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Barriers to prosperity: the harmful impact of entry regulations on income inequality

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Abstract Entry regulations, including fees, permits and licenses, can make it prohibitively difficult for low-income individuals to establish footholds in many industries, even at the entry-level. As such, these regulations increase income inequality by either preventing access to higher paying professions or imposing costs on individuals choosing to enter illegally and provide unlicensed services. To estimate this relationship empirically, we combine entry regulations data from the World Bank's Doing Business Index with various measures of income inequality, including Gini coefficients and income shares to form a panel of 115 countries. We find that countries with more stringent entry regulations tend to experience more income inequality. In countries with average inequality, increasing the number of procedures required to start a new business by one standard deviation is associated with a 7.2% increase in the share of income accruing to the top decile of earners, and a 12.9% increase in the overall Gini coefficient. This result is robust to the measure of inequality, startup regulations, and potential endogeneity. We conclude by offering several policy recommendations designed to minimize the adverse effects of entry regulations.

Keywords Income inequality · Regulation · Entry regulations · Doing business · Gini coefficient

JEL Classification D31 · J38 · K20

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1 Introduction

Entry regulations, which include fees, permits, occupational licensure and educational requirements, inhibit the ability of people to start new businesses or enter specific trades or professions. Entry regulations especially can limit labor market opportunities for low-income earners. In the United States, for example, licensing requirements may make entry prohibitively challenging for workers in entry-level occupations such as bus driving, cosmetology and pest control (de Rugy 2014).[1] As such, these regulations act as costly barriers to entry that disproportionately affect low-income entrepreneurs and workers.

Would-be providers unable to afford entry are faced with two undesirable choices: abandon their occupation or business choice in favor of a less preferred and likely less remunerative alternative, or enter the industry surreptitiously and operate illegally.[2] Those who choose to enter a market illegally face the risk of both civil and criminal sanctions, including prosecution, fines, asset and property forfeiture/seizure, and imprisonment. Moreover, illegal entrants must waste real resources in order to evade detection and enforce contracts outside normal judicial channels (i.e., civil court). Finally, given that most consumers are risk averse and perceive licensing as indicative of provider quality, the artificially limited number of licensed providers can command premiums for their services. Comparing the wages of licensed and unlicensed workers across the United States using the National Longitudinal Survey of Youth, Gittleman and Kleiner (2016) find that holding an occupational license is associated with higher pay. Indeed, recent studies estimate that occupational licensing raises the wages of licensed workers by between 15 and 19% (see Kleiner and Krueger 2010, 2013). Consequently, regardless of a would-be entrant's choice to abandon their first-best occupational choice or operate illegally, the result is lower income for the affected individual and greater overall income inequality for society.

Indeed, economists have long recognized that organized special interest groups are quite effective in securing favorable regulations from government, which are at variance with the interests of consumers and would-be competitors (e.g., Olson 1965; Stigler 1971). In exchange for campaign contributions and other political payoffs, Peltzman (1976) predicts that self-interested regulators will draft rules favorable to special interest groups, including industry lobbyists seeking to erect barriers to entry. Once in place, such restrictions (which include occupational licensure and other business startup regulations), enable existing producers to restrict supply, create monopoly rents, and maximize their profits and incomes (see, for example, Friedman 1962). Moreover, incumbent producers, protected from new competition by entry regulations, have weaker incentives to provide higher-quality service, despite claims to the contrary by regulation's advocates (who instead emphasize that regulations weed-out unqualified providers).[3] For example, Carroll and Gaston (1981) find evidence that restrictive licensing of electricians does lower the quality of service. They also discover an unfortunate unintended consequence: a positive relationship between the licensing of electricians and the rate death rate from accidental electrocutions across states

[1] For example, to obtain a license to offer hair-braiding services legally in Pennsylvania, a person must train for 300 h at a licensed school, have a minimum of a tenth-grade education, and pass both a theory and a practical exam (McLaughlin 2013).

[2] While the impact of government regulations on the size of the shadow economy and overall income inequality (including income earned in both the formal and informal sectors) is a very interesting topic, data limitations prevent further exploration. The best available data (see Dreher et al. 2014; Schneider 2005) covers the very limited time span of 1999–2003.

[3] For a literature review of the early empirical research surrounding the relationship between occupational licensing and quality, see Gross (1986).

because homeowners do electrical work themselves rather than hiring a professional. More recently, Carpenter (2012) finds little difference between the quality of floral arrangements in Louisiana, where florists are licensed, and Texas, where florists are not licensed. McLaughlin et al. (2014) review 16 empirical studies of the effects of licensing on service quality and find only three studies that report a positive correlation between licensing and quality, whereas 13 studies report neutral or negative correlations or find mixed or unclear results.[4]

While many of the economic effects of political rent-seeking are well documented, the impact of such behavior on a nation's income distribution is less well understood. Stigler (1970) observes that politically dominant groups use the coercive powers of the state to redistribute resources to their own advantage. Consistent with Director's Law, Stigler (1970) demonstrates that the dominant group in the United States has been the middle class throughout the nineteenth and twentieth centuries.[5] Because both the poor and wealthy are affected negatively by redistribution, the overall impact on income inequality is ambiguous. Furthermore, Stigler (1970) postulates that in the long-run, as income becomes easier to track (and tax) and voting impediments disadvantaging low-income people disappear, the dominant political group will become the poor, who will use their collective political power to benefit themselves by transferring income away from wealthy and middle-class households. Although the plausibility of this long-run prediction is debatable in light of the findings of Olson (1965), Stigler (1971) and Peltzman (1976), the lack of a clear-cut prediction underscores the need for empirical evidence on the relationship between startup regulations and income inequality.

Despite the plausible yet ambiguous connection between the extent of a nation's entry regulations and inequality, we are unaware of any empirical studies of this important topic. Until recently, scholarly neglect likely reflected the unavailability of data on business startup regulations. However, the World Bank's Doing Business dataset, which includes information on the number of regulatory procedures, cost and length of time required to start a new business in 211 nations between 2004 and 2016, permits empirical studies of the relationship between entry regulations and income inequality across countries and over time.

This paper fills the gap in the literature by estimating the impact of startup regulations on income inequality empirically. We find that a larger number of steps required to open a business is associated with greater income inequality. Specifically, we find that in countries with income inequality measures equal to the sample mean, increasing the number of procedures required to start a new business by one standard deviation causes a 7.2% increase in the share of income accruing to the top decile of income earners, and an 12.9% increase in the overall Gini coefficient. Those results are robust to both the measurement of startup regulations and income inequality.

Furthermore, following La Porta et al. (1998) and Djankov et al. (2006), we instrument entry regulations with the country's legal origin, principal religion, percentage of English

[4] McLaughlin et al. (2014) review three studies that observe a positive relationship between licensing and service quality. Feldman and Begun (1985) find that occupational restrictions in optometry improve the quality of eye examinations, Martin (1982) discovers a positive correlation between reciprocal licensing and quality, and Holen (1978) finds that entry requirements for dentists are associated with a lower rate of dental-health neglect. The authors reviewed 13 studies that observe a neutral or negative relationship between licensing and quality, including Carroll and Gaston's (1981) study of electricians.

[5] Stigler (1970) provides several examples of regressive public policy, including the provision of higher education, fire and police services, farm policy, minimum wage rules, Social Security, public housing and tax exemptions for charitable contributions.

speakers, initial GDP per capita, and absolute global latitude. As noted in Djankov et al. (2006), these variables perform well as instruments because they help define many "substantive and procedural aspects" of both law and regulation. It follows that the same variables also plausibly explain the number and complexity of country-level entry regulations. And because legal origins, as well as the cultural and geographic variables, were established centuries ago, we can reasonably assume no direct link to changes in income inequality in the recent time period we examine. This two-stage instrumental variables approach permits us to infer a causal relationship by ruling out potentially endogenous factors.

