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Institutions as predictors of government discrimination

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Abstract

Exclusion of some groups caused by the misuse of government power remains a major problem across the world. We propose that market-oriented institutions and policies have the capacity to reduce such exclusion. To test this, we use an overall measure derived from the V-Dem dataset, capturing government discrimination based on political group, social group, socio-economic group, and gender, which we combine with the Fraser Institute's Economic Freedom of the World index. The sample consists of 153 countries for 1970–2020, which we organize in a panel consisting of consecutive, non-overlapping 5-year periods, rendering up to about 1,200 observations. Our estimates show a clear negative association between the rule of law and government discrimination in electoral democracies and electoral autocracies but not in single-party autocracies. There are, however, reasons for not considering the finding for electoral autocracies causal. Two further areas of economic freedom seem to matter: free trade is negatively related to government exclusion in electoral democracies, while regulatory freedom is so in both types of autocracies. Thus, it seems as if a market-economic system may be able to constrain public officials in the direction of non-discrimination.

1 | INTRODUCTION

There is a vibrant debate in both academia and popular media about discrimination in markets.[1] Becker ([1957]) famously argued that the stronger the market forces, the less taste-based discrimination there will be.[2] The reason is that in a competitive market, those who do not employ people on the basis of their productivity will be at a disadvantage. If, say, an antisemitic employer refuses to hire Jews, even if they are better at carrying out the tasks at hand and not more expensive to employ, he will tend to lose out to non-discriminatory competitors, which will stifle his discriminatory tendencies.

Still, it seems clear that discrimination _does_ occur in markets—see, for example, Bertrand and Duflo ([2017]), Flage ([2018]), and Neumark ([2018]). Perhaps economic decision-makers with strong dislikes towards certain groups of people are willing to incur the productivity costs of discrimination? Or perhaps statistical discrimination is at play (as suggested by, e.g., Ewens et al., [2014]), such that people belonging to certain groups are treated worse on the expectation that they will be less productive? The presence of discrimination, for whatever reason, has led to anti-discrimination laws in many places, the purpose of which is to deter discrimination. Politicians and public officials are thus seen (or aim to be seen) as "benevolent constrainers" in the market process.

However, governments can themselves discriminate, which is the topic of this study. We turn the tables and look at _government_ discrimination as a function of the degree to which the economy is market-oriented. Our main question is whether market mechanisms (including the institutional basis of the market economy and the rule of law) stifle discrimination in the public sector. We see three possible reasons for a negative relationship. First, there is a legal channel: the rule of law can directly constrain politicians and other public officials by outlawing discrimination. Second, there is an economic channel: market-oriented institutions and policies can induce more competition in the economy, which makes politico-bureaucratic incompetence, including not hiring the most competent staff, more salient, and more costly to companies. This makes the practice politically more costly, which works against discriminatory practices. Third, there is a cultural channel: the rule of law, and possibly market-oriented institutions and policies, has been shown to relate to cultural characteristics, not least social trust and tolerance, which make people averse to discrimination.

To investigate whether the rule of law and other market-oriented institutions and policies are associated with less government discrimination, we conduct a study for the period 1970–2020, applying data from up to 153 countries in consecutive 5-year periods. The outcome variables are from the Varieties of Democracy (V-Dem) data on government discrimination. We use data for four bases of such exclusion—gender, political group, socio-economic position, and social group—which we aggregate into a single discrimination indicator. The degree of discrimination for each of these categories is assessed through a survey directed at national experts in three or four categories. The three that are assessed for all four groups are access to public services, access to state jobs, and access to state business opportunities. For gender and political group, there is an additional category: to what extent civil liberties with respect to belonging to a specific gender or political group are respected. Each category is assessed on a five-unit scale, from 0 (_extreme exclusion_) to 4 (_no exclusion_), and exclusion for each group is the re-scaled average of the three or four underlying ratings.

We relate the resulting indicator of government discrimination to the Economic Freedom of the World index (Gwartney et al., [2022]), an indicator of how market-oriented institutions and policies are in five areas: the size of government, the rule of law, monetary policy, free trade, and regulation. These are based on a mix of expert surveys

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[1]We use "discrimination" and "exclusion" synonymously.

[2]Taste-based discrimination is undertaken on the basis of personal animus towards individuals belonging to a certain group. The other main form of discrimination, statistical discrimination, occurs in the presence of imperfect information about personal qualities. Those making economic decision can, in such a situation, infer personal characteristics "probabilistically" from some group to which an individual belongs. If, say, the group on average is not very productive, the individual member, absent any credible individual-specific information to the contrary, will be assumed not very productive, which can lead to discrimination.

and objective data and use a scale from 0 to 10, for each area, with a higher number indicating a more market-oriented setting.

The empirical analysis reveals a clear negative relationship between government discrimination and the rule of law, which is the only area of economic freedom that is significant for the full sample. However, when differentiating between three types of political systems—democracies, electoral autocracies, and single-party autocracies—further findings emerge. First, regarding democracies, the rule of law is robustly, negatively related to government discrimination—but so is free trade, although to a smaller extent. Second, in electoral autocracies, the rule of law is generally negatively related to government discrimination (with the possible exception for the case of discrimination of political groups)—and so is a liberal regulatory framework (with the possible exception for the case of discrimination of socio-economic status). Third, in single-party autocracies, no area of economic freedom relates to government discrimination of the kinds we study, suggesting that such governments are unresponsive to market-oriented institutions and policies.

How novel is this study? First, it stands in stark contrast to most studies of discrimination in the social sciences, which as mentioned above primarily focus on discrimination in private markets (especially in the labor and housing markets). We stand out by looking at potential determinants of government discrimination. Second, among studies that do look at government discrimination, this study is, to our knowledge, the first that empirically and systematically relates the degree to which economic-legal institutions are conducive to free markets to government discrimination. Fishback (1989) is somewhat related, but his setting of coal companies in West Virginia competing for labor and thus influencing the government to discriminate less against blacks in the educational system is very different. Still, that case does show that companies may react against and influence political decision-makers when they discriminate. Also somewhat related, Lührmann et al. (2018) explore the development of certain aspects of government discrimination in different political systems, but this development is not related to our type of explanatory factors.[3]

We consider our contribution to be not only novel but also significant, because government discrimination has negative consequences and violates moral norms, which makes it important to pinpoint what policy instruments may reduce it. Let us expand briefly on each of these three points.

First, the consequences of government discrimination are of two main kinds: lower well-being and a worse-functioning economy. As for the former consequence, we can relate to research showing that the legal exclusion of gay people leads to lower subjective well-being, both among gay people *and* in the general population (Berggren et al., 2017, 2018). Even though sexual orientation is not among the excluded groups in our data, these findings are relevant for documenting that there are real, human costs of government discrimination. As for the latter consequence, in accordance with the Beckerian logic, one can expect worse quality and lower productivity when government discrimination occurs, both directly (by not employing people exclusively on the basis of merit and competence) and indirectly (by providing worse service and partial decisions when making decisions that affect companies and citizens). These effects can of course in turn reduce well-being as well. If one cares about these outcomes, it becomes important to pinpoint what affects government discrimination, as we try to do in this study.

Second, *even if* government discrimination did not result in lower subjective well-being or a worse-functioning economy, it can still be regarded as an instance of injustice that violates widely shared moral norms. Government decisions should, according to these norms, conform to notions of universality (as espoused by, e.g., Kant) or generality (see Buchanan & Congleton, 1998, and their argument for a constitutional generality principle that prohibits discrimination in political decision-making). If one shares such a moral outlook, it becomes important to investigate whether government discrimination exists and what might influence its scope (as we do in this study), irrespective of consequences.

