with government size are 0.42 and 0.45, respectively. The patterns in these plots are thereby consistent with previous research, yet Figure 2 in particular exhibits two potential outlier countries: Italy and Greece.

However, these figures can evidently not inform about the causality or robustness of the relations, nor can they tell us much about the role of veto institutions. We therefore turn to the formal results reported in Tables III-V. We begin with the static model and report the estimates of the average effect of economic freedom in odd-numbered columns and add an interaction with the strength of veto players in even-numbered columns.

Table III first of all demonstrates the relevance of our minimal specification: GDP per capita is strongly negatively related to entrepreneurship, as is the unemployment rate. Similarly, we observe that post-communist countries have substantially lower rates of entrepreneurship, at least as measured by self-employment. Conversely, we find no evidence of any direct effects of veto institutions, as measured by the index of political constraints.

Turning to the main purpose of this paper in odd-numbered columns where we test the specification in Equation (1), we find substantial evidence consistent with previous studies that self-employment is negatively associated with the rule of law and positively associated with the government size index (i.e. negatively associated with the actual size of the government sector). In the even-numbered columns, where we test Equation (2) and thus include interaction terms, we find one notable result: the effect of government size is significantly and substantially decreasing in political constraints. In other words, we observe that an increase in the degree of government fiscal intervention in the economy is more detrimental to entrepreneurial activity when the incumbent government is not subject to strong veto institutions.

While we observe that the point estimate on government size is somewhat reduced when excluding Finland from the sample, the variation in the government size estimate is insignificant in a full jackknife test, and the effects of government size remain significant and sizeable. Conversely, excluding Italy renders the rule of law estimate much smaller and insignificant, thereby questioning how robust or generalizable the result is. Yet, we must also note that the static model may not provide ideally consistent estimates due to stationarity problems in some specifications. We therefore turn to the results in Table IV, where we report

Table III.
Main results,
static model

	Rule of law		Government size		Regulation		Market openness	
Log GDP per capita	-15.282*** (0.710)	-16.109*** (0.765)	-13.607*** (0.989)	-15.812*** (0.614)	-11.231*** (0.878)	-11.634*** (0.881)	-11.290*** (0.875)	-13.799*** (0.727)
Unemployment rate	-0.226*** (0.021)	-0.196*** (0.021)	-0.102*** (0.019)	-0.116*** (0.019)	-0.106*** (0.023)	-0.117*** (0.023)	-0.108*** (0.023)	-0.132*** (0.023)
Post-communist	-6.147*** (0.298)	-6.821*** (0.304)	-4.587*** (0.607)	-4.791*** (0.449)	-4.171*** (0.330)	-4.398*** (0.348)	-4.173*** (0.329)	-5.170*** (0.295)
Economic freedom	-0.303*** (0.078)	-0.320** (0.146)	0.243*** (0.059)	0.807*** (0.146)	-0.018 (0.063)	0.169 (0.185)	0.004 (0.031)	-0.104 (0.136)
Political constraints	-0.495 (0.412)	-2.279 (1.839)	-0.464 (0.411)	4.711*** (1.102)	-0.341 (0.328)	2.363 (2.682)	-0.341 (0.329)	-2.089 (1.982)
Freedom x constraints		0.238 (0.222)		-1.146*** (0.245)		-0.355 (0.326)		0.201 (0.255)
Annual FE	Yes							
Observations	560	560	560	560	560	560	560	560
R ²	0.927	0.931	0.919	0.936	0.892	0.909	0.887	0.911
Wald χ^2	1416.27	2626.86	332.67	2978.46	354.09	543.07	400.90	912.32

Note: **p < 5 percent; ***p < 1 percent

	Rule of law	Government size	Regulation	Market openness
Lagged	-0.016*** (0.006)	-0.008* (0.004)	-0.010** (0.004)	-0.009* (0.005)
entrepreneurship				
L Log GDP per capita	-0.049 (0.153)	-0.301** (0.126)	-0.428*** (0.133)	-0.299** (0.144)
Δ Log GDP per capita	0.807 (2.113)	0.958 (2.118)	1.346 (2.089)	0.445 (2.121)
L Unemployment rate	-0.012** (0.005)	-0.011** (0.006)	-0.009 (0.005)	-0.008 (0.005)
Δ Unemployment rate	-0.083** (0.019)	-0.081*** (0.019)	-0.079*** (0.019)	-0.079*** (0.019)
Post-communist	0.174* (0.092)	0.198** (0.088)	0.175* (0.090)	0.195** (0.087)
L Economic freedom	-0.037** (0.017)	-0.017 (0.012)	0.049 (0.032)	0.027 (0.027)
Δ Economic freedom	-0.011 (0.016)	-0.048** (0.024)	-0.199* (0.119)	0.008 (0.026)
L Political constraints	0.177 (0.127)	0.217* (0.121)	0.299** (0.118)	0.228* (0.127)
Δ Political constraints	-0.001 (0.002)	-0.001 (0.002)	-0.000 (0.002)	-0.001 (0.002)
L Freedom x constraints	-0.039 (0.085)	0.087 (0.086)	0.077 (0.172)	0.031 (0.141)
Δ Freedom x constraints	0.005 (0.003)	0.288 (0.222)	0.029 (0.435)	0.527** (0.204)
Annual FE	Yes	Yes	Yes	Yes
Observations	530	530	530	530
R^2	0.167	0.172	0.166	0.169
Wald χ^2	506.27	501.00	506.76	491.41
	535.22	490.84	518.45	514.32