The remainder of this paper proceeds as follows. First, we review the literature pertaining to the empirical determinants of income inequality. Next, we describe the data and estimate the relationship between entry regulations and income inequality. After confirming the robustness of our findings, especially with respect to potentially endogenous factors, we then discuss policy implications, followed by the conclusion.

2 Determinants of income inequality

Researchers have explored the impact of entry regulations on income inequality within specific occupations. For example, Kleiner and Park (2010) demonstrate empirically that income inequality between dentists and hygienists declines in states where dental hygienists are allowed to be self-employed. However, we are unaware of any empirical studies that examine the broader impact of entry regulations on overall income inequality within a panel of countries. Before specifying our model, we discuss other plausible determinants of income inequality identified in the existing literature and construct a consensus model incorporating these variables as controls.

Apart from Stigler (1970), research directly examining the relationship between regulations (regardless of type) and income inequality is sparse. A notable exception is Shughart et al. (2003), which finds that (all else equal) Gini coefficients are higher in states with more influential special interest groups. Thus, the rent seeking behavior of interest-groups, which includes lobbying for new regulations, is empirically associated with higher income inequality. That said, some scholars have focused on a related topic: the relationship between economic freedom and income inequality. Carter (2007) finds a positive relationship between economic freedom and income inequality using data from the United Nations University World Institute for Development Economics Research (UNU-WIDER) World Income Inequality Database[6] and the *Economic Freedom of the World Annual Report*,[7] which incorporates an index of regulatory freedom. Carter emphasizes that theory does not give us a clear idea of the effect of economic freedom on income inequality. While economic freedom may provide more opportunities for upward mobility, it is also true that economically free nations have the lowest levels of income redistribution. Moreover, consistent with Sigler (1970), economic freedom may enable dominant coalitions of voters to expropriate resources from the rest of society, thereby raising or lowering income inequality, depending on the group's relative position in the nation's distribution of income.

[6] The database can be accessed at https://www.wider.unu.edu/project/wiid-world-income-inequality-database.

[7] The reports and datasets are available from the Fraser Institute at http://www.freetheworld.com/reports.html.

Another paper related to our topic is Calderón et al. (2004), which explores the relationship between labor market regulations and income inequality. Specifically, the authors examine cross-country data on statutory (de jure) regulations and de facto regulations that are put into practice and enforced administratively. They find that de facto labor market regulations are associated with reductions in income inequality. However, that relationship is weak; moreover, and Calderón and his coauthors find no evidence that de jure regulations affect income inequality. They also look at specific labor market regulations such as the minimum wage, union membership, and regulations surrounding the workplace environment and find that de facto labor regulations are associated with reductions in income inequality.

Apart from entry regulations, the literature identifies many potential determinants of income inequality. The oldest and probably most widely employed one is the relationship between economic development and inequality identified by Kuznets (1955), whereby income inequality rises during the early stages of economic development and eventually declines as countries become richer. Barro (2000) finds empirical support for the Kuznets hypothesis and shows that more inequality slows growth in poor countries and encourages growth in rich countries. However, Barro points out that rates of growth do not explain much of the variation in inequality across countries. Notably, Barro (2000) also includes measures of human capital (specifically years of primary, secondary and higher education), trade openness, democracy, and rule of law as additional control variables in his inequality regression models.

Researchers also have focused on the relationship between components of economic and political freedom and income inequality other than regulations, including political openness, trade openness and financial market development. Subrick (2007) reports evidence that financial development and openness to trade reduces income inequality. Mahler and McKeever (2009) find evidence that trade (as measured by the KOF Index of Globalization)[8] exacerbates income inequality (as measured by the Gini coefficient), when controlling for varying levels of economic development, ethnic fractionalization, political democracy, and government expenditures on education. To measure the direct effect of ethnic fractionalization on inequality, Mahler and McKeever (2009) use an index compiled by Fearon (2003) that attempts to measure a country's ethnic homogeneity; they report a strong positive relationship between ethnic fractionalization and both after-tax and before-tax inequality. Mahler and McKeever (2009) argue that ethnically heterogeneous countries have more difficulty redistributing income than their more homogeneous counterparts.

Taken together, these findings suggest that economic development, human capital, international trade, access to credit, political openness and ethnic heterogeneity are important determinants of income inequality. Therefore, we enter these control variables when estimating the relationship between entry regulations and income inequality in Sect. 4.

[8] The KOF Index of Globalization measures globalization along three dimensions (economic, social, and political). The data and a description of the methodology used in its construction are available at http://globalization.kof.ethz.ch/.

3 Data

For our analysis, we adopt two measures of income inequality. The first is the share of national income accruing to the top 10% of earners, published in the World Bank's World Development Indicator (WDI) database.[9] The World Bank bases its estimates primarily on household survey data obtained from the Luxembourg Income Study database and governmental statistical agencies.[10] As an alternative measure of income inequality, we also use the Gini coefficient, a popular measure of a country's income distribution that ranges in value from 0 (complete equality) to 100 (total inequality).[11] The data for the Gini coefficient come from the All the Ginis (ATG) database, which compiles a large number of observations, calculated exclusively from household survey data, from nine primary sources, including the Luxembourg Income Study (LIS) and the World Income Inequality Database (WIID).[12] The ATG combines these disparate data sources to construct a relatively consistent unbalanced panel of Gini coefficients (labeled "giniall"), which we use as our preferred measure of inequality.

Our measure of entry regulations comes from the World Bank's Doing Business dataset.[13] The dataset includes variables that measure the easiness of doing business, including the number of regulatory procedures and the length of time required to start a new business; it and covers 211 nations (and regions) spanning the period from 2004 to 2016. The World Bank's procedural startup steps are defined as any interaction between an entrepreneur and outside parties required to start a business legally, ranging in value from as few as one to 21 in the dataset. The Doing Business dataset exhibits significant variation across countries in numbers of requirements, time and cost required to formally open a business. For example, in 2004, an entrepreneur seeking to start a new business in Colombia needed, on average, to complete 19 steps, to spend 28.0% of his or her income, and to wait 43 days. Such delays are far from benign, and can block legitimate entry to the formal economy. Worryingly, de Soto (1989) notes that the growth of the underground economy can be fertile ground for organized criminals and terrorists. By contrast, in the same year, an entrepreneur wanting to open a new business in the United States needed to complete only six steps, spend 0.7% if his or her income, and wait 6 days.

In addition to the variables of interest, we enter control variables commonly used in the literature, including total education, credit market development, trade openness, log per capita real GDP (chained, PPP-adjusted 2011 US dollars), democratization and ethnic heterogeneity. The observations on total education come from the Barro and Lee (2013) dataset, and are measured as the average number of years of total schooling for the population above age 15.[14] Because the Barro and Lee dataset reports educational attainment at 5-year intervals (i.e., 1950, 1955, …, 2010), we replace the missing observations with the closest prior observation (e.g., we use the 2010 educational observation to

[9] The WDI income share data are available at http://data.worldbank.org/indicator/SI.DST.10TH.10.

[10] Despite the care taken to produce a comparable income share series, the data are not perfectly compatible across countries, as nations use different survey designs and measure different welfare concepts (i.e., income versus expenditures). As there is no systemic relationship between income and expenditure shares, we follow Lakner and Milanovic (2013) and make no adjustment for this difference.

[11] The Gini coefficient is equivalent to twice the area between the Lorenz curve and the line of perfect equality.

[12] The ATG dataset is available at http://go.worldbank.org/9VCQW66LA0.

[13] The Doing Business database is available at http://www.doingbusiness.org/data.