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[3]There are, furthermore, studies documenting government discrimination on specific grounds in specific places, which provides general credibility to the exclusion measures of V-Dem. However, these studies do not relate discrimination to economic-legal institutions. For example, there is public discrimination of Roma people in Hungary and Czechia (Simonovits et al., 2022; Mikula and Montag, 2023); there is discrimination of LGBT people, offering them fewer legal rights (ILGA, 2020; Ploszka, 2023); the US government initiated and upheld racial segregation (Rothstein, 2017); and US election officials have discriminated against Latinos (White et al., 2015).

Third, we consider our work significant not only because it is possible to problematize government discrimination, as we have just done, but also because the kind of explanatory factor that we examine, legal-economic institutions and policies, makes the findings useful for policymakers. This is not least the case since the effect sizes can be considered "economically significant." Since the institutions and policies we look at can be reformed, our results give guidance about what can be done to reduce government discrimination, should one wish to do so.

2 | THEORETICAL CONSIDERATIONS

Theoretically, we propose that economic freedom can mitigate government discrimination through three channels—a legal one, an economic one, and a cultural one—and we provide certain references to studies that offer tentative support for them being of importance.

First, there is *a legal channel*. The rule of law can directly constrain politicians and other public officials by outlawing discrimination and by providing procedures that contribute to ensuring honesty, non-corruption, transparency, procedural fairness (such as the requirement for public officials to follow the formal rules and not make decisions according to personal preferences), and efficiency in public decision-making, not least in the legal system.[4] Of course, the fact that there are *de jure* rules in place does not ensure that these are enforced and followed de facto (some of the studies mentioned in footnote 3 illustrate that; see also Gutmann et al., 2024).[5] After all, public decision-making is executed by people, with all their imperfections and biases. Still, we do expect less government discrimination where the de facto application of the rule of law is stronger. Furthermore, as we discuss in connection with the third channel below, there can be cultural spillovers from legal institutions (see Engl et al., 2021), and they can reflect relevant cultural factors.

Second, there is *an economic channel*, which we propose has two parts. The first one is that with economic freedom, the lower competence and productivity of the public sector due to discrimination become more salient when there is a clearer private benchmark for people and companies to relate to. This makes people and companies take the costs of government discrimination into consideration to a higher extent, making the phenomenon more politically costly and thus less likely to appear to the extent that private companies are politically relevant. Fishback (1989) provides a pertinent example, where government discrimination in education impaired the quality of the potential labor force and which made companies counteract these exclusionary practices, with successful effect. The second part is that with economic freedom, the competition for jobs between the public and private sector intensifies. When people have more opportunities in a dynamic private sector, recruiting people for government jobs becomes more difficult, especially if you are prone to discriminate, which leads to less discrimination. One may relate this effect to school segregation in the United States and how blacks "voted with their feet" as a consequence (Margo, 1991)—but in our case, they would not need to move to another place, necessarily, but to a private company. However, in cases in which sector-specific labor markets exist, the de facto monopsony power of the public sector may enable discrimination even in the presence of economic freedom, as the market power of the public sector allows managers and politicians to discriminate at low cost.

Third, there is *a cultural channel*. The idea is that economic freedom, through the institutions that the concept embodies and through the market mechanisms they enable, cause people's preferences to change in a direction that reduces their taste for discrimination and, hence, less actual discrimination. This also applies to public decision-makers, as well as to citizens' voting behavior. There is indeed an emerging empirical literature documenting effects of this kind: for example, the rule of law and institutions enabling globalization have been shown to relate to social trust (Berggren & Bjørnskov, 2023; Berggren & Jordahl, 2006; Cassar et al., 2014; Martinangeli et al., 2023), and

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[4]Indeed, one of the eight components of the indicator of the rule of law we use in the empirical analysis is the degree to which courts are impartial, which speaks directly to the issue of discrimination. Another is the integrity of the legal system, which also directly captures a relevant aspect of the rule of law.

[5]This may particularly be the case for regulation, as defined by administrative law and practice, which in some systems lack the political and judicial oversight given to constitutional and regular statutory legislation.

institutions, such as those encompassed under the rubric of economic freedom and the related phenomenon of globalization, are positively related to various indicators of tolerance (Berggren et al., 2019; Berggren & Nilsson, 2013, 2014, 2015, 2021; Eriksson et al., 2021). Enforcement institutions have been found to have cultural spillovers in a prosocial direction (Engl et al., 2021), and greater levels of prosociality follow from a higher degree of market integration and higher payoffs to cooperation (Henrich et al., 2005). There also appears to be a robust positive association between market participation and moral behavior towards anonymous others, and market-integrated participants display universalism in moral decision-making, which arguably goes against discrimination (Agneman & Chevrot-Bianco, 2023). A similar result is obtained when relating market interaction (measured as bilateral trade flows) to the language used about different countries in the *New York Times*: there is a positive relationship between market interaction and the use of moral and virtuous language (Harris et al., 2023). In addition, intercommunity trade and money are associated with prosocial behavior, social trust, and universalist moral values (Enke, 2023), and there is empirical support for a reciprocity-based theory of market interaction inducing more prosociality (Dufwenberg et al., 2022). Market-oriented societies indeed seem to have a greater aversion to unethical behavior and higher levels of trust, and they are not significantly associated with lower levels of morality (Callais et al., 2022).[6]

So far, we have identified three theoretical channels through which economic freedom could reduce government discrimination. However, it remains an open question how these channels operate in different political systems. In the empirical part of this study, we differentiate between single-party autocracies, electoral autocracies, and electoral democracies. Matching the three types of channels through which institutions might affect government discrimination, we see three primary ways in which the type of political institution could affect how market-oriented institutions and policies shape government discrimination.

First, on a general level, the contents of legislation vary in predictable ways across political institutions. Single-party regimes by definition exclude everyone from the political process who does not agree with the incumbent government: they are allowed to vote but not to vote for anyone else than the incumbent. Although market institutions may affect citizens' preferences, they are unlikely to affect government discrimination through any form of voting mechanism. Most single-party regimes also exclude broader swaths of the population for other reasons, such as China's Uyghur population, many social scientists and specific ethnic populations in the Soviet Union, and gay men in Cuba (Guerra, 2011; see also Gregory et al., 2011). Multi-party autocracies exclude comparatively less and appear to typically target identifiably public enemies of the regime while allowing more private freedom (Berggren & Gutmann, 2020). Although not as responsive as democracies, autocracies with multi-party elections also to some extent have to take voters' preferences into account, thereby allowing market institutions to affect government exclusion through popular pressure (see, e.g., Gandhi & Lust-Okar, 2009). Democracies, conversely, allow their citizens more personal freedom, do not intervene in civil society, and almost by definition tend to cause far less government exclusion. Most democracies for example enforce constitutional non-discrimination provisions and allow previously excluded groups in society to form political organizations or otherwise gain representation.

These general exclusion patterns, especially present in autocracies, affect, we propose, the way economic freedom influences the specific types of exclusion we look at through each of the main channels: the de facto rule of law is undermined (especially in single-party autocracies, where all public officials can be expected to follow the dictates of the government and where, therefore, not even the quality of the legal system matters for the degree of government discrimination), the economic mechanisms "disciplining" governments who discriminate are weakened, and markets are not perceived as well functioning but politically rigged, reducing their ability to generate prosocial values.

A further aspect relates to intertemporal predictability. The world's established democracies are among the most institutionally stable countries, and we therefore expect economic freedom to reduce government discrimination

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[6]Storr and Choi (2019) provide a comprehensive and well-written overview of the topic.

more in democracies, where the political system as such, and the basic non-discriminatory legal-economic institutions that are part of it, is expected to remain in place over long periods of time.[7] In essence, the effects of markets take time to materialize and also depend on expectations about the likelihood of future changes in both political and economic institutions.