Notes: * $p < 10$ percent; ** $p < 5$ percent; *** $p < 1$ percent

Table V.
Main results, ECM
with double lags

	Rule of law	Government size	Regulation	Market openness
Lagged entrepreneurship	-0.021* (0.013)	-0.011 (0.011)	-0.015 (0.011)	-0.016 (0.010)
<i>L</i> Log GDP per capita	-0.433* (0.254)	-0.409 (0.265)	-0.516** (0.254)	-0.588** (0.251)
Δ Log GDP per capita	-0.632 (2.687)	-0.115 (2.628)	-1.433 (2.766)	-1.149 (2.677)
<i>L</i> Unemployment rate	-0.012 (0.012)	-0.012 (0.012)	-0.007 (0.012)	-0.007 (0.012)
Δ Unemployment rate	-0.092*** (0.023)	-0.084*** (0.024)	-0.085*** (0.024)	-0.082*** (0.025)
Post-communist	0.386** (0.188)	0.479*** (0.170)	0.489*** (0.154)	0.423*** (0.157)
<i>L</i> Economic freedom	0.119 (0.078)	-0.063 (0.073)	0.149 (0.149)	0.099 (0.134)
Δ Economic freedom	-0.144** (0.065)	-0.072 (0.092)	0.149 (0.118)	0.071 (0.089)
<i>L</i> Political constraints	3.135** (1.499)	0.378 (0.499)	2.142 (3.189)	0.804 (2.201)
Δ Political constraints	0.527 (0.481)	0.239 (0.433)	0.263 (0.425)	0.257 (0.490)
<i>L</i> Freedom \times constraints	-0.312* (0.175)	0.067 (0.157)	-0.168 (0.379)	-0.018 (0.284)
Δ Freedom \times constraints	1.591*** (0.677)	-0.432 (0.767)	-0.591 (1.799)	-0.571 (1.239)
Annual FE	Yes	Yes	Yes	Yes
Observations	261	261	261	261
R^2	0.278	0.259	0.260	0.260
Wald χ^2	218.37	145.15	126.12	139.05

Notes: * $p < 10$ percent; ** $p < 5$ percent; *** $p < 1$ percent

the results of employing an ECM. These results are more consistently measured, but – as we argue above – also likely to mainly capture short- to medium-run consequences.

In Table IV, we first of all observe a clear convergence/regression to mean effect as the lagged entrepreneurship rate is significantly negatively associated with annual changes in entrepreneurship. While we observe that the association between entrepreneurship and GDP is fragile and fails when our measure of economic freedom is the rule of law, the short-run estimates of effects of unemployment are strong and significant throughout. This provides a first indication that the unemployment estimates identified in the static model in Table III primarily reflect short-run effects, i.e. deviations from some stable or slow-moving natural rate of unemployment.

Turning again to the main estimates, we find a negative effect of the rule of law. We nevertheless refrain from commenting further, since this result is also highly sensitive to the exclusion of Italy. Similarly, we note that the apparently significant interaction result with market openness in column 8 is driven almost entirely by the inclusion of the Netherlands. As such, the only clearly significant and statistically robust finding in the table is that changes in government size are associated with short-run changes in entrepreneurship, and much more so when the incumbent government does not face strong veto institutions. Yet, while the static model identifies a strongly positive effect of limited government in the long run, the ECM identifies a significantly negative effect in the short run. We note that this combination is consistent with the existence of substantial transition costs that may take the form of J-curve adaption. However, the long-run estimates from the ECM – that identify any effects occurring after the first year of an institutional change – are small and insignificant[8].