[14] The Barro-Lee dataset is available at: http://www.barrolee.com.

fill-in the missing values for 2011 through 2014). Data for credit market development comes from the World Bank World Development Indicators database,[15] and measures domestic credit supplied to the private sector as a percentage of GDP. The data for trade openness come from the World Bank World Development Indicators database, and is calculated as the sum of exports and imports expressed as a fraction of GDP. Data for PPP-adjusted real GDP per capita come from the Penn World Table 9.0 (see Feenstra et al. 2015).[16] Observations on democratization are taken from the Freedom House's ratings of civil liberties (Freedom House 2014). The ratings range from 1 to 7, where 1 represents the highest level of civil liberties and 7 represents the lowest level of civil liberties. Lastly, ethnic fractionalization data come from an indicator compiled by Fearon (2003), who quantifies ethnic heterogeneity across countries. That variable ranges from 0 to 1: a higher value represents more ethnic fractionalization.[17] Descriptions of these variables, along with their summary statistics in the combined dataset, are provided in Table 1.

The top income decile series ranges in value from 20.14% (Slovenia in 2008) to 54.25% (South Africa in 2006), with an overall average of 29.73%, and a standard deviation of seven percentage points. As seen in Fig. 1, the distribution is skewed heavily to the right, with a large proportion of nations possessing top deciles above the sample average. Overall, the top decile series comprises 611 observations, covering 118 countries and spanning the 2004–2013 period. Table 2 lists the countries and time periods included in the top decile panel.

As for the Gini coefficient, the average value is 39.16, with a standard deviation of 9.38. As with the top deciles, the same nations claim the largest and smallest values in our combined dataset. The largest value belongs to South Africa (2008), at 69.80, while the smallest belongs to Slovenia (2008), equaling 23.10. Like the top income decile shares, the empirical (kernel density) distribution of the Gini coefficient is highly skewed (see Fig. 1). The Gini coefficient series is 16% smaller than the top income decile series, consisting of 513 observations, covering 118 countries and spanning the years from 2004 to 2012.

The entry regulations data from the World Bank's Doing Business database indicate that the average number of steps required to start a new business is just under nine (8.55), with a standard deviation just above three (3.37). Colombia required the most steps (19) to open a business in 2004. Ugandan entrepreneurs faced the second-largest number of steps to open their businesses (18 from 2004 to 2009). When alternatively measuring startup barriers by the time needed to start a business legally (summary statistics not reported in Table 1), a similar pattern of extreme values emerges (see Fig. 1 for kernel density estimates of the distribution of these startup measures). New Zealanders could open a business in just half a day between 2009 and 2012, compared to a staggering 260 days in Haiti in 2004. Encouragingly, some nations have improved significantly over the sample period. For example, in 2004, it took 168 days to open a business in Indonesia, but by 2016 that number had dropped to 46.5 days.

[15] The data are from the World Development Indicators database at http://data.worldbank.org/data-catalog/world-development-indicators.

[16] The Penn World 9.0 dataset is available at: http://www.ggdc.net/pwt.

[17] The observations on ethnic fractionalization are reported for the early 1900 s only, so we employ those measures under the assumption that ethnic fractionalization is slow to change.

Table 1 Descriptions of variables

Variable	Description	Mean	SD	Minimum	Maximum
Top decile	Share of income going to the top 10% of earners	29.73	7.00	20.14	54.25
Gini	Gini coefficient	39.16	9.38	23.10	69.80
Startup steps	Number of procedures required to start a new business	8.55	3.37	1.00	19.00
Education	Average total years of education of population above age 15	8.09	2.90	1.08	13.18
Credit	Domestic credit to private sector (% of GDP)	57.11	49.28	0.19	312.15
Trade	The sum of exports and imports (% of GDP)	89.98	52.85	19.12	439.66
Log GDP	Purchasing power parity–adjusted real GDP per capita	9.07	1.20	6.20	11.46
Democracy	Rating of civil liberties that ranges from 1 (best) to 7 (worst)	3.01	1.68	1.00	7.00
Ethnic	Increasing measure of ethnic heterogeneity ranging from 0 to 1	0.44	0.25	0.01	0.95
French legal origin	Dummy variable equal to 1 if nation's legal origins are French	0.54	0.50	0.00	1.00
German legal origin	Dummy variable equal to 1 if nation's legal origins are German	0.13	0.33	0.00	1.00
UK legal origin	Dummy variable equal to 1 if nation's legal origins are British	0.29	0.45	0.00	1.00
Absolute Latitude	Distance from the equator as measured by absolute latitude	28.10	17.72	0.02	64.96
Muslim	Predominant religion is Islam	0.21	0.41	0.00	1.00
Protestant	Predominant religion is Protestantism	0.17	0.37	0.00	1.00
Catholic	Predominant religion is Catholicism	0.35	0.48	0.00	1.00
English language	The proportion of residents who are English speakers	6.33	21.91	0.00	98.41

4 Baseline model and results

4.1 OLS estimation of baseline model

To determine if entry regulations are associated with greater income inequality, we first regress top income shares on the number of business startup steps, period fixed effects and various combinations of the control variables, i.e., total education, credit, trade openness, log GDP, log GDP squared (to capture any Kuznets's curve relationship), democracy and ethnic heterogeneity:

$$Inequality_{it} = a + \delta_t + \beta \cdot Steps_{it} + X_{it}B + u_{it}, \quad (1)$$

where i is the cross-sectional country index, t is the time index, δ_t is time fixed effects, $Steps_{it}$ is the required number of business startup steps, X_{it} is a matrix of control variables,

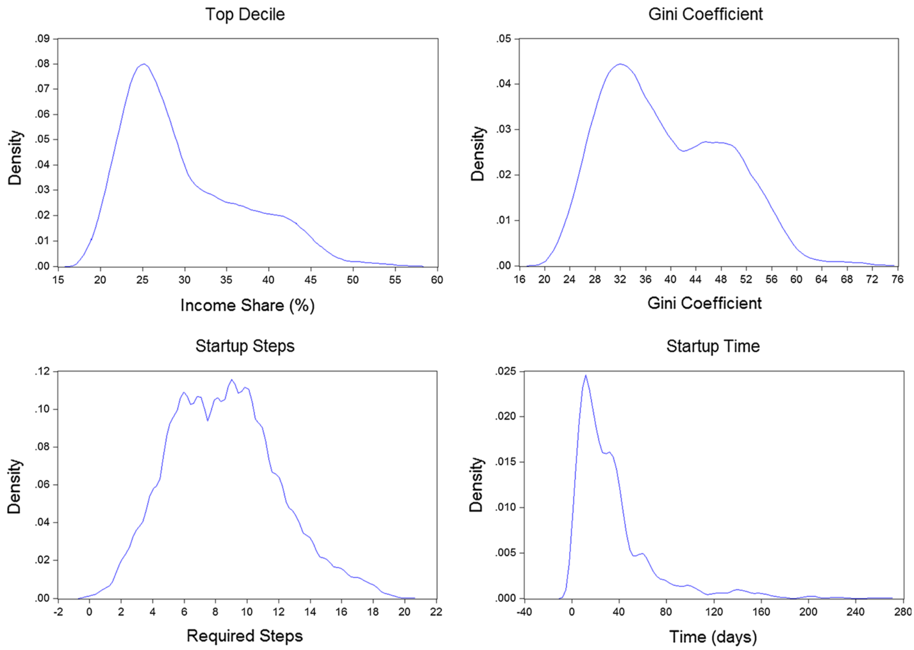

Fig. 1 Empirical kernel density distributions. *Note* Epanechnikov kernel density estimator of pooled cross sectional data

and u_{it} is a mean zero error term.[18] The estimation results for Eq. (1) are reported in Table 3 with White robust standard errors.