A related point is that democracies may offer more credible protection of the economic freedom that is in place by virtue of having more checks and balances or division of power, which we expect, in turn, will entail stronger mechanisms for constraining government discrimination. The underlying mechanism as outlined by Justesen and Kurrild-Klitgaard (2013) is that for institutions to have systematic and visible effects, major political decisions have to be locked in by strong veto institutions. In our context, economic freedom is unlikely to have real effects on government discrimination if it is likely (or even expected) that economic institutions and policies giving free reign to markets are abandoned or substantially changed within a year or two. This perspective allows us to hypothesize that the effects of economic freedom on government exclusion are likely to be largest and most precisely measured in democracies, which tend to have the strongest veto institutions, and smallest (perhaps even non-existent) in single-party regimes that virtually never have effective veto institutions.

Keeping in mind the expected differences of the three channels between the political regime types, as discussed in preceding paragraphs, what can be said about the expected _general_ importance of each? We expect the legal channel to be the strongest, as we theorize a direct effect (the legal channel), which is powerful in itself, and an indirect effect, especially through the cultural channel, where the rule of law has been shown to relate positively to outcomes with an anti-discriminatory nature, such as trust and tolerance. We consider the cultural channel the second most important, as the values embedded in people's minds have ramifications for actual behavior. However, we note that cultural influences arguably work over the long term and can be hard to capture in short- or medium-term analysis. Still, if the culture is characterized by tolerance and trust, we consider it a likely influence on attitudes that in turn impact government discrimination. Lastly, we expect the economic channel to be the least important, as competitive pressures are weak in many economies, especially in relation to the public sector.

Lastly, what about reverse causality or other endogeneity problems? On theoretical grounds, it is hard to rule these options out. For instance, if government discrimination goes up, one can imagine this to affect the quality of policy decisions, and it is conceivable that this leads to less economic freedom. Also, since both government discrimination and economic freedom are the result of political decisions, they could be co-determined by some third variable. Therefore, we consider our study as exploratory—suggesting why it is reasonable to expect economic freedom as having a negative effect on government discrimination and presenting empirical findings that are _consistent with_ such a causal direction, even if other causal directions cannot definitively be ruled out. Since we consider this the first study on the link between market-oriented policies and institutions, on the one hand, and government discrimination, on the other, there is scope for further studies to delve into more thorough analysis of causal directions with data that allow it.

3 | DATA AND EMPIRICAL STRATEGY

We use a sample of 153 countries over the period 1970–2020, which we organize in a panel consisting of consecutive, non-overlapping 5-year periods. We do so both because the data on economic freedom are available every 5 years between 1970 and 2000, and because we aim to capture stable, medium-run effects instead of short-run reactions, which could well be quite different.

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[7]Holcombe and Boudreaux (2013) find that autocracies have less economic freedom, while Rode and Gwartney (2012) find that stable democratizations tend to lead to more economic freedom.

3.1 | Measuring government discrimination

First, our primary dependent variables are from the V-Dem dataset and capture exclusion (or, as we prefer to call it, discrimination) by governments defined as "when individuals are denied access to services or participation in governed spaces based on their identity or belonging to a particular group" (Coppedge et al., 2023, p. 211). Although they are intended to capture different dimensions of discrimination and exclusion, we primarily use an index comprised of four of the exclusion categories available in V-Dem, which we call the overall discrimination index. We derive this index from factor analysis, which we outline in detail below in this section. The specific variables capture discrimination against particular political groups (of those who are affiliated with a particular political party or candidate), social groups (caste, ethnicity, language, race, region, religion, migration status, or some combination thereof), socio-economic positions (where groups are defined on the basis of wealth, occupation or other economic circumstances, such as owning property), and gender (which overwhelmingly means women). The primary data are based on national-expert surveys, and the degree of exclusion is assessed on a 5-point scale, with 0 being the most exclusionary situation and 4 being a situation of equal treatment. The ratings for each category are based on three or four more detailed assessments: exclusion by political group consists of assessments of "political group equality in respect for civil liberties," "access to public services distributed by political group," "access to state jobs by political group," and "access to state business opportunities by political group"; exclusion by social group is composed of "access to public services distributed by social group," "access to state jobs by social group," and "access to state business opportunities by social group"; exclusion by socio-economic group consists of assessments of "access to public services distributed by socio-economic position," "access to state jobs by socio-economic position," and "access to state business opportunities by socio-economic position"; and exclusion by gender is correspondingly composed of "gender equality in respect for civil liberties," "access to public services distributed by gender," "access to state jobs by gender," and "access to state business opportunities by gender." All four indicators are subsequently rescaled from 0 to 1 such that higher numbers imply more exclusion; more detailed information can be found in Coppedge et al. (2023, pp. 211–222).

In principle, these detailed data should inform us about the likely mechanisms. However, as we show here, they all tap strongly into an underlying concept of discrimination to an extent that speaks against separating them. The table rather clearly shows that the four exclusion variables load heavily onto a single factor, which we use as our primary discrimination index. The loadings vary between .85 and .96, and the eigenvalue of the first factor is 3.3, while a second factor receives an eigenvalue of .009. This pattern does not change if we perform factor analyses separately for democracies and autocracies (not shown).[8] We are therefore forced to interpret our findings as evidence of effects on overall discrimination, or exclusion, and not any specific type, as they are evidently so closely related. We report details in Table 1.

As noted, the four measures from V-Dem aim at capturing four separate dimensions of government discrimination. The factor analysis thus provides evidence that government discrimination is a phenomenon that permeates different parts and decision processes of the government sector, instead of being an isolated thing. An additional reason for doing so is that we thereby mitigate concerns about multiple test bias, which would be a problem when testing five components of economic freedom with four different discrimination measures and several robustness tests. We therefore use the factor analysis to derive a form of average with weights defined by the analysis, which we use in the following.[9]

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[8]The main pattern also does not change if we perform factor analysis with the 15 underlying subcomponents of the four main variables. Yet, while a simple factor analysis yields a single factor with an eigenvalue of 10, rotation yields three poorly defined factors. We therefore prefer the more precise analysis with the four main variables.

[9]While factor analysis can, under some conditions, yield results that are slightly fragile, ours is extremely robust. Three exercises—excluding one of the discrimination variables, focusing only on democracies, and excluding what may appear as outliers (observations in which there are large differences across the four indices)—yield factor scores that are almost identical. Among the different permutations, the lowest correlation we find is .96.

TABLE 1 Factor analysis of discrimination measures

	Factor 1	Factor 2	Uniqueness
Political discrimination	.851	.073	.271
Social discrimination	.959	.022	.079
Socio-economic discrimination	.917	−.044	.157
Gender discrimination	.911	−.047	.169
Eigenvalue	3.315	.009	
LR test chi squared	6,780.80		

Note: Numbers in Columns 1 and 2 are factor loadings. The number of observations is 1,525. The factors are rotated orthogonally using Varimax.

3.2 | Control variables and empirical strategy

The main explanatory variables are indicators of the degree to which policies and institutions are market-oriented, in the form of the five areas of the widely used Economic Freedom of the World (EFW) index (Gwartney et al., 2022): size of government (where the variable is negatively related to indicators of the size of government), legal system and property rights (which we call the rule of law), sound money, freedom to trade internationally (which we call free trade), and regulation (which is a measure of the absence of regulation and which we therefore call regulatory freedom). In certain empirical specifications, we take the average of the three latter areas and call it the market-orientation of policy. Each category is based on a set of underlying variables, and each is measured based on a mix of national-expert surveys and objective data, from 0 (*which implies no economic freedom*) and 10 (*which implies maximum economic freedom*). In all cases, we observe economic freedom at the beginning of a 5-year period in order to minimize obvious causality problems. The full index is specified in Table S1.