In order to gain further insight into whether the difference between short- and long-run findings is due to transitional costs or statistical problems, we test a similar ECM in Table V, but in which we estimate the determinants of two-year changes in entrepreneurship. The apparent long-run effects in Table V are therefore still a mix of medium-run effects (above two years) and “true” long-run effects while the identified short-run effects are now average consequences within the first two years after an institutional change. If the appropriate average lag structure is better approximated by applying two years as the short run, the resulting effects should therefore be more precisely estimated.

We nevertheless find somewhat weaker and often insignificant permanence of entrepreneurship as well as GDP. Conversely, the short-run effects of unemployment remain

strongly significant, as does the post-communist difference. In column 1, we again find significant and apparently contradictory short- and long-run consequences of rule of law. However, as in previous tables, these results turn out to be fragile to the exclusion of specific countries and therefore not statistically robust. In the remaining columns, we also fail to find any significant effects of institutional differences.

In summary, we here combine the study of institutional effects on entrepreneurship with new insights in institutional economics and political economy, indicating that the main institutional effects on long-run growth only appear when sufficient veto institutions are in place to make particular institutional choices permanent. We find one particular result that is both significant and statistically robust, and which exhibits systematic heterogeneity that is conditional on the strength of veto players. Before proceeding to the conclusions and a discussion of the economic significance of our findings, we provide examples of the robust result. We do so by providing conditional long-run point estimates of changing the size of government, evaluated at three levels of the strength of veto institutions.

First, the static interactions indicate that the point estimate at low levels of veto strength (around 0.2) is approximately 0.6. This indicates that with weak veto institutions, a one-standard decrease in government size – roughly the difference between present levels of government size in Denmark and Finland, or between the UK and the USA – results in an increase in entrepreneurial activity of approximately a quarter of a standard deviation, or approximately one percent of the labor force. At median levels of veto strength, this effect is cut in half while we find no significant evidence for any consequences with the very strongest veto institutions (above 0.6). As such, even within a sample of relatively homogenous countries in the OECD, we find evidence of the negative dynamic consequences of developing comprehensive welfare states that intervene directly in the economy (cf. Henrekson, 2005).

However, the positive long-run consequences at low levels of veto strength are accompanied by negative and significant, albeit small, short-run losses of about six percent of a standard deviation. While we can only speculate, one of several potential reasons for such effects to occur is that the benefits of lobbying when policies are changed are substantially larger when veto institutions subsequently can lock in the policy decisions. As such, it remains particularly uncertain whether the short-run entrepreneurial effects are productive or not.

5. Conclusions

In this paper, we combine two literatures that have so far remained distinct: the emerging literature on institutions as precedents of entrepreneurial activity and the literature on the role and importance of veto institutions. We thereby respond to one of the lacunae in the study of entrepreneurship identified by the analytical survey in Bjørnskov and Foss (2016). We first survey the two literatures and combine insights to be able to discuss what one might theoretically expect. We next test the joint influence of institutional differences and veto institutions in a panel of 30 OECD countries observed between 1993 and 2011. Our second innovation is that we provide separate tests that are likely to pick up short- and long-run consequences of institutional change.

Our overall findings on average support previous results in the study of entrepreneurship. We find that strong property rights institutions are associated with less entrepreneurial activity. While this result remains puzzling in the light of most theoretical discussions since Schumpeter, strong judicial and contract institutions as well as easy and market conformist regulations might potentially enable entrepreneurial activity within firms as an alternative to new start-ups, i.e. fostering intrapreneurship (cf. Foss and Klein, 2012). Yet, we also note that this particular result turns out to be highly sensitive to the sample composition and is often driven by the inclusion of Italy. Evidently, we cannot

resolve the puzzle of why some theoretically intuitive explanations for differences in entrepreneurial activity continue to receive only mixed and fragile support in the empirical literature.

Second, we find a strong and sizeable association with the size of government: countries with more extensive and generous welfare state institutions tend to have substantially lower levels of entrepreneurship. Contrary to the former result, the association between entrepreneurship and government size is strongly robust. We note that this particular result is entirely in line with previous studies despite our using a different measure of entrepreneurship and a somewhat different sample of countries.

The main, novel finding is the result that the effects of government size are substantially stronger when veto institutions are weak. As such, we observe that it is not only that factors such as large government consumption and heavy and progressive taxation reduce the incentives for entrepreneurial activity, but that political institutions that allow for more variable and potentially less predictable government intervention are particularly detrimental to the type of market dynamism represented by entrepreneurial start-up firms. Yet, in our short-run model, we also find that changes in government size are associated with larger entrepreneurial responses when veto institutions are weak: reductions in the size of government tend to lead to fewer start-ups in the short run when veto institutions are weak while we cannot observe any short-run effects when veto institutions are strong.