Both trade and credit are statistically insignificant in every version of Eq. (1) in which they appear. With the exception of the democracy variable, the remaining control variables are both statistically significant and consistent with a priori sign expectations.[19] The coefficient estimates of total education are universally negative and statistically significant, suggesting that increases in the population's average educational attainment is associated with declining income inequality. Specifically, increasing average educational attainment by 1 year reduces the share of national income received by the top decile by between 1.11 and 1.35 percentage points. In the variations of Eq. (1) in which they are both entered (i.e., versions 6 through 8), Log GDP and its square are statistically significant at the 1% level and carry signs consistent with Kuznets's hypothesis (i.e., an inverted "U" shape), with an

[18] Because fewer than three observations are available on so many nations in our unbalanced panel, we cannot estimate model Eq. (1) with country-level (cross-sectional) fixed effects (α_i), as doing so would effectively dummy-out most of the variation in the dependent variable.

[19] To ensure that this result is not idiosyncratic to Freedom House's measure of democracy, we also re-estimate columns 7 and 8 from Table 3 using an alternative measure of democracy, the Polity IV (2016) panel, which reflects six institutional/governmental qualities, such as political competition and executive authority, for up to 167 countries between 1800 and 2016. The index ranges in value from − 10 (monarchy) to + 10 (consolidated democracy). The estimation results are virtually identical across all of the models' covariates, with positive and statistically significant coefficients on the polity measure of democracy. Like the Freedom House measure, these results suggest that more authoritarian nations exhibit less income inequality, all else equal. The Polity-based estimates are not reported, but are available from the authors upon request.

Table 2 Countries included in top decile panel

Country	Observations	Country	Observations	Country	Observations
Albania	3	Honduras	10	Pakistan	4
Argentina	10	Hungary	9	Panama	10
Armenia	10	Iceland	9	Paraguay	10
Australia	2	India	3	Peru	10
Austria	9	Indonesia	3	Philippines	3
Bangladesh	2	Iran	3	Poland	9
Belgium	9	Iraq	1	Portugal	9
Benin	1	Ireland	9	Romania	9
Bolivia	9	Israel	3	Russia	9
Botswana	1	Italy	9	Rwanda	2
Brazil	9	Ivory Coast	1	Senegal	2
Bulgaria	7	Jamaica	1	Serbia	7
Burundi	1	Japan	1	Sierra Leone	1
Cambodia	7	Jordan	3	Slovakia	9
Cameroon	1	Kazakhstan	10	Slovenia	9
Canada	3	Kenya	1	South Africa	3
Central African Rep.	1	Kyrgyzstan	9	Spain	9
Chile	4	Laos	2	Sri Lanka	3
China	3	Latvia	9	Sudan	1
Colombia	10	Lesotho	1	Swaziland	1
Congo, Republic of	2	Liberia	1	Sweden	9
Costa Rica	10	Lithuania	9	Switzerland	6
Croatia	5	Luxembourg	9	Syria	1
Cyprus	9	Malawi	2	Tajikistan	3
Czech Republic	9	Malaysia	3	Tanzania	2
Denmark	9	Maldives	2	Thailand	8
Dominican Republic	10	Mali	2	Togo	2
Ecuador	10	Mauritania	2	Tunisia	2
Egypt	1	Mauritius	2	Turkey	9
El Salvador	10	Mexico	6	Uganda	3
Estonia	9	Moldova	10	Ukraine	10
Fiji	1	Mongolia	4	United Kingdom	9
Finland	9	Morocco	1	United States	4
France	8	Mozambique	1	Uruguay	10
Gabon	1	Namibia	1	Venezuela	3
Germany	6	Nepal	1	Vietnam	5
Ghana	1	Netherlands	9	Yemen	1
Greece	9	Nicaragua	2	Zambia	3
Guatemala	3	Niger	3		
Haiti	1	Norway	9		

Table 3 OLS estimates of baseline model

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Startup steps	0.9981***	0.6134***	0.6040***	0.5568***	0.5554***	0.4289**	0.4298***	0.3289**
	(0.1601)	(0.1638)	(0.1745)	(0.174)	(0.1751)	(0.1679)	(0.1638)	(0.1452)
Education		− 1.1988***	− 1.1642***	− 1.1238***	− 1.1114***	− 1.3288***	− 1.3455***	− 1.3457***
		(0.2403)	(0.2476)	(0.2506)	(0.3014)	(0.3221)	(0.3222)	(0.3311)
Credit			− 0.0030	− 0.0038	− 0.0034	0.0115	0.0115	0.0172
			(0.0094)	(0.0093)	(0.0116)	(0.013)	(0.0131)	(0.0131)
Trade				− 0.0138	− 0.0139	− 0.005	− 0.005	− 0.0178
				(0.0115)	(0.0114)	(0.0133)	(0.014)	(0.0141)
ln(GDPC)					− 0.0536	28.3639***	30.1996***	38.5889***
					(0.7981)	(8.1899)	(8.478)	(8.2729)
$\ln(GDPC)^2$						− 1.5737***	− 1.7156***	− 2.1845***
						(0.4613)	(0.4897)	(0.4719)
Democracy							− 0.7429	− 1.0348**
							(0.4616)	(0.4818)
Ethnic								6.3293**
								(2.6512)
R^2	0.2484	0.3778	0.3709	0.3779	0.3780	0.4346	0.4476	0.4986
Observations	600	600	588	588	588	588	586	557

Dependent variable is the share of national income held by the top decile of households

Intercept and time period fixed effects included in each model but not reported

White robust period standard errors in parenthesis; ***, **, and * denote 1, 5, and 10% statistical significance respectively average turning point of $7146 PPP-adjusted 2011 dollars.[20] In the one variant of Eq. (1) containing ethnic heterogeneity, the estimated coefficient is positive and statistically significant at the 5% level, which is consistent with the findings of Mahler and McKeever (2009) and Subrick (2007), who argue that heterogeneous countries find it more difficult to redistribute income than homogeneous countries owing to the emergence of ethnic special interest groups.

Focusing on the variable of interest, the estimated coefficients on number of required business startup steps are positive and statistically significant at either the 1 or 5% level of significance in all eight variants of Eq. (1). With coefficient estimates ranging in value from 0.9981 to 0.3289, the overall mean coefficient value of startup steps (averaging across all eight variants of Eq. 1) is 0.5644. That estimate implies that adding one additional startup requirement increases the share of national income accruing to the top 10% of households by just over one-half of a percentage point. To put the number into perspective, in nations with an average level of income inequality (i.e., the top decile receives 29.73% of national income), a one standard deviation increase in startup requirements (3.37 steps) will in turn raise the share of income accruing to the top income decile by 1.9 percentage points ($3.37 \times 0.5644 = 1.90$) or by 6.4% ($1.90/29.73 = 0.064$).