Theoretically, the five areas of economic freedom are related to government discrimination in different ways. As for size of government, we do not expect it to influence government discrimination in any of the three political systems, as it is not the size of government per se but how government power—however extensive—is used that matters. When it comes to the rule of law, we expect it to constrain discrimination but with differing degrees of certainty.[10] The strongest effect should appear in electoral democracies, as democratic countries tend to have incorporated and to respect the rule of law, as well as strong veto institutions. An intermediate effect should appear in electoral autocracies, as a system with certain civic-political rights tends to respect the rule of law to some degree. The smallest effect, if there is any, should appear in single-party autocracies, since they are basically ruled by fiat. In some instances, the ruling party or ruler may pay some respect to the rule of law, but usually not. When it comes to sound money, it is not straightforwardly clear on theoretical grounds if the degree of responsibility of monetary policy (and ensuing low and stable inflation) affects government discrimination. We are, therefore, agnostic of differences between the political systems. One might argue that low and stable inflation provides the system of enterprise with a clearer unit of account, which improves the competitive process, which could have a beneficial effect, but we do not expect this effect to be particularly strong, if it exists at all. Regarding free trade, the general effect of free trade is to sharpen the competitive element of the economy and to affect values (Berggren & Nilsson, 2015). The latter is more difficult the more autocratic the regime, which speaks in favor of an effect (or a larger effect) in democracies. The former can also be expected to be stronger in democracies, as autocracies are prone to intervene in the

[10]It should be emphasized that our rule of law indicator—the "legal system and property rights" area of the Economic Freedom of the World index—is based on a relatively narrow concept of the rule of law. It essentially aims at capturing the independence of the judiciary from political or military influence, and how well it protects the property, contracts, and lives of private individuals. The indicator therefore does not overlap conceptually in a clear way with the specific legal enforcement of non-discrimination legislation.

economic process with outside competition through various counteracting policies, such as industrial policy and regulations, which stifle the competition-enhancing effect. Lastly, as for regulation, we expect effects across political systems—perhaps larger effects in electoral autocracies, since regulation can more easily come in the form of favors to particular groups, perhaps tied to the government or related to it through corruption, and when such regulation is dismantled, the scope for partial treatment should be reduced. Single-party autocracies may be generally more insensitive to competitive pressures, and thus, we expect the smallest effect (if any) there.

We keep the specification relatively parsimonious. First, we include controls for whether a country is officially communist or socialist and whether it has a recent past as either a communist or socialist society (which we term post-communist); the data are from Bjørnskov and Rode (2020). We argue that the remains from such a past, in the form of legislation, judges, and legal traditions, may affect discrimination for some time. Second, we add the natural logarithm to population size as a control, not least as studies on repression suggest that more populous countries tend to repress more (Bjørnskov & Voigt, 2020; Fariss, 2014). Third, we add the logarithm to real, purchasing power adjusted Gross Domestic Product per capita (real GDP per capita) to control for the many ways in which wealth can affect exclusion, repression, and tolerance (see, e.g., Berggren & Gutmann, 2020; Berggren & Nilsson, 2013; Callais et al., 2022; Hall & Lawson, 2014). Both of the latter variables derive from the Penn World Tables, mark 10 (Feenstra et al., 2015). In a set of additional robustness tests, we also include trade volumes and the relative investment price (the price of capital goods divided by a consumption price index) from the Penn World Tables and the well-known Political Constraints III index from Henisz (2000) as a direct indicator of veto player strength. With the latter variable, we also include a dummy variable capturing if there are no veto players at all.

Finally, we throughout include a three-category regime variable from Bjørnskov and Rode (2020), which distinguishes between electoral democracies, single-party autocracies, and multi-party autocracies. A country is defined as democratic if elections are conducted, these are free and fair, and if there is a peaceful turnover of legislative and executive offices following those elections. The distinction between the latter two categories is that in single-party autocracies, there is de facto only one party that can win elections, either because elections are manipulated, not respected ex post or performed with restricted suffrage. This means that we effectively observe regime changes by period-to-period changes in the categorical variable, of which we observe 340 in the present sample. In a robustness test, we replace this regime-type categorization with one of two alternatives from Gründler and Krieger (2021): the machine-learning (ML)-based Democracy Index, both in a dichotomous (democracy or not) and a continuous form, scaled between 0 and 1.

In an additional set of tests, we add three variables as direct proxies for transmission channels. We first calculate an overview index of the strength and participation in civil society organizations (CSOs), based on three variables from the V-Dem: entry and exit restrictions on CSO, government repression of CSO, and the degree to which there is an effective participation in the CSO environment (Coppedge et al., 2023). We calculate the index using factor analysis as the discrimination measure—with equally high factor loadings on all three components resulting in one factor explaining all variation—and use this index as a proxy for the strength of the cultural channel, which we hypothesize. Second, we proxy for a purely economic channel using two different variables from the Penn World Tables. One is employment frequency, calculated as overall fulltime equivalent employment relative to population size while the other is changes in the human capital index included in the dataset. We do so because the economic channel would entail both broader employment when government discrimination declines and better economic competitiveness. The latter is often proxied through indices including both institutional factors directly included in the Economic Freedom of the World index as well as human capital measures. We use the period-to-period change in human capital precisely as a proxy for the non-institutional components of competitiveness.

We present descriptive statistics in Table 2.

Our empirical strategy follows from the exploratory nature of the paper and reflects the parsimonious specification. We apply a standard OLS estimator with annual and country fixed effects, and estimate regression equation (1) in which $G_{i,t}$ is government discrimination in country i at time t, $\mathbf{I}_{i,t}$ is a vector of institutional characteristics in

TABLE 2 Descriptive statistics

Variable	Mean	Standard deviation	Observations
Discrimination, political	.427	.297	1,526
Discrimination, social	.419	.280	1,527
Discrimination, socio-economic	.425	.279	1,530
Discrimination, gender	.409	.265	1,529
Overall discrimination index (from factor analysis)	−.000	.976	1,525
Size of government	6.039	1.621	1,385
The rule of law	4.891	1.577	1,572
Sound money	7.124	2.105	1,266
Free trade	6.241	2.159	1,187
Regulatory freedom	6.106	1.418	1,230
Market-orientation of policy	6.551	1.632	1,160
Single-party autocracy	.309	.462	1,620
Multi-party autocracy	.243	.429	1,620
Electoral democracy	.449	.498	1,620
Communist	.127	.333	1,620
Post-communist	.052	.222	1,620
Log population size	2.019	1.723	1,502
Log real GDP per capita	8.981	1.248	1,502
Trade volume	.501	.489	1,502
Investment price	1.271	.662	1,502
Political constraints III	.219	.220	1,490
No political constraints	.446	.497	1,490
ML democracy, dichotomous	.546	.498	1,493
ML democracy, continuous	.539	.409	1,493
CSO index	.000	.963	1,510
Employment frequency	.385	.088	1,452
Δ human capital	.098	.116	1,388

country i at time t, **R** is a vector of regime types, **X** is a vector of control variables, D are country fixed effects, E are period fixed effects, and ε is an error term:

$$G_{i,t} = a + b\left(\mathbf{I}_{i,t} + c\,\mathbf{R}_{i,t}\right) + \mathbf{X}_{i,t} + D_i + E_t + \varepsilon_{i,t}. \tag{1}$$