Our results thus support well-known findings while providing new qualifications. Although we can only speculate about the detailed mechanisms behind the empirical patterns that we observe, we believe that they bear at least two implications for future research. First, the variability of institutions appears important. While this may indicate that uncertainty as to whether institutional changes are permanent or not is important to entrepreneurial decisions, our short-run findings are inconsistent with a simple view that more stability (imposed by veto players) is always desirable. It also begs the question if the particular decisions that affect the overall size of government are qualitatively different when veto institutions are stronger. In addition, we note that our main findings reflect a problematic situation that is well known in the study of public choice and political economy when the short-run consequences of policy changes are the exact opposite of their long-run effects. In particular, such situations often lead to Tullock's (1975) transitional gains trap, where short-run costs prevent myopic governments from introducing reforms with substantial long-run benefits. We also note the possibility that part of the increased short-run entrepreneurial activity when government increases its economic influence despite strong veto institutions may reflect increased lobbying and other rent-seeking activity.

Second, consistent with many studies, we find no clear short- or long-run effects of regulatory differences across OECD countries. A tradition from Schumpeter nonetheless argues that regulations provide entry barriers that theoretically ought to affect the rate of start-ups as well as the character of entrepreneurial activity. As such, our findings again highlight that existing regulatory indicators may not capture relevant factors, and may not properly reflect the extent to which *de iure* regulations are actually enforced.

Clearly, as emphasized by Bjørnskov and Foss (2016), the study of institutional determinants of entrepreneurship remains in its infancy and most studies – the present included – raise more questions than they answer. Yet, at another level, our paper follows a long line of studies in public choice and political economy that document the perils of large and interventionist government. Theoretically, large government could reduce growth and activity by both reflecting rent-seeking policies, simple crowding-out of private activity or increased policy uncertainty. Our study suggests that government interventions may be less unproductive when strong veto institutions stabilize policy and reduce policy uncertainty.

Notes

1. Using the COMPENDIA data set, one must first accept that these measures do not capture entrepreneurship within existing firms (i.e. intrapreneurship). Second, the choice of this particular data set limits the sample to 30 countries. However, no other data set effectively captures intrapreneurship and other sources do not provide data based on identical or comparable definitions.
2. Some lobbyists and other destructive entrepreneurs can of course be self-employed, yet we do not think that this is a major problem. The cumulative file of the European Social Survey (2017) includes more than 30,000 self-employed respondents across the seven waves and 32 countries. Considering that entrepreneurs can potentially belong to three different NACE categories (70, 74, and 94), there are fewer than 2,000 self-employed that may be considered lobbyists or otherwise rent-seeking self-employed.
3. The only available alternative with some coverage across time is the survey data set from the Global Entrepreneurship Monitor. This data set has been used in previous studies but only covers a sizeable sample from the mid-2000s. Yet, we note that the self-employment measure from COMPENDIA and entrepreneurship measures from the Global Entrepreneurship Monitor appear to be affected by very similar institutional characteristics (cf. Bjørnskov and Foss, 2008; Nyström, 2008).
4. Although the Fraser Institute's Economic Freedom of the World index (Gwartney *et al.*, 2016) has evolved to be the standard measure, the Heritage data have the advantage of being available on an annual basis since 1993 (except labor freedom, which is available since 2003). The Fraser data are only annually available since 2000. However, imputing the missing years of data in the Fraser index yields relatively similar results (available upon request).
5. Tsebelis (2002) assesses that the PolCon variable is conceptually very closely correlated with a simpler count of institutional veto players. Yet, count alternatives such as Keefer and Stasavage's (2003) "checks" variable can be criticized for not assessing the polarization of veto players, i.e. the willingness to veto decisions.
6. By including annual fixed effects, we run the risk of under-estimating the true effects of economic freedom when changes are spatially correlated. However, the efficiency benefits of including them are arguably greater than the opposite case (Plümper *et al.*, 2005). Recent research in Hall *et al.* (2016) also finds no evidence of spatial correlation across US states.
7. In addition, with regard to the dependent variable, varying specifications of Fischer's test identify a common macroeconomic change in the years around the dot-com bubble from 1997 to 2000. A split shows that no unit roots are found for the variable if the test is made separately for the years of 1993-1999 and 2000-2011. This questions if self-employment is non-stationary. The common change can thus be accounted for by the inclusion of year dummies that capture common shocks, such that there is less concern for co-integrated relationships.
8. In addition to the estimates reported here, we have also rerun all specifications with the aggregate index of economic freedom. We find results that are never significant, but relatively similar to those obtained for the regulatory component. Conversely, we find very similar results when we test the general robustness to using a dichotomous version of the political constraints index, as well as when we replace it with a dummy for bicameral political institutions. These results are available upon request.

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