Although these initial results are promising, we cannot rule out the possibility of an endogenous relationship between income inequality and business startup regulations. Specifically, if startup requirements and other barriers to entry result in economic rents accruing to the wealthiest (and presumably most powerful) members of society, then greater income concentration at the top may result in the adoption of even more startup regulations. If such cronyism/corruption exists, one cannot treat startup regulations as strictly exogenous. Before re-estimating the model using two-stage least squares (TSLS), we can informally test for systematic differences in the impact of startup regulations on income inequality. That is, do startup regulations have a greater impact on income inequality in nations with low inequality than in nations with high inequality? If inequality and startup regulations are endogenously related as just described, then one would expect additional regulations to have a more pernicious effect (at the margin) in countries with substantial income inequality, as the purpose of such regulations would be to generate larger economic rents for existing business owners and other elites. Using the preferred specification of Eq. (1)—i.e., column 8 of Table 3—we employ quantile regression techniques to re-estimate the model at various conditional percentiles (known as quantiles) of the dependent variable (top income share). Figure 2 plots the startup step coefficient estimates for countries with varying levels of income inequality that span the interquartile range of income inequality (i.e., 25th percentile through the 75th percentile), along with 95% confidence intervals. Clearly, little variation is evident in the magnitudes of the coefficient estimates, suggesting that the inequality-promoting effect of business startup regulations does not vary much between low- and high-inequality countries. To test this hypothesis formally, we employ the quantile slope equality test of Koenker and Bassett (1982), in which the equality of the low (0.25) and median quantiles are tested jointly with the equality of the median and high (0.75) quantiles. The resulting Wald test statistic under the null hypothesis of coefficient equality across all quantiles equals 0.2463 (with two degrees of freedom), with a corresponding p value of 0.88; the null thus cannot be rejected. This test result, combined with the visual evidence from Fig. 2, suggests strongly that the

[20] The average coefficient on Log GDP is 32.3841 and the average coefficient on Log GDP squared is $-$ 1.8246. The turning point of the parabola thus is $-32.3841/(2 \times (-1.8246)) = 8.8743$ or $7146 (Exp[8.8743]).

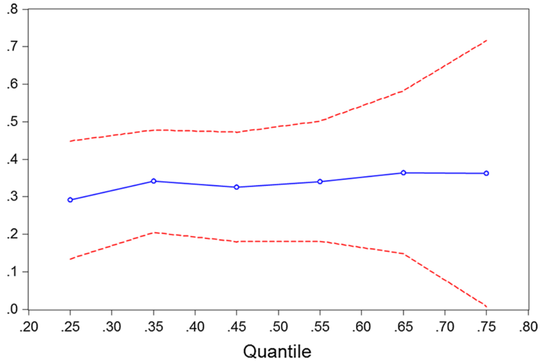

Fig. 2 Baseline model quantile regression estimates of startup step coefficients with 95% confidence intervals

impact of startup regulations on income inequality does not vary between low and high inequality nations.

4.2 Two stage least squares estimation of baseline model

Out of an abundance of caution, we re-estimate Eq. (1) by TSLS. Fortunately, the literature documents several regulatory instruments suitable for our purposes. In their pioneering work, La Porta et al. (1998) demonstrate that a nation's legal origin is a strong predictor of protections for shareholders, with nations of French legal origin offering the weakest investor protections and nations with English origins offering the strongest protections. Thus, the quality of civil law in general, and financial regulation in particular, is influenced by a nation's legal origin. Geography is another important factor, as it is correlated with many economic, political and historical factors, making it an attractive instrument for regulation. Measured by distance from the equator, many researchers have noted that economic development and growth are positively correlated with absolute latitude (see Andersen et al. 2016; Hsiang and Meng 2015; Hernando 2012; Ram 1997; and Theil and Galvez 1995, among others). Other important historical and institutional factors related to absolute latitude include colonial history (Ertan et al. 2016), ethnic fractionalization (Campos et al. 2011) and corruption (Delavallade 2006). The primary religion of a nation is also linked closely to its history, culture and legal framework. Regarding the use of religion as an instrument for regulations, researchers have found that religion influences both labor regulations (Domenech 2011; Algan and Cahuc 2006) and social trust (Addai et al. 2013; Leon and Pfeifer 2013). Finally, more economically developed nations often have larger governments, bureaucracies and regulatory systems. Nicoletti and Scarpetta (2003) note the buildup of regulations in OECD nations, and their probable role in the slow growth of many large, continental European economies. Initial real per capita output, therefore, is likely correlated with the size and complexity of a nation's regulatory system.

We follow Djankov et al. (2006) by instrumenting business startup regulations with initial log GDP per capita, absolute latitude, dummy variables for the primary religion, the proportion of residents who are English speakers and the origin of the nation's civil and criminal laws. Collectively, these instruments have the advantage of being either predetermined (initial output, legal origins and absolute latitude) or persistent through time (language and religion), meaning that they can be safely treated as exogenous. Moreover, these variables are highly relevant predictors of the regulatory environment because they capture aspects of a nation's culture, geography and the scope and procedural complexity of the legal code (which is closely related to administrative law and regulatory rule creation). As such, they are good instruments for capturing cross-sectional variation in startup regulations that are orthogonal to income inequality shocks.

Before discussing the TSLS results, it is instructive to examine the first-stage regressions in which the suspected endogenous variable (startup regulations) is regressed on the above set of instruments and the remaining exogenous right-hand-side independent variables from Eq. (1). The results, reported in Table 4, generally conform to a priori expectations. Specifically, the coefficients on the legal origin dummies are universally positive, with the smallest magnitudes associated with English legal origin, followed by German legal origin and, finally, the largest coefficient estimates associated with French legal origin. Therefore, English (common law) origin nations are friendlier to business startup investors and would-be entrepreneurs. Geography also is important, with the regulatory burden declining as nations are located further from the tropics. The dummy variables for predominant religion universally are statistically insignificant. Not surprisingly, the coefficient on the percentage of English language speakers is negative and statistically significant throughout, as this would tend to associate with nations possessing either a classical liberal heritage and/or English common law origins (i.e., the United States, along with the United Kingdom and other British Commonwealth nations). Initial real log GDP per capita, which enters in specifications of Eq. (1) that do not include contemporaneous real output (i.e., columns 1–4), is statistically insignificant. The statistical significance of the remaining right-hand-side exogenous variables from Eq. (1) is mixed. Education, credit and ethnic fractionalization are statistically insignificant. The coefficient on trade is negative and statistically significant in every first-stage regression in which it appears. That result suggests that nations with more trade activity erect fewer regulatory barriers to business startups. Like the Kuznets curve for income inequality, an inverted "U"-shaped relationship is found between startup regulations and real output (i.e., the coefficient on log real per capita GDP is positive, while the coefficient on the square of that variable is negative). While this finding suggests that real output has a common, predictable, structural impact on the conditional mean of both regulations and income inequality, it does not disqualify real output from serving as a valid instrument, as that would require that shocks to income inequality be correlated with shocks to output. Finally, the coefficient on democracy is positive and statistically significant. Recall that democracy ranges in value from 1 to 7, with larger numbers associated with *fewer* civil liberties. Thus, a positive coefficient implies that declining civil liberties (i.e., larger democracy scores) lead to more business startup regulations.

Using the startup regulations identified above, along with the remaining right-hand-side regressors (which serve as their own instruments), Eq. (1) is re-estimated by TSLS, with the results reported in Table 5. To aid comparison with Table 3, the first eight columns of Table 5 match the control variables entered in Table 3. Overall, the estimation results are very similar. Regardless of the control variables entered, the estimated coefficients on business startup steps are positive and statistically significant, but notably larger in magnitude. The average coefficient estimate (columns 1–8) on startup steps is 2.15, which is just under four times the average coefficient value (0.5644) from Table 3. Thus, adding just one more startup requirement increases the share of national income accruing to the top 10% of households by approximately 2.15 percentage points. For nations with an average level of inequality (i.e., the top decile receives 29.73% of national income), a one-step increase in the business startup process increases the share of income accruing to the top income decile by 7.2% (2.15/29.73 = 0.072).