As such, we control for all joint effects over time and changes in country coverage, as well as all approximately time-invariant factors including, for example, history, colonial legacies, geography, and social trust. The country fixed effects also control for most potential effects of constitutional protection against discriminatory action, which is typically very stable over decades (see, e.g., Chilton & Versteeg, 2020). While we report the results for the full specification, we also perform tests in which we interact our institutional variables with the three-category regime variable. In these cases, we report all conditional marginal effects with the correct conditional standard errors, calculated by the Delta method, in a lower panel of the table (Brambor et al., 2006).
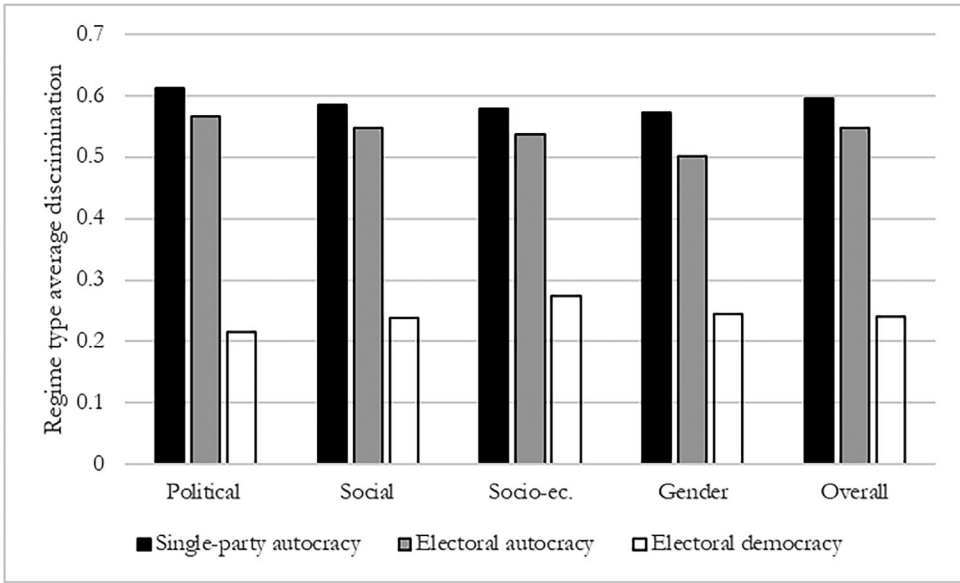
FIGURE 1   Four types of discrimination and overall index for three regime types. *Note*: "Overall" denotes the overall discrimination index, which is the average of the four separate measures, calculated with weights derived from the factor analysis and rescaled for comparison to the same scale as the separate measures.

4 | EMPIRICAL RESULTS

Before presenting regression results, we start by documenting some regular patterns in the raw data. Starting with Figure 1, we show that government discrimination of all four types is on average most prevalent in single-party autocracies and least prevalent in democracies. Multi-party autocracies have slightly lower scores than single-party-autocracies, although they are statistically indistinguishable from each other, except for gender discrimination. The figure also shows that the pattern is identical when we use the overall discrimination index (rescaled to a 0 to 1 scale to be comparable to the rest).

As Figure 2 illustrates, there is a clear negative relationship with the area of economic freedom that we, on theoretical grounds, think is most strongly related to discrimination: the rule of law. While this indicates (perhaps not surprisingly) that better judicial institutions are associated with less government discrimination, the figure also illustrates how these are clearly different concepts. The autocratic countries have more discrimination than democracies, as well as a lower quality of their legal systems. However, this relationship can in principle be driven by other variables, which is why we now continue with regression analysis, which also covers the other areas of economic freedom.

4.1 | Baseline results

As these simple differences indicate an association, we proceed to our baseline findings, which we present in Table 3. All results in the table, as well as the next, are derived using the factor analytical average of four measures.

The results in the table confirm the pattern in Figure 2. In the full sample, the only component of economic freedom that is significantly associated with discrimination is the rule of law. In addition, we find that electoral democracies are much less likely to discriminate against citizens while larger countries also tend to discriminate less. Electoral autocracies appear to be slightly more likely to discriminate, although the coefficient is only weakly significant, and the regime type consists of very heterogeneous countries. Conversely, we find no clear association with income,
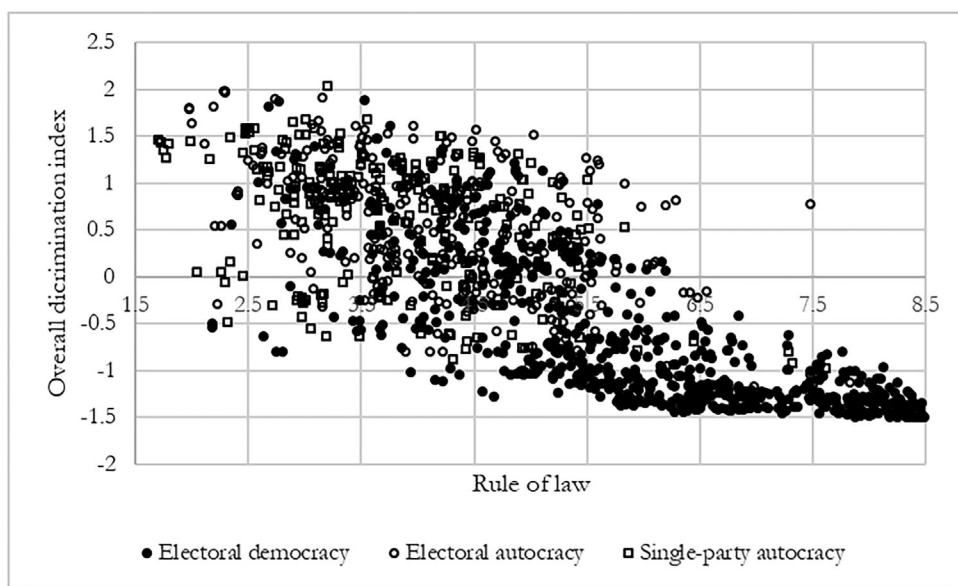
FIGURE 2 Association between the rule of law and the overall discrimination index

TABLE 3 Main results, overall discrimination index

	1	2	3	4
Size of government	.007 (.013)	.000 (.013)	.003 (.013)	.005 (.013)
Rule of law	−.096*** (.034)	−.116*** (.030)	−.099*** (.032)	−.098*** (.031)
Market-orientation of policy	−.021 (.015)			
Sound money		−.004 (.007)		
Freedom to trade			−.012 (.009)	
Regulatory freedom				−.029 (.019)
Electoral autocracy	.087* (.049)	.081* (.047)	.087* (.046)	.081* (.048)
Electoral democracy	−.281*** (.065)	−.264*** (.055)	−.267*** (.059)	−.276*** (.059)
Communist	−.079 (.102)	−.036 (.078)	−.081 (.099)	−.085 (.086)
Post-communist	.030 (.049)	.034 (.046)	.034 (.044)	.018 (.045)
Log population size	−.275*** (.069)	−.217*** (.071)	−.257*** (.071)	−.240*** (.072)
Log real GDP per capita	−.089 (.071)	−.083 (.060)	−.088 (.066)	−.085 (.067)
Country FE	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes
Observations	1,122	1,208	1,147	1,174
Countries	153	153	153	153
Within R squared	.584	.580	.596	.582
F statistic	12.97	16.59	14.76	13.64

Note: Numbers in parentheses are standard errors clustered at the country level.
*$p < .1$.**$p < .05$.***$p < .01$.