Table 4 Baseline TSLS model: first stage regression of startup steps on instruments

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Legal origin (French)	3.35***	3.335***	3.411***	3.051***	3.095***	2.84***	2.805***	2.819**
	(1.103)	(1.12)	(1.111)	(1.097)	(1.09)	(1.063)	(1.047)	(1.2)
Legal origin (German)	2.876***	2.848***	2.955***	2.754***	2.8***	2.691***	2.784***	2.94***
	(0.95)	(0.996)	(1.035)	(1.007)	(1.008)	(0.962)	(0.935)	(1.079)
Legal origin (UK)	2.211**	2.192**	2.273**	2.239**	2.287**	2.009**	2.056**	2.366**
	(1.026)	(1.058)	(1.03)	(1.021)	(1.015)	(0.967)	(0.946)	(1.021)
Absolute latitude	− 0.048***	− 0.049***	− 0.046**	− 0.053***	− 0.054***	− 0.044***	− 0.038**	− 0.05***
	(0.018)	(0.019)	(0.019)	(0.017)	(0.017)	(0.017)	(0.017)	(0.018)
Muslim	− 0.242	− 0.214	− 0.311	− 0.419	− 0.446	− 0.562	− 0.793	− 0.631
	(0.556)	(0.571)	(0.575)	(0.553)	(0.559)	(0.553)	(0.572)	(0.611)
Protestant	0.6499	0.6619	0.6185	0.0554	0.0375	0.3754	0.5171	0.3809
	(0.6918)	(0.6975)	(0.7156)	(0.6868)	(0.6881)	(0.6827)	(0.6805)	(0.8087)
Catholic	0.5594	0.567	0.6013	0.4282	0.3714	0.5266	0.7336	0.6109
	(0.6302)	(0.6427)	(0.6574)	(0.6076)	(0.6017)	(0.5931)	(0.6146)	(0.6783)
English language	− 0.0313***	− 0.0315***	− 0.0285***	− 0.033***	− 0.0332***	− 0.029***	− 0.0283***	− 0.0302***
	(0.0106)	(0.0105)	(0.0099)	(0.0106)	(0.0107)	(0.0106)	(0.0103)	(0.0116)
Initial GDP	− 0.4192	− 0.4487	− 0.4275	− 0.27	–	–	–	–
	(0.298)	(0.3663)	(0.4551)	(0.4125)				
Education		0.0212	0.0018	0.0453	0.0249	− 0.0598	− 0.0562	− 0.0762
		(0.1287)	(0.1294)	(0.1294)	(0.1326)	(0.1341)	(0.1297)	(0.1376)
Credit			− 0.0027	− 0.0036	− 0.0047	− 0.0004	0.0003	0.0006
			(0.006)	(0.0056)	(0.0053)	(0.0056)	(0.0053)	(0.0057)
Trade				− 0.0147***	− 0.0149***	− 0.0117***	− 0.0121***	− 0.0141***
				(0.0036)	(0.0036)	(0.0042)	(0.0042)	(0.004)

Table 4 continued

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln(GDPC)					− 0.162	6.9178**	6.3821**	7.5317***
					(0.4007)	(2.9117)	(2.9544)	(3.0434)
$\ln(GDPC)^2$						− 0.4065***	− 0.3691**	− 0.4348***
						(0.1654)	(0.1679)	(0.173)
Democracy							0.2969*	0.2836*
							(0.17)	(0.1715)
Ethnic								− 1.1068
								(1.1257)
Regression F-statistic	42.16***	40.04***	35.44***	40.94***	40.74***	42.46***	41.97***	40.47***
R^2	0.375	0.375	0.368	0.418	0.416	0.438	0.446	0.466
Observations	1356	1356	1298	1279	1279	1279	1277	1186

Dependent variable is the number of procedures required to start a new business (startup steps)

Intercept and time period fixed effects included in each model but not reported

White robust period standard errors in parenthesis; ***, **, and * denote 1, 5, and 10% statistical significance respectively

Initial log real GDP per capita included as instrument in columns 1–4 when ln(GDPC) excluded from TSLS regression model

Columns 8 and 9 in Table 5 employ the same instrument sets, so only the results for Column 8 is reported

Table 5 TSLS estimates of baseline model

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)[a]
Startup steps	2.1955***	1.8976***	2.3447***	2.2437***	2.3332***	2.1763***	2.1453***	1.9018***	1.9213**
	(0.2817)	(0.4038)	(0.6319)	(0.5942)	(0.6182)	(0.6065)	(0.5885)	(0.6010)	(0.8133)
Education		− 0.3838	− 0.3357	− 0.4322	− 0.6508*	− 0.7693**	− 0.7901**	− 0.8902**	− 0.8663***
		(0.3423)	(0.379)	(0.3418)	(0.3934)	(0.3761)	(0.3729)	(0.3874)	(0.3374)
Credit			0.0294	0.0275	0.0205	0.0254	0.0251	0.0211	0.0056
			(0.0191)	(0.0181)	(0.0186)	(0.0187)	(0.0183)	(0.0181)	(0.0181)
Trade				0.0107	0.0125	0.0147	0.0143	0.0114	0.0227
				(0.0166)	(0.0171)	(0.016)	(0.0153)	(0.0202)	(0.0257)
ln(GDPC)					1.0573	12.9868	15.0619	18.6468	24.751**
					(1.2019)	(9.5193)	(9.8735)	(12.0409)	(11.5844)
$\ln(\mathrm{GDPC})^2$						− 0.6642	− 0.8174	− 0.9940	− 1.3159*
						(0.5469)	(0.5796)	(0.7135)	(0.6903)
Democracy							− 0.6819	− 0.8836*	0.0561
							(0.5139)	(0.5354)	(0.5844)
Ethnic								6.0864*	5.9695**
								(3.3559)	(2.9903)
Observations	600	600	588	588	588	588	586	557	557

Dependent variable is the share of national income held by the top decile of households

Intercept and time period fixed effects included in each model but not reported

White robust period standard errors in parenthesis; ***, **, and * denote 1, 5, and 10% statistical significance respectively

Instruments include legal origins, principle religion, the percentage of English speakers, absolute latitude, and exogenous regressors

Initial log real GDP per capita included as an additional instrument in columns 1–4 when ln(GDPC) excluded from model

[a]The equation in column (9) also includes additional control variables not reported: principle religion dummy variables, and the percentage of English speakers

For the above results to be valid, it must be the case that the chosen instruments are uncorrelated with the error term. As mentioned previously, the startup regulations dataset is relatively new, and only spans the period from 2004 to 2016. When those observations are combined with an unbalanced panel of income inequality measures, the result is an unbalanced panel with relatively few observations per nation (see Table 2). In consequence, most of the variation in the panel is cross-sectional (i.e., across nations), not temporal. That change effectively prevents the use of nation-specific fixed effect intercepts, which would capture nation-specific (and time-invariant) factors impacting income inequality, but the resulting panel dataset would dummy-out most of the variation in the dependent variable. Recall that the regulatory instruments are effectively time-invariant measures of a nation's legal origins, geography and culture (i.e., language and religion). As such, it is important to conduct a test for over-identifying restrictions so as to ensure that the instruments are indeed orthogonal to the omitted fixed effects, which are implicitly compounded in the error term.[21] The preferred specification of Eq. (1), which contains the full set of control variables (column 8 of Table 5), fails the test of over-identifying restrictions. That result confirms that the omitted fixed effects are correlated with our predetermined instruments.

To resolve this problem, we augment the set of control variables (X_{it}) by adding a subset of the regulatory instruments, specifically the culture variables (i.e., language and religion).[22] Those instruments act as proxies for the omitted fixed effects, thus moving the component of the fixed effect correlated with the instruments from the residual and into the model. The downside to this strategy is that it introduces multicollinearity. Although multicollinearity introduces no bias, it does reduce the precision with which the regulatory step coefficient can be estimated, as reflected in less statistical significance. The estimation results for this new model, which passes the test for over-identifying restrictions, are reported in column (9) of Table 5.[23] Interestingly, the coefficient estimates are nearly identical to column (8), but the statistical significance of the Kuznets curve (which was absent in column 8) is restored. The coefficient for startup steps is significant at the 5% level and equals 1.9213, implying that a one-step increase in the number of startup regulations increases the share of income accruing to the top income decile by 6.5% (1.9213/29.73 = 0.065), while a one standard deviation increase in startup steps increases the top decile income share by a staggering 21.8% (3.37 × 1.9213/29.73 = 0.218).

5 Robustness results

To explore the robustness of the foregoing results, we also re-estimate Eq. (1) with an alternative measure of income inequality (the Gini coefficient) and an alternative measure of startup regulations (startup time). In both cases, we find strong evidence that regulatory restrictions lead to greater income inequality.

[21] Under the null hypothesis that the instruments are valid, the test for over-identifying restrictions regresses the residuals from the TSLS regression on the full set of instruments. Multiplying the goodness of fit (R^2) by the sample size (N) yields a test statistic that is asymptotically χ^2 distributed under the null hypothesis, where the degrees of freedom equal the instrument rank less the number of endogenous variables.

[22] Including all of the regulatory instruments as control variables would violate the rank condition, so we must exclude at least one regulatory instrument.

[23] The over-identification test statistic (with 24 degrees of freedom) equals 12.49, well below the 10% critical value of 33.20.

5.1 An alternative measure of inequality: Gini coefficient

To confirm that our results apply to other measures of inequality, we use the highly consistent Gini coefficient panel from the ATG dataset as our alternative dependent variable. We also add three additional dummies included in the ATG dataset to control for differences in welfare concepts and units of measure: dh_{it} equals 1 if the Gini coefficient refers to households (or 0 if it refers to individuals); di_{it} equals 1 if the Gini concept is income (or 0 if it is expenditures/consumption); and dg_{it} equals 1 if the above income/expenditure/consumption concept is gross (or 0 if it is net). Apart from these welfare/unit of measure dummies, the remaining control variables and instruments are identical to those discussed in Sect. 5. Table 6 provides the TSLS estimation results for the Gini model.

Despite the inherent differences between income shares and the Gini coefficient, and differences in nation/time period coverage, the TSLS estimation results are very similar to the ones presented earlier. Regardless of the control variables employed, the coefficients on startup steps are positive and statistically significant. Averaging across the first eight iterations of this model, the average coefficient on startup steps is 1.3743, implying that raising the number of steps required to start a new business by one increases the Gini coefficient by 1.3743. For nations with an average level of inequality as measured by the Gini coefficient (39.16), a one standard deviation increase in business startup steps (3.37) increases the nation's Gini coefficient by 11.8% ($3.37 \times 1.3743/39.16 = 0.118$). While the magnitude of that effect is smaller than the 21.8% estimated increase in the top decile income share reported in Sect. 5, it nonetheless is quite large. Moreover, it is very difficult to compare these two results directly as the change in the Gini coefficient is influenced not only by the change in the top decile income share, but also by the changes in the nine other lower income deciles which collectively lost share. If the lowest income households were affected most directly, the change the Gini coefficient would be greater than if the loss of income came from middle-class households. Ultimately, our purpose in estimating this alternative model is to demonstrate that startup regulations boost income inequality, regardless of how that inequality is measured.

As in Sect. 5, the preferred specification of the Gini model, which contains the full set of control variables (column 8 of Table 6), fails the test of over-identifying restrictions. Employing the same remedy summarized before, we augment the set of control variables by including the invariant measures of culture (i.e., language and religion). The estimation results for this model, which does pass the test for over-identifying restrictions, are reported in column (9) of Table 6.[24] The estimated coefficient on business startup steps is positive (1.5028) and just misses statistical significance at the 5% level (the p value is 0.0554). The coefficient's magnitude is slightly larger (by 9.4%) than the average of the startup coefficient estimates in columns 1–8 (1.3743). That result implies that a one standard deviation increase in startup steps (3.37) raises the average nation's Gini coefficient by 12.9% ($3.37 \times 1.5028/39.16 = 0.129$).

5.2 An alternative measure of entry regulations: startup time

As discussed in Sect. 4, the World Bank's Doing Business dataset includes several alternative measures of the regulatory requirements for doing business, including the

[24] The over-identification test statistic (with 27 degrees of freedom) equals 21.19, well below the 10% critical value of 36.70.

Table 6 TSLS estimates of model with alternative measure of inequality: Gini coefficient

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)[a]
Startup steps	1.7416***	1.3936***	1.7214***	1.5988***	1.4068***	1.0577**	1.1045**	0.9700**	1.5028*
	(0.3631)	(0.3708)	(0.4996)	(0.4634)	(0.4720)	(0.4872)	(0.4792)	(0.4878)	(0.7821)
Education		− 0.7687**	− 0.7080*	− 0.7800**	− 0.4839	− 0.7821*	− 0.7776*	− 0.7917*	− 1.1923***
		(0.3520)	(0.3668)	(0.3510)	(0.4020)	(0.4293)	(0.431)	(0.4356)	(0.3876)
Credit			0.0254	0.0221	0.0272	0.0398*	0.0405*	0.0470**	0.0154
			(0.0248)	(0.0231)	(0.0231)	(0.0224)	(0.0228)	(0.0226)	(0.0235)
Trade				0.0052	0.0030	0.0061	0.0065	0.0027	0.0186
				(0.0157)	(0.0149)	(0.0141)	(0.014)	(0.0169)	(0.0196)
ln(GDPC)					− 1.7734	23.8455**	23.921**	30.2580**	40.5994***
					(1.4597)	(10.6398)	(11.125)	(12.5688)	(12.7586)
$\ln(GDPC)^2$						− 1.4517**	− 1.455**	− 1.7736**	− 2.2653***
						(0.6092)	(0.6463)	(0.7329)	(0.7636)
Democracy							− 0.0795	− 0.2845	− 0.0917
							(0.6463)	(0.5995)	(0.7661)
Ethnic								9.9744***	9.5485***
								(3.1167)	(3.1946)
1st stage F statistic	18.87***	17.95***	16.67***	18.64***	18.60***	17.98***	18.04***	17.37***	17.37***
Observations	437	437	431	430	430	430	428	408	408

Dependent variable is the Gini coefficient from All The Ginis Dataset (2014)

Intercept, time period fixed effects, and dummy variables for welfare concept (gross income, consumption) and unit of measure (household or individual) included but not reported

White robust period standard errors in parenthesis; ***, **, and * denote 1, 5, and 10% statistical significance respectively

Instruments include legal origins, principle religion, the percentage of English speakers, absolute latitude, and exogenous regressors

Initial log real GDP per capita included as an additional instrument in columns 1–4 when ln(GDPC) excluded from model

[a]The equation in column (9) also includes additional control variables not reported: principle religion dummy variables, and the percentage of English speakers

length of time needed to start a new business. As a final robustness check, we re-estimate Eq. (1), using the top income decile as our measure of inequality, and replacing startup steps with startup time. The control variables and instruments are identical to those used in Sect. 5. Table 7 provides the TSLS estimation results for the startup-time variants of Eq. (1).

Not surprisingly, the coefficient estimates reported in Table 7 are very similar to the TSLS estimates shown in Table 5. Averaging across the first eight iterations of this model, the average coefficient on startup time is 0.3080, implying that a 1-day longer investment of time to clear the regulatory hurdles for starting a new business increases the share of income accruing to the top income decile by 0.3080 percentage points. Alternatively, in nations with an average level of income inequality (i.e., the top decile's share is 29.73% of national income) a one standard deviation increase in the amount of time required to start a new business (33.08 days) increases the share of income accruing to the top income decile by 10.2 percentage points ($33.08 \times 0.3080 = 10.2$) or 34.3% ($10.2/29.73 = 0.343$).