TABLE 4 Main results, overall discrimination index, separate for regime types

Third EFW component	1 Market-orientation	2 Sound money	3 Freedom to trade	4 Regulatory freedom
With single-party autocracy				
Size of government	.012 (.020)	.001 (.016)	.001 (.019)	.012 (.019)
Rule of law	−.054 (.047)	−.066 (.045)	−.060 (.043)	−.045 (.043)
Third EFW component	−.019 (.028)	−.013 (.016)	.002 (.016)	−.052** (.026)
With multi-party autocracy				
Size of government	.023 (.024)	.022 (.025)	.009 (.022)	.033 (.024)
Rule of law	−.116** (.054)	−.151*** (.043)	−.124*** (.049)	−.100** (.047)
Third EFW component	−.029 (.027)	.000 (.016)	−.002 (.018)	−.069** (.034)
With democracy				
Size of government	−.002 (.015)	−.010 (.015)	.004 (.016)	−.018 (.013)
Rule of law	−.096*** (.030)	−.102*** (.028)	−.089*** (.029)	−.104*** (.029)
Third EFW component	−.019 (.017)	−.004 (.009)	−.027** (.011)	.009 (.018)
Observations	1,122	1,208	1,147	1,174
Countries	153	153	153	153
Within R squared	.589	.588	.603	.595
F statistic	11.37	13.90	13.06	11.97

Note: Numbers in parentheses are standard errors clustered at the country level. All regressions include the full specification in Table 3, including country and period fixed effects, as well as interactions between components of economic freedom and regime categories. The estimates in the table are conditional marginal effects with conditional standard errors, as calculated by the Delta method (Brambor et al., 2006).
*$p < .1$.**$p < .05$.***$p < .01$.

communist status, or remnants of communism.[11] However, as we also note in our theoretical considerations, the effects may well differ depending on the type of political institutions. We therefore add an interaction with the three categories of our regime type indicator and economic freedom and report the results in Table 4. All results are obtained using the full specification from Table 3. Throughout Tables 4 and 5, we report the conditional marginal estimates for each regime type (cf. Brambor et al., 2006).

Once we estimate heterogeneous effects, conditional on regime type, it becomes clear that the effects of the rule of law in Table 3 are driven entirely by electoral autocracies and electoral democracies, for which we find approximately equal estimates. Conversely, we find no significant associations for single-party autocracies. While size of government never becomes important, we find three additional significant estimates: the freedom to trade is significantly and negatively associated with discrimination in democracies and regulatory freedom becomes significantly and negatively associated with discrimination in both types of autocracies.

We develop more granular findings by re-estimating effects with each of the four exclusion indices from V-Dem; all results are reported in Tables S2–S5. Overall, the most robust result remains the association between the rule of law and exclusion in democracies, which is significant throughout. The rule of law also remains significant in most specifications in electoral autocracies, although it occasionally fails conventional significance with either political or social exclusion as the dependent variable. Conversely, the effects of regulatory freedom in electoral

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[11]The inclusion of the log to GDP per capita may arguably be a bad control if the relevant channel is economic, that is, if institutions affect income per capita, which in turn affects government discrimination. However, excluding GDP per capita renders all statistically significant institutional results qualitatively unchanged, and even only changes their size by less than 10%.

TABLE 5 Main results, including proxies for transmission mechanisms

	1	2	3	4	5	6
With single-party autocracy						
Size of government	.012 (.020)	.011 (.021)	.011 (.020)	.014 (.021)	.017 (.021)	.016 (.019)
Rule of law	−.054 (.047)	−.037 (.039)	−.056 (.047)	−.056 (.049)	−.047 (.049)	−.047 (.049)
Market-orientation	−.019 (.028)	−.006 (.025)	−.018 (.028)	−.027 (.029)	−.023 (.029)	−.024 (.028)
CSO index		−.275*** (.057)				
Employment frequency				−.756 (.759)		
Δ human capital						.011 (.499)
With multi-party autocracy						
Size of government	.023 (.024)	.042* (.025)	.021 (.024)	.015 (.024)	.005 (.023)	.005 (.023)
Rule of law	−.116** (.054)	−.067** (.029)	−.116** (.054)	−.122** (.055)	−.116** (.057)	−.119** (.054)
Market-orientation	−.029 (.027)	−.023 (.024)	−.026 (.027)	−.027 (.027)	−.011 (.026)	−.011 (.026)
CSO index		−.370*** (.064)				
Employment frequency				−.507 (.695)		
Δ human capital						.133 (.354)
With democracy						
Size of government	−.002 (.015)	.004 (.012)	−.003 (.015)	−.004 (.015)	−.000 (.016)	−.000 (.016)
Rule of law	−.096*** (.030)	−.029 (.019)	−.095*** (.030)	−.096*** (.030)	−.093*** (.031)	−.092*** (.031)
Market-orientation	−.019 (.017)	−.014 (.014)	−.019 (.017)	−.018 (.017)	−.019 (.017)	−.019 (.017)
CSO index		−.266*** (.046)				
Employment frequency				−1.101** (.524)		
Δ human capital						−.002 (.042)
Observations	1,122	1,122	1,116	1,116	1,080	1,080
Countries	153	153	153	153	139	139
Within R squared	.589	.695	.585	.592	.603	.603
F statistic	11.37	18.19	11.27	10.91	11.85	11.46

Note: Numbers in parentheses are standard errors clustered at the country level. All regressions include the full specification in Table 3, as well as interactions between components of economic freedom and regime categories. The estimates in the table are conditional marginal effects with conditional standard errors, as calculated by the Delta method (Brambor et al., 2006).

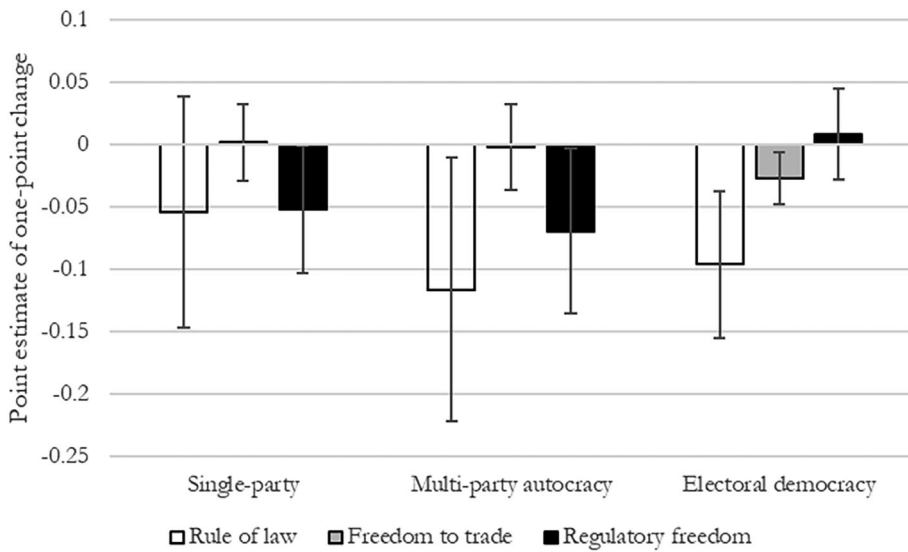*$p < 0.1$.**$p < .05$.***$p < .01$.

FIGURE 3 Point estimates for overall discrimination (from Table 4)

autocracies are either statistically weaker or insignificant, and the estimate of freedom to trade in democracies is only weakly significant for gender exclusion.

Although we must emphasize that not all findings therefore appear ideally robust, we illustrate our main findings in Figure 3, which refers to estimates in Table 4. Noting that the standard deviation of the discrimination index is approximately one, the figure essentially visualizes the size of medium-run effects of changing the rule of law, freedom to trade, and regulatory freedom by one point. The figure makes it evident that the strongest association is between the rule of law and discrimination in electoral autocracies and democracies, where a one-point change in the former is associated with a decline in government discrimination of approximately 10% of a standard deviation. One point increased regulatory freedom is associated with a decline of 5% to 7% of a standard deviation in electoral autocracies, while the size of the estimate for freedom to trade in democracies is somewhat smaller.