Consistent with our prior findings, the preferred specification of the startup time model, which contains the full set of control variables (column 8 of Table 7), fails the test of over-identifying restrictions. Employing the same remedy as before, we augment the set of control variables by entering the time-invariant measures of culture (i.e., language and religion). The estimation results for this model, which passes the test for over-identifying restrictions, are reported in column (9) of Table 7.[25] The estimated coefficient on startup time is positive (0.1750) and just misses statistical significance at the 10% level (the *p* value is 0.1225). As discussed previously, entering the cultural regulatory instruments as independent variables exacerbates any pre-existing multicollinearity on the regression's right-hand side. Multicollinearity may be responsible for the decline in statistical significance in the startup time coefficient between columns (8) and (9). Nevertheless, the coefficient on startup time in column (9) is a bit over half the size of the average coefficient in columns (1)–(8), implying that a one standard deviation increase in the time required to open a business increases inequality in the average nation by 19.5%, which is still substantial.

6 Policy implications

Our results strongly suggest that entry regulations are highly correlated with the levels of income inequality across countries, implying that reducing the stringency of entry regulations should have the opposite effect. Unfortunately, public choice theory suggests that entry regulations will be difficult to remove because entrenched interests will lobby to keep the restrictions in place. As noted by Tullock (1975), rules that help special interests often yield immediate transitory benefits (i.e., windfalls), but as the value of these benefits are capitalized, industry profits revert to normal levels. In spite of this, sheltered special interests will resist reform efforts vigorously to prevent loss of these capitalized benefits. Tollison and Wagner (1991) stress that reform is not a "free lunch", and that entrenched special interests rationally are willing to spend more than reformers (i.e., up to the value of their rents) to defend their special privileges. Furthermore, Tollison and Wagner (1991) rightly emphasize that such lobbying and reform efforts represent a repeated game, and any victories by reformers are likely to be short-lived because special interests always have

[25] The over-identification test statistic (with 24 degrees of freedom) equals 22.94, well below the 10% critical value of 33.20.

Table 7 TSLS estimates of model with alternative measure of entry regulations: startup time

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)[a]
Startup time	0.3314***	0.2927***	0.3209***	0.3166***	0.3162***	0.3049***	0.3049***	0.2765***	0.1750
	(0.0683)	(0.0773)	(0.0961)	(0.0957)	(0.0960)	(0.0932)	(0.0991)	(0.0998)	(0.1131)
Education		− 0.3806	− 0.4486	− 0.4188	− 0.3371	− 0.4329	− 0.4217	− 0.4604	− 0.7182**
		(0.3975)	(0.3957)	(0.4041)	(0.4172)	(0.4219)	(0.4238)	(0.4020)	(0.3664)
Credit			0.0210	0.0206	0.0233	0.0278	0.0276	0.0272	0.015
			(0.0196)	(0.0196)	(0.0212)	(0.0207)	(0.0205)	(0.0214)	(0.0197)
Trade				− 0.0096	− 0.0098	− 0.0065	− 0.0066	− 0.0118	− 0.0094
				(0.0192)	(0.0190)	(0.0191)	(0.0191)	(0.0250)	(0.0209)
ln(GDPC)					− 0.3424	9.3643	9.5208	13.9826	26.8388**
					(1.0623)	(9.1930)	(10.1764)	(12.5465)	(11.7173)
$\ln(GDPC)^2$						− 0.5362	− 0.5438	− 0.8011	− 1.5243**
						(0.5023)	(0.5756)	(0.7151)	(0.6777)
Democracy							0.0335	− 0.1799	0.3899
							(0.6509)	(0.7333)	(0.6011)
Ethnic								2.1708	3.5924
								(4.3990)	(3.4614)
1st stage F statistic	42.16***	23.91***	23.19***	22.23***	22.11***	22.68***	22.40***	19.76***	19.76***
Observations	600	600	588	588	588	588	557	557	557

Dependent variable is the share of national income held by the top decile of households

Intercept and time period fixed effects included in each model but not reported

White robust period standard errors in parenthesis; ***, **, and * denote 1, 5, and 10% statistical significance respectively

Instruments include legal origins, principle religion, the percentage of English speakers, absolute latitude, and exogenous regressors

Initial log real GDP per capita included as an additional instrument in columns 1–4 when ln(GDPC) excluded from model

[a]The equation in column (9) also includes additional control variables not reported: principle religion dummy variables, and the percentage of English speakers

incentives to seek future government protections. That said, how should reformers proceed? Tollison and Wagner (1991) address the question thusly: “our analysis suggests that reformist activity should be directed toward the prevention of future deformities and not toward the eradication of past ones, for these can be eliminated only at a cost that exceeds the value of doing so.” Therefore, *if* reformers could draft rules with constitutional weight rather than procedural changes to existing legal statutes or administrative procedures currently in place (which can easily by circumvented by politicians and regulators), we propose the following two broad policy goals to mitigate the effects of new entry regulations on income inequality.

First, proposed entry regulations that do not solve a demonstrable social problem should be prohibited. Before erecting an entry barrier, regulators should identify the social problem that they are seeking to solve and provide sufficient evidence that the social problem is widespread or systemic.[26] McLaughlin et al. (2014) point out that performing such analyses can direct attention toward actual systemic social problems and prevent regulation when it is likely to be ineffective.

Second, legislators and regulators should be required to evaluate a broad suite of alternative policies when considering intervention. If done properly, regulators may be forced to acknowledge that it is optimal to implement a less restrictive form of entry regulation. For example, many entry regulations are claimed to protect consumer safety and reduce informational asymmetries. However, providing consumers with adequate information through other less restrictive regulations, such as mandatory labeling or information disclosure, may solve the underlying problem at a lower cost. With specific regard to occupational licensing, McLaughlin et al. (2014) point toward three, less restrictive alternatives: registration, certification and titling. By examining those lighter forms of occupational licensing, countries and states may be able to reduce barriers to entry that limit opportunities for low-income workers.

7 Conclusion

There is strong reason to believe that entry regulations are regressive, having a disparate negative impact on poorer entrepreneurs and workers. Both the loss of potential income for those harmed by occupational licensing and other regulatory interventions, along with the inflated financial gains for protected incumbents, increases income inequality without compelling evidence of an improvement in the quality of goods and services provided to consumers. Despite this sobering prediction, we are unaware of any studies that examine the topic empirically. This paper therefore fills a gap in the literature by estimating the impact of entry regulations on income inequality in a large panel of countries spanning 2004–2013. We find that countries with more stringent entry regulations tend to experience greater income inequality. The finding is robust to the measurement of income inequality (the share of income received by the top income decile and the Gini coefficient), and the measurement of business startup regulations (the number of steps required to open a new business and the number of days needed to gain legal permission to open a new firm). Moreover, correcting for potential endogeneity between inequality and startup regulations does not alter the results. In countries with average income inequality, increasing the number of steps required to start a new business by one standard deviation is associated

[26] In fact, this is supposed to be the first step undertaken by a regulatory agency when performing an economic analysis of a proposed rule. See Ellig and McLaughlin (2012) and Ellig et al. (2013).

with a 7.2% increase in the share of income accruing to the top decile of income earners, and a 12.9% increase in the overall Gini coefficient.

The results presented herein are consistent with public choice reasoning that incumbent producers benefit from entry regulations (Stigler 1971), including occupational licensing, which skew income toward politically connected producers and away from individuals who lack the resources necessary to navigate the legal and regulatory framework (see Peltzman 1976; Stigler 1970). Unfortunately, public choice theory also predicts that reform efforts will be both difficult and ultimately too costly to succeed, and therefore reform efforts should be directed toward preventing future protectionist distortions (see Tullock 1975; Tollison and Wagner 1991). In light of this, we propose two broad policy goals aimed at mitigating the effects of future entry regulations on income inequality. First, proposed entry regulations that do not solve a demonstrable social problem should be prohibited. Second, legislators and regulators should be required to evaluate a broad suite of alternative policies when considering intervention.

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