To summarize the main results and the additional tests, the general pattern is that the rule of law is the most robust predictor of less government discrimination (except in single-election autocracies) while two other areas of economic freedom appear to matter: free trade in democracies and regulatory freedom in both types of autocracies. These findings are broadly in line with our theoretical predictions and are consistent with market-oriented policies and institutions leading to governments treating their citizens more equally and fairly.

4.2 | Are effects causal in the short term?

As noted above, we cannot claim that the estimates are causal, and we have not been able to find viable instruments to conduct an instrumental-variable analysis nor have we been able to come up with some other form of explicit causality test.[12] What we *have* done is to make use of the fact that for the period 2000–2020, we have access to annual data on economic freedom. This allows us to investigate what happens to government discrimination after *reforms of* the rule of law, that is, the component of economic freedom for which we find clearly robust results. Exploring the

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[12]Finding viable and valid instrumental variables in fixed effects panel settings in political economy is known to be a tall order. The instruments have to provide within-country identification over time while still being credibly exogenous, which is extremely difficult.
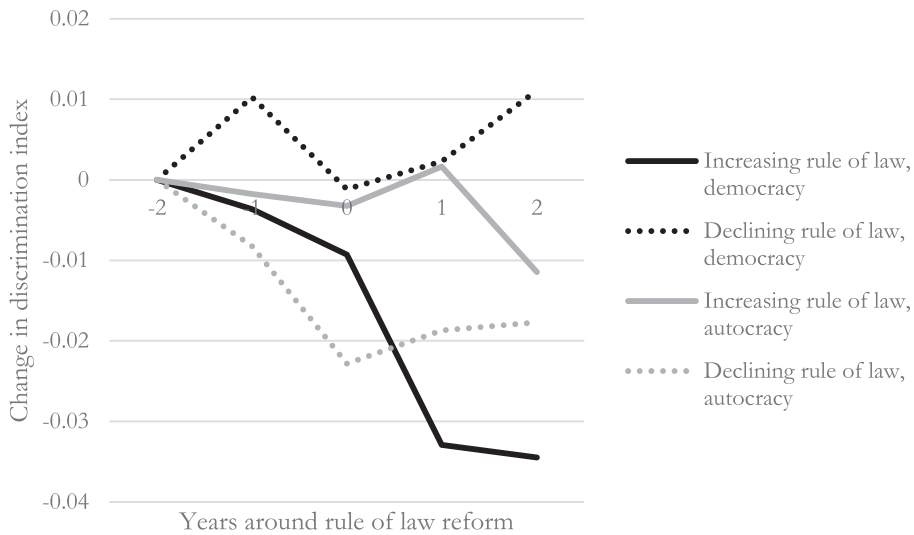
FIGURE 4 Reforms of the rule of law and preceding and subsequent discrimination changes. *Note*: The lines show the yearly change in the discrimination index before and after reforms (improvements in the rule of law or declines in the rule of law) taking place at time 0 for democracies and (both types of) autocracies.

temporal order of changes can thereby justify our claim of a form of Granger causality. We define a reform as a change (positive or negative) in the rule of law of more than two standard deviations of the average annual change and separate them into positive and negative reforms, and reforms that take place in democracies and autocracies.[13] We thus have four types, for which we plot the average change in the overall discrimination index from 2 years prior to a reform to 2 years after in Figure 4.

The figure quite clearly illustrates how only positive reforms in democracies stick out. While discrimination is falling moderately prior to a positive reform (the full black line), this decline is slightly slower than implied by the overall long-run trend in democracies. The simple figure indicates that the parallel trends condition necessary for causal identification appears satisfied. Yet discrimination decreases substantially in the 2 years after the reform in democracies but not in autocracies. We even find that discrimination decreases around negative reforms in autocracies, that is, when they become institutionally weaker, but also that the change occurs *prior* to the reform. When exploring the available annual data, it appears that the overall findings in Table 4 might be interpreted causally for democracies but arguably not for autocracies. Although this is not hard evidence or clearly identified causality, we believe that this type of Granger-like test makes it quite unlikely that the democratic results reflect the reverse causal direction.

4.3 | Robustness analysis

In order to explore whether the findings are robust to various challenges and to explore their generalizability, we have also conducted a few robustness tests. First, to see whether the results pertaining to economic freedom are driven by extreme observations, we have excluded the top and bottom 10% discrimination scores and rerun all regressions in Table 4; we report results in Tables S6 and S7. This exercise gives virtually identical estimates

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[13]This methodology follows Berggren and Bjørnskov (2017). We bundle the two types of autocracies, noting that the vast majority of autocracies since 2000 have been electoral.

compared to Table 4, as freedom to trade remains a significant determinant of discrimination in democracies. However, excluding extreme observations renders the effects of regulatory freedom in autocracies insignificant and also tends to yield substantially smaller estimates.

Second, another test is to exclude all single-party regimes and post-communist countries, as our results might arguably be driven by regimes with no actual opposition and major institutional transitions away from them. The estimates thereby only rely on a comparison between multi-party autocracies and electoral democracies. This test nevertheless yields qualitatively identical findings, as can be seen in Table S8, although estimates of the effects of the rule of law become less precisely estimated in democracies, but more precisely so in autocracies.

Third, another type of test consists of adding more controls for which we have almost full data availability. We specifically add trade volumes (as percent of GDP), the relative investment price (capital goods prices relative to consumption prices), and Henisz's (2000) political constraints III index of veto player strength to the specification. We report the results of these tests in Table S9, which replicates Table 4 including the new variables (which are never significant). However, our main findings remain statistically robust and of almost identical size.

Fourth, one may worry that both the rule of law and regulatory freedom in the economic freedom data include subcomponents capturing the impartial application of rules and legislation. Specifically, we parse the rule of law of the "impartial courts" component and the regulatory freedom index of its "impartial public administration" component of business regulations. In addition, the rule of law index includes a gender adjustment for differential access to the legal system for men and women. However, when we recode these indicators without the gender adjustment and the impartiality measures, we get more or less identical results in Table S10. This is hardly surprising given that the recoded indices have correlations with the original indices well above .9. We nevertheless note that the estimates with the measures parsed of their impartiality components actually yield slightly more accurate estimates, and in the case of rule of law in democracies, also somewhat larger effects. If anything, these parts of the standard measures appear to add noise instead of driving our main findings.

Fifth, we use an alternative way to categorize regime types: the ML Democracy Index from Gründler and Krieger (2021) in both its dichotomous and continuous version. This is a test of whether the results are driven by our particular choice of regime indicator; we report the results in Table S11, with a dichotomous democracy indicator in the first two columns (the difference being that in the second, single-party states have been removed) and a continuous one in the last two columns (with single-party states excluded in the last column). With the dichotomous indicator, we get virtually identical estimates for democracies, compared to our previous results. Using the continuous indicator renders the rule of law significant at $p < .05$ when the democracy indicator reaches 1/3 in the full sample, while it is always significant in the reduced sample. We take this to demonstrate that our results are robust to a different regime-type categorization.

Sixth, in Table S12, we calculate simple estimates of long-run effects. We do so by regressing the 1985–2015 changes in government discrimination on changes in the same period in population size, GDP per capita, and our institutional variables. We follow the format in Table 4 and additionally control for whether countries were communist in 1985. We do not distinguish between electoral autocracies and democracies, because country status changes substantially during the period. Our cross-sectional results in the table again show that the rule of law is important and indicate a long-run effect, which is approximately double the medium-run effects in previous tables.

Finally, we address the potential problem of omitted variable bias, which in our case may be particularly important as a long literature associates the rule of law with a number of other outcomes. We do so by performing Oster's (2019) test, as implemented by Diegert et al. (2023). This test provides us with a statistic on the likelihood that omitted variables drive our main result—that the rule of law causally affects government discrimination. Using our main specification in Table 4, Column 1, we find that the sign of the rule of law is insensitive to omitted variable bias as long as the selection on unobservables (of which in our case is a long list) is at most 51.7% as large as selection on observables. In other words, alternative factors would have to be at least as important as the full specification before our main result would be overturned.

4.4 | Testing mechanisms

Given the robustness of our main findings, our final question is to which degree it is possible to distinguish between our three main channels: a legal, an economic, and a cultural channel through which institutions could affect government discrimination. As noted above, the direct effects through a legal channel are already captured by the rule of law indicator, while the broader effects of an economic channel, to some extent, are captured by the freedom to trade (in democracies) and regulatory freedom (in autocracies). We nevertheless also test alternative economic mechanisms by controlling directly for employment frequency, that is, a labor market mechanism, and human capital development, which we associate with the dynamics of competitiveness. Finally, to test the cultural channel, we include tests with a measure of civil society strength and participation—the CSO index. The idea behind using the CSO index is that strong market economies allow for a rich civil society that fosters characteristics such as trust, tolerance, and cooperation (e.g., Paxton & Ressler, 2017; Storr & Choi, 2019), which we in turn associate with a rejection of government discrimination. In addition, a strong civil society provides organizational capital, which enables coordination among people in resisting government practices they dislike, such as discrimination (e.g., Madison, 2016).

All results are reported in Table 5, in which results in odd-numbered columns are the simple specification and even-numbered columns add one variable, with both using the same sample. Starting with the CSO index, we find clear evidence for a cultural channel. First, the CSO index is significant for all three regime types and with a relatively large effect size. Second, for electoral autocracies, the inclusion of the CSO index approximately cuts the size of the rule of law indicator in half, although it remains statistically significant, indicating both a direct and an indirect effect of the rule of law. Third, in democracies, the inclusion of the CSO index cuts the rule of law estimate to a third and renders it insignificant, indicating that most of the effect may run through a cultural channel.

Conversely, although the employment frequency is statistically significant in democracies, neither it nor the inclusion of human capital development affects any of the estimates of the rule of law or other elements of economic freedom. Apart from the indications for freedom to trade in democracies and regulatory freedom in autocracies in previous regressions, we find no clear evidence of an economic channel through which institutions affect government discrimination. We must nevertheless stress that these additional variables are imperfect proxies and our tests here must be thought of as preliminary.

Overall, the tests indicate that our main result—the importance of the rule of law for overall government discrimination as well as four types—remains robust and suggest that a legal and a cultural channel are directly relevant, while there is scarce support for an economic channel. Our simple exploration of short-run changes in Section 4.2 also indicates, in the absence of stronger tests, that the findings for democracies may reflect causal influences. We therefore proceed to discussing the findings and their relative importance.

5 | DISCUSSION AND CONCLUSIONS

The issue of discrimination and exclusion is vexing. It violates widespread notions of justice, reduces well-being, and entails a loss in productive capacity (cf. Klasen, 2018; Neumark, 2018). To counteract it in markets, many see government action as a key solution—for example, by its implementing and enforcing anti-discrimination legislation. In this study, we look at the "inverse" situation: the presence of *government* discrimination against groups of citizens and how market-oriented institutions and policies, and the market process, may counteract it. At first, this may seem like an odd way of thinking, but we argue that, on theoretical grounds, there are three channels through which markets can constrain governments from excluding people from benefits and rights.

The first is a legal channel: through the rule of law, a key component undergirding markets, politicians, and civil servants are legally constrained from discriminating. The second is an economic channel: by increasing salience and by competition for labor from the private sector and by having people and companies react against lower

competence and productivity, there are political costs of discrimination. The third is a cultural channel: market-oriented institutions and processes tend to shape human preferences in a prosocial direction, making people less inclined, also when working for the government, to treat certain people worse than others.

In this first, explorative study of the link between economic freedom and government discrimination of people on the basis of politics, socio-economic status, social group, and gender, we use cross-country panel data, in which we combine the Economic Freedom of the World dataset with the V-Dem dataset on government discrimination covering a period of 50 years, to test our theoretical considerations. Our findings suggest a clear negative relationship between the rule of law and governments treating some citizens worse than others. This holds for electoral democracies and electoral autocracies but not for single-party autocracies, who seem quite resistant to this kind of institutional influence. We also find evidence of a significant association between discrimination and the freedom to trade in democracies, although the size of the estimates is somewhat smaller. However, a rudimentary causality analysis indicates that the findings for electoral autocracies may not be causal, while the findings for electoral democracies are consistent with a causal influence.

The effects are not only statistically significant but also economically and politically meaningful. We find that a one-standard-deviation change in the rule of law is on average associated with a decrease in overall government discrimination of about 10% of a standard deviation in the medium run. The long-run effect is likely to be approximately double, such that a one-standard deviation improvement in the rule of law would imply a change equivalent to a reduction in the government discrimination in the United States to the level of Sweden, or a reduction of the government discrimination in Serbia to the level of the United Kingdom, as measured in recent years. As such, the findings appear politically meaningful and well worth discussing, not least considering that we strictly focus on medium-run effects such that consequences in the very long run could well be larger. Moreover, our findings may be taken to suggest that the presence of elections with more than one party is necessary for the rule of law to constrain government discrimination. We may, therefore, be underestimating the full effects of economic freedom, as it also appears to be a condition for maintaining a stable democracy (cf. Lawson & Clark, 2010). In all, we find support for a negative association between aspects of economic freedom and government discrimination, especially in electoral democracies and multi-party autocracies, with legal and cultural channels driving the results. This is arguably an overlooked beneficial consequence of economic freedom, which strengthens the case for it.

A strength of focusing on the various areas of economic freedom as potential determinants of government discrimination is that one can derive policy implications since the five areas of the Economic Freedom of the World index consist of variables that almost exclusively are the results of policy decisions. The clearest policy implication is that strengthening the rule of law (e.g., stronger judicial independence, more impartial courts, reduced military interference in the legal system and in politics, stronger protection of property rights, stronger legal enforcement of contracts, and higher reliability of the police) is likely to result in noticeably less government discrimination. In democracies, there are indications that more free trade could have a similar effect, just as more regulatory freedom might in multiparty autocracies (except for government discrimination based on socio-economic status). Our findings offer several strategies for politicians who are determined to reduce unequal treatment by public sector decision-makers.

Still, we recognize the limitations of this study, especially in establishing a causal connection, which is why we recommend that it is read as a first exploratory study and not a final statement on the question. This also gives reason for caution when considering the policy implications. For example, even if a stronger rule of law does reduce government discrimination in a causal sense, it may be that factors that determine the strength of the rule of law to some extent also determine the willingness by government officials to discriminate. Then, one would not expect such officials to initiate a stronger rule of law, at least not for this reason alone.

In order to move the literature forward, we see scope for further research building on our work. The most important thing would be to establish causality in a stronger fashion. While this is always a challenge when using cross-country data, there could be room for innovative case studies (perhaps in the form of natural experiments) as well as the application of matching methods that could reduce the endogeneity problem. At least, we bring some robust correlations to the table as a starting point for an expanding literature.

ACKNOWLEDGMENTS

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from the corresponding author upon reasonable request.

CONFLICT OF INTEREST

The authors declare no conflict of interest.